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The New York Academy of Medicine.

By Samuel S. Purple M.D.,

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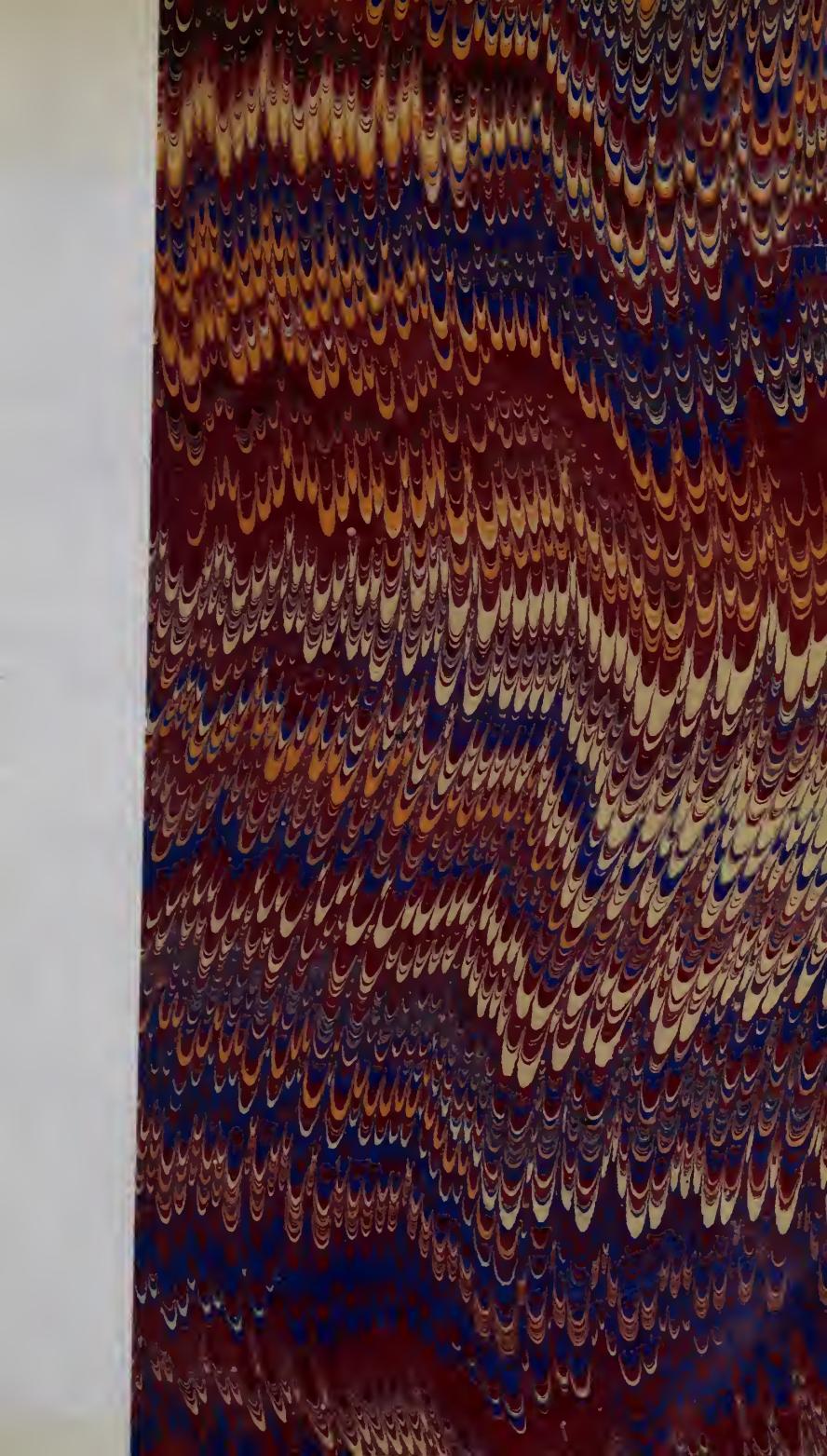

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL

FOR JANUARY, 1858.

ORIGINAL COMMUNICATIONS.

ART. I.—*Practical Remarks on the Evidences of Pregnancy* (*Continued from page* 166, *Vol. XIV*): By DR. J. E. THOMPSON, Roseville, Ark.

Visible Evidences.—IN my first paper I spoke of those evidences ascertained through the female by the history of her own case, denominated *presumptive evidences,* and those sensible to the medical attendant by *manipulation.* I now come to speak of those evidences which are sensible to the accoucheur through the medium of sight, denominated *visible evidences.*

The changes that occur in the female breasts in pregnancy are the effects of their sympathy with the uterus. Of the strong reciprocal sympathy that exists between the uterus and mammæ, every day's experience must sufficiently convince us. These changes consist in an increase in size and sensibility of these organs, a distention of their veins and a development of the nipples and surrounding follicles, with the formation of the areölæ, and the secretion of a serous or milky fluid. These phenomena do not all constantly occur, neither is the period of their occurrence invariable, some of those which generally appear about the third month being occasionally earlier and often not showing themselves until immediately before labor. The formation of the *areölæ* or dark circle around the nipple is much relied on by some as being characteristic of pregnancy, while others attach but little importance to it. Dr. Hunter is said to have placed such confidence in the occurrence of the areölæ that he on one occasion in the dissecting room, pronounced a female subject pregnant upon this single sign, although the hymen was perfect; and on dissection his opinion proved correct. Dr. Gooch thinks that the formation of the areölæ rarely depends upon any other cause than that of pregnancy, and that it may be looked upon generally as an evidence that the female either is, or has been pregnant; yet, says he, "I saw two

young women newly married, within two days, who had made preparation for lying-in, who were not pregnant. In both the areölæ were dark, though (if their history is to be trusted) they had never had children." (Gooch p. 205.) Dr. Denman, with all his practical opportunities and acute powers of discrimination, however, arrives at quite a different conclusion. After stating the arcöla to be the effect of the previous enlargement and alteration of the breasts, and that it may be produced by any cause capable of effecting an alteration in their state resembling that which they assume in pregnancy, he remarks: "the areöla is therefore formed in many of the complaints which resemble pregnancy, and though generally, not invariably, in pregnant women." Dr. Kennedy comes to the conclusion "that although it is not unworthy of our attention, taken with other symptoms, particularly in first pregnancies, yet, from the frequency of its occurrence in cases where pregnancy did not exist, and its absence in cases where it did, without strong corroborative proofs at least, we can place but little confidence in it." (Obstet. Auscult. p. 60.) Dr. Hamilton relies chiefly upon the turgescence of the areölæ; he only observed it in preguant females. Dr. Montgomery, whose observations are ackuowledged to be the best ever preseuted to the profession on this subject, relies more "on the development of the little glandular follicles, and the moisture around the areöla, and that the alteration of color is by no means the circumstance most observable; though, pregnancy may exist and the areöla remain deficient in at least one of its essential characters, and that too, in its color." Dr. Ingleby considers the areölæ as good proof of the existence of utero-gestation. Dr. Dewees remarks: "The areölæ must be cousidered as equivocal in any but a first pregnancy; in this case, did areölæ form, I should place great dependence upon them; for, so far, I have not been deceived." (Diseases of Females, p. 164, 8th edit.) Dr. Maygrier remarks: "The dark and brownish color of the areölæ and the nipple, is generally regarded as a sign of pregnancy. * * * * * This sign, however, would not always be sufficient to confirm the existence of pregnaucy; some females have naturally a dark arcöla, and in others who have even borne several children, this part is unaltered, and has always preserved a slight rosy color, even after several pregnancies." (Mid. Illust. p. 80, by Doane, 3d edit.)

If there be any sign respecting the areölæ that could be more uniformly relied on than others, it is the inflated, moistened areölæ, and especially at the base of the erect, reddish brown nipple. Much confidence cannot, therefore be placed in the color of the arcöla, its turgescence and enlarged papillæ, for there are so many instances of uterine affections as dysmenorrhœa and amenorrhœa, where the changes and appearances of

the breast have so perfectly simulated pregnancy, that, as a sign, taken *per se*, it cannot, especially in the earlier months, be much relied on; but as a co-existing sign of pregnancy, it is of great value. In negroes the color of the areolæ is worth but little to the accoucheur as a diagnostic sign of pregnancy.

The secretion of fluid, whether of a serous or milky character, in the breast of the pregnant female, is still more fallacious as an evidence, and may be produced by a variety of causes quite independent of pregnancy. Mere titillation or friction of the breasts or nipples may excite a secretion in the glands. I have frequently observed it in infants, and even abscesses to form from the secretion of milk in their breasts. Virgins have been known to secrete milk, as well as aged females, and even men have performed the office of wet-nurses. M. Bandeloeque speaks of a girl who had milk formed in her breasts as early as the age of eight years (Vol. 1, p. 219). A case was related to me by a lady of much intelligence and respectability, of an old lady sixty-five years of age, who upon noticing her daughter weaning her own child remarked, "come here you sweet little dear and grand ma will give you titty;" and accordingly took the child and *nursed it three years*. Dr. Kennedy met with a similar case in the person of an old mid-wife sixty-five years of age, who could at pleasure extract milk, or a fluid resembling it, from her breasts. She was frequently in the habit of suckling her grand-children. "The Virginia Medical Journal reports a case of free lactation in an old woman, induced by a grand-child, who had lost its mother, sucking the nipple at night. It is seven years since the old lady had a child, and she is now suckling abundantly. She is to be seen in Richmond, Virginia." (New Orleans Med. News and Hospital Gazette, vol. 4, p. 435.) Dr. Dewees speaks of a lady who had been married seven years, but had never been pregnant, having been, at the time referred to, separated from her husband two years. Considerable quantities of milk having formed in her breasts, caused her to name it to a female friend, who, from an impression that she was pregnant, told it to another friend as a great secret; who in her turn named it to another friend, and thus, after having enlisted fifteen or twenty to help them keep the secret, it got to the ears of the lady's brother, who became much enraged, and accused her in the most violent terms of incontinency towards her husband. Upon examination, however, she proved not to be pregnant; but died in about eight months after of phthisis pulmonalis. (Diseases of Females, p. 165.) Humboldt, in his Personal Narrative, and the Bishop of Cork in the *Phil. Trans.* for 1741, give an account of a man who upon the death of his wife, succeeded in suckling and raising his child. Dr. Dewees records

a case related to him by Mr. Heber Chase, of Hopkinton, New Hampshire: "James Hildreth, of Hopkinton, N. H., was about three years since, (December 9th, 1834) from the abundant secretion of milk in his right breast, enabled, by pressing it with his fingers, to project milk from the nipple two or three feet. Mr. Chase declares he has seen him do so often at the young people around him. He was a robust, muscular man, somewhat dissipated, and a blacksmith by trade. He was about thirty years of age, and the father of several children." (Diseases of Females, p. 165.)

This sign cannot be looked upon as of much importance as it may be produced by many causes foreign from that of pregnancy.

Pouting out of the umbilicus invariably takes place after the sixth month, and sometimes at an earlier period; but without the occurrence of other evidences, it by no means follows that the female is pregnant. This circumstance may be produced by several causes independent of pregnancy, that are capable of distending the uterus to a size equal to that of the sixth or seventh month; as in ascites, sanguineous sarcoma, hydatids, chronic enlargement of the liver, or some other of the abdominal viscera. Dr. Denman attaches considerable importance to this sign, and states, "that we have the umbilicus constantly emerging throughout the progress of pregnancy, until it comes on a level with the integuments of the abdomen." The uterine tumor, by distending the abdominal integuments more than the skin can readily admit of, will sometimes produce an inflammatory action in it, attended with a yielding of the cuticle and a weeping of lymph. The *cutis vera* will also occasionally give way, the cuticle remaining uninjured, causing the skin to assume a curious *fissured* appearance. This, however, may be met with in enlarged ovary, or extreme abdominal distention. This occurrence is by no means general in utero-gestation; therefore, no great reliance can be placed upon it as a diagnostic evidence of this state.

The state of the countenance and appearance of the skin has also been regarded as evidencing conception and pregnancy. The eyes of the pregnant female assume a peculiar hollow, languishing appearance, altogether wanting its usual lustre, whilst a dark circle surrounds them. The countenance is observed to assume a sunken, hollow and wasted look, whilst the skin may be covered with freckles or dark-colored spots. In some females, however, the countenance becomes suffused, with determination of blood to the head; or eruptions on the face and body may occur. These circumstances, although they do occasionally occur in pregnancy, can scarcely be looked upon as rational evidences of this state; from the fact, that pregnancy frequently occurs where these signs are not present; and

that they very often occur in the absence of this state, and that, too, in those very cases which will afford us most embarrassment, such as stomachic and uterine derangement, accompanied with suppression or altered catamenia and gastric irritation and distention.

Jaundice has also been classed with the signs of pregnancy. It is, however, seldom met with in early pregnancy; but when it does occur, it depends upon the deranged state of the alimentary canal and liver; when it occurs in advanced pregnancy, which is the time it mostly occurs, it depends upon the pressure of the enlarged uterus on the gall duct. It is, at all events, so rare an accompaniment to utero-gestation, that it, as well as several other signs enumerated, scarcely merits our attention as such.

Swelling and *œdema* of the lower extremities, sometimes extending to the groins, vulva and lower part of the abdomen, have been looked upon as evidencing pregnancy. These circumstances certainly not unfrequently occur in this state, more particularly in debilitated females, or those who are obliged to be very much in the erect posture, attending to laborious occupations. Still, if we consider how frequently they occur in dropsy of the abdomen, both general and encysted, inorganic disease, enlargement of the abdominal thoracic, and pelvic viscera, as well as in functional diseases of the uterus, attended by suppression of the catamenia, and even in cases of extreme debility, we shall at once pronounce them unworthy of confidence.

A *varicose* state of the veins of the legs occasionally occur in pregnancy. As a sign in pregnancy, however, its utility is of meagre importance, when we consider in how small a proportion of pregnant females it is met with, and that it may occur either as an idiopathic affection from phlebitis or other diseases of the venous system, or as the effect of a variety of causes, which, like pregnancy, prevent the free return of blood from the lower extremities.

Hæmorrhoids has also, by some, been reckoned among the signs of pregnancy, but when we consider the variety of causes that may produce them, we are led to place no confidence whatever in their occurrence.

The appearance of a peculiar substance denominated *Kyesteine* in the urine has attracted considerable attention among the profession. *Kyesteine* was first observed by Savanola in the year 1486; also about this same time Avicenna partially noticed its peculiar properties and appearance. But the credit of the discovery of this chemical test is perhaps justly conceded to M. Nauche, of France. He was quickly followed, however, by the researches of Golding Bird, and still later by those of M. Equisier and M. Tanchau, of Paris; Mr. Letherby, of London Dr.

Stark, of Edinburgh; *Drs. Elisha K. Kane*, of the U. S. Navy, and Isaac E. Taylor, of New York. M. Nauche's observations, however, attracted but little attention, and the subject of *Kyesteine* in the urine of pregnant females was scarcely noticed by the profession until M. Tanchau published his observations in *Lancette Française*. M. Nauche states that pregnancy may always be detected by "allowing the urine of pregnant women or nurses to stand for some time, say, from thirty to forty hours, when a deposit takes place of white, flaky, pulverulent, grumous matter, being the caseum or peculiar principle of the milk formed in the breasts during gestation.: This deposit is called *Kyestiene*" (Lancet for August 27th, 1831.)

M. Tauchau examined the urine of twenty-five pregnant females, and in every instance detected *Kyesteine*. The following is his description of the various changes which the urine, during pregnancy, exhibits, and of the characters by which its peculiar ingredients may be recognized: "The urine of a pregnant female, collected in the morning, is always of a pale yellow color, and slightly milky in appearance; it is not coagulable by heat, or by any other tests which indicate the presence of albumen. Left to itself, and exposed to the air after the first day, there begins to appear suspended in it, a cottony-looking cloud and at the same time, a flocculent whitish matter is deposited at the bottom of the fluid. * * From the second to the sixth day, small opaque bodies rise from the bottom to the top of the fluid; these gradually collect together so as to form a layer which covers the surface; this is *Kyesteine*. It is of a whitish or opaline color, and may be aptly compared to the layer of greasy matter which covers the surface of fat broth when it has been allowed to cool. Examined by the microscope, it exhibits the appearance of a gelatinous mass, which has no determined form. Some small cubical crystals can be perceived in it, when it has become stale.

"The *Kyesteine* continues in this state we have now described, for three or four days; the urine then becomes muddy, and minute opaque bodies detach themselves from the surface to settle at the bottom of the vessel; the pellicle thus becomes soon destroyed." (Amer. Journ. Med. Sci. for Feb. 1840, p. 484.) This peculiar appearance has been described by Avicenna, as "*Sicut grana ascendentia et descendentia.*" Dr. Stark, of Edinburgh, considers this as a substance "*sui generis*," which from his experiments, he has concluded that it is a new substance, and accordingly termed it *gravidine*, from *gravidus—big with young*. Dr. Griffiths, of London, regards this substance as being the "lithate of ammonia," which opinion is conceded to by Dr. Taylor. Dr. Ramsbotham remarks, "In order to obtain the *Kyesteine*, the urine should be taken from a female as free from

disease as possible; and that passed early in the morning should generally be selected. Expose it in a tall narrow glass to a temperature of 70° Fahr. In two or three days if the woman be pregnant, the first indication is turbidness; in from two to three days more, a thin pellicle forms on the surface, and this gradually acquires consistence up to a fortnight." (System of Obstet. p. 85-6, by Keating.)

Dr. Taylor says: "The earliest period I have noticed it was at three weeks in two cases, and this was calculated from the time the urine was taken till the commencement of labor, which was eight months after. * * * Every month for three months, the urine was examined, and each month the test was observed. Some of the cases did not give the test on the first trial, but have during the second and third. * * * I have known it to remain two weeks without giving any pellicle or deposit; but when placed in a room of sixty-eight or seventy degrees would show its true character." (Taylor's edit. of Kennedy, p. 67.) Mr. Letheby says that "that out of fifty cases experimented on by him, at all dates between the second and ninth months of utero-gestation, he obtained unquestionable evidence of its presence in forty-eight." Of the twenty-five in which M. Tanchau detected the presence of *Kyesteine* in the urine, seventeen occurred in women who were pregnant from four to nine months; four in women who had quickened, and who considered themselves as laboring under disease of the uterus; and the remaining four in patients who had been under treatment for casual complaints—one for sciatica at the Hôtel Dieu, another for ascites in the city, a third from an ulcer in the neck, at La Pitié, and the last had been cauterized twice a week for a pretended disease of the uterus. In nine of the twenty-one cases, had the existence of pregnancy been suspected, although in every one of them the fact was soon placed beyond a doubt." (Amer. Journ. Med. Sciences for Feb., 1840, p. 484.) There is considerable discrepancy of opinion with regard to the peculiar smell of *Kyesteine*. Dr. Golding Bird says it is "like that of beef beginning to putrefy." Some regard it as resembling that of old cheese; while others assumes it simulates the ordinary ammoniacal smell. But where I have noticed it, it was of a cheesy smell. With reference to the value of *Kyesteine* as a diagnostic sign of pregnancy, it is hard to determine, for while some look upon it as *positive*, others place but little confidence in it. The late and much lamented Dr. E. K. Kane remarks: "I cannot regard *Kyesteine* as an unerring test of pregnancy; I am convinced too, that the *Kyesteine* pellicle is not always distinguishable from the pellicles appearing on the surface of the urine. I have no doubt, however, that *Kyesteine* is *among the best*, if indeed it be not *the most certain* of the early indications of pregnancy."

Out of 500 cases referred to by Dr. Taylor, 450 gave the test of pregnancy. Hence his remarks: "From the number of cases adduced, I should conceive there could not be a question, but that it is a sign of considerable value, and especially, I think the earliest sign that can be tested, though liable to the restrictions and exceptions which will ever belong to all the signs of pregnancy, and could not be considered as a positive or certain sign." (Taylor's edit. of Kennedy, p. 68.) In eighty-five cases examined by Dr. Kane, sixty-eight gave the *Kyesteine* well marked; in eleven it was not well defined; and in six it was entirely wanting. He also observed that a white flocculent precipitate similar to that described, subsided spontaneously after twenty-four hours, not only from the urine of pregnant females, but also in an equally great quantity from a virgin ætat. 14, and that of a woman nurse two months; and that in all the cases of pregnancy the urine was found to contain a small quantity of *albumen* in its uncoagulated state, although this was not observed in the urine of unimpregnated females contemporaneously examined.

M. Nauche regarded the presence of *Kyesteine* as a *certain* evidence of pregnancy; although M. Pereira was experimentally satisfied of the insufficiency of his test.

Dr. Ramsbotham comes to the following conclusions, to wit: "1. *Kyesteine* is not peculiar to pregnancy, but may occur whenever the lacteal elements are secreted without a free discharge from the mammæ. 2. That though sometimes obscurely developed, and occasionally simulated by other pellicles, it is generally distinguishable from others. 3. That where pregnancy is possible, the exhibition of a clearly-defined *Kyesteine* pellicle is one of the least equivocal signs of pregnancy. 4. That when this pellicle is absent in the more advanced stages of pregnancy, the probabilities, if the female be healthy, are as 20 to 1 that the prognosis is incorrect." (Syst. Obstet., p. 86, by Keating.)

Audible Evidences.—Of the foregoing evidences there is scarcely one on which even the most skilful and practical accoucheur can so far rely in all cases as to say, I am convinced from the presence of this or that sign, that a female is or is not, pregnant. There may appear, and no doubt are exceptions to this rule, such as the active or passive motions of the child; but even in these we may be deceived, for they cannot always be observed when pregnancy is present. When there is difficulty or uncertainty with regard to pregnancy, the means at present in general use are often insufficient to detect, or to enable us to pronounce with exactness upon this state; and our embarrassment, so far from being diminished, is rather increased by the number and variety of what are usually denominated the *evidences of pregnancy*. Hence, the necessity of having in cases

of this kind, a limited number of *distinct and demonstrable evidences* to depend upon, such as could be detected with facility, and which it would be impossible to counterfeit, in place of leaving it, measurably, to the result of inference drawn from innumerable, indefinite, and often fortuitous circumstances, many of which are individually insignificant, and worthy of our attention only when taken collectively. The evidences which we are now about to consider, denominated *audible*, if they do not enable us to arrive at this desirable point, may, at least assist us much in doing so; and when taken in conjunction with a few of the leading evidences already dwelt upon, we shall by their assistance be enabled to pronounce with pretty good certainty, in almost every case, provided the pregnancy be sufficiently advanced.

Auscultation is the means by which the accoucheur can make up a positive diagnosis in all cases of utero-gestation, however perplexing and embarrassing they may be in their nature.

The individual whose claim is established as the first to call the attention of the profession to auscultation, as a means of diagnosis in utero-gestation, is M. Mayor, of Geneva. By applying the ear to the abdomen of a female in an advanced state of pregnancy, M. Mayor detected the fœtal heart's action through the abdominal parietes. M. Mayor's discovery, however, stopped here, no doubt from a neglect to prosecute it further. Dr. Kergaradec, of Paris, without any previous acquaintance with Mayor's previous discovery, arrived at a knowledge of the same fact accidentally. Prompted by a praiseworthy spirit of investigation he followed up his inquiries and further discovered a simple pulsation, accompanied by a peculiar sound resembling the *souffle* observed in certain diseases of the heart, which he thought denoted the place of the attachment of the placenta to the interior surface of the uterus.

Auscultation is divided into immediate and mediate. Immediate—when the naked ear, cheek or hand is applied to the abdomen; mediate—when an instrument called the stethoscope is used to detect the sounds, etc., through the abdominal parietes. Each of these methods has its advantages and its disadvantages to a greater or lesser extent. However, *cæteris paribus*, we may state this to be the fact, that our success will in general be greatest with the means we have been most in the habit of employing. M. Foderé objects to the use of the stethoscope, asserting that the *souffle* can be heard by the naked ear when it is not at all perceptible through the stethoscope. Siebold calls it a "hearing trumpet," and notwithstanding the many experiments he made with it, he "could not hear anything with it." When the phenomenon we wish to explore

is confined to a small circumscribed spot, more particularly if there be other sounds in its neighborhood likely to drown it by their force, or be confounded with it, we shall generally succeed better with the stethoscope; on which account it is always preferable to use it in searching for the fœtal pulsation, particularly when the placental *souffle* is observed in the same region. In the early months of utero-gestation, when the placenta is proportionably so much more developed than the fœtus, we can, by applying the stethoscope and varying its position frequently recognize the pulsation of the latter, where, from the confusion produced by the extent of surface covered by the head, and the impossibility of applying the ear to a particular part, we could not otherwise have succeeded. Dr. Kennedy remarks: "In exploring for the placental *souffle*, although Foderé says you can hear it more distinctly with the naked ear, we have seldom or never met with a case in which it could be heard with the naked ear that it could not also be heard quite as distinctly with the stethoscope; and cases frequently occurred to us where, from its situation and existing at a small point only, it was impossible to detect the sound by the naked ear, when it was sufficiently distinct with the intervention of the stethoscope." (Obset. Auscult. p. 75.) Hence, mediate auscultation is preferable for the following reasons: *First.* It can be used where the naked ear cannot conveniently be applied. *Second.* A more minute exploration can be made with the stethoscope than with the naked ear. Hence the different sounds that occur in the abdomen will not be confounded. *Third.* It is preferable from motives of delicacy, being less objectionable to the patient. *Fourth.* Our diagnosis in doubtful and perplexing cases is more certain, and consequently shields us from the calumny and regret that follows an incorrect diagnosis. However, though mediate auscultation is preferable, it is not meant to exclude the other method, as cases frequently occur in which we may reap decided advantages from having recourse to both.

The first indication of pregnancy detected by auscultation is termed by the French, *souffle*. Kergaradec terms this sound *battemens simples avec souffle*. M. DeLeus terms it *pulsations placentaires*. But *souffle* has the advantage of being still shorter and equally as intelligible, besides it has come into general use.

By applying the stethoscope or the naked ear to the abdomen of a pregnant female, provided the pregnancy be sufficiently advanced, we shall observe, in nearly every case, a peculiar blowing or hissing sound. It assumes different varieties, which Laënnec describes under the term, *bellows' sound*, which he likens to a continuous murmur, similar to that of the sea; familiarly exemplified by the application of a large shell to the

ear ; the rasping or sawing sound which is occasionally found so exactly imitated as to lead the listener to imagine an artizan at work quite close to him ; and the musical or hissing sound so well described by Laënnec. A sound, resembling the cooing of a dove, is sometimes observed, but this is comparatively rare. A more frequent peculiarity to be noticed, is a strange drone resembling that of a bag-pipe accompanying the sound, but without interfering with it. The most constant form we meet with, however, is a combination of the bellows or sawing with the hissing sound, commencing with the latter ; and this is in general, so protracted, that the last *souffle* is audible when the subsequent one commences. These sounds are, from the distention of the uterus and consequent facility of examination, easily detected in advanced pregnancies ; and although not so loud or sonorous in the earlier stages, yet, to the practised ear they become equally distinct. The *souffle* is most frequently met with in the lateral and inferior parts of the uterus ; but it may be found in any part of it ; however, cases will occur, though rarely, even if proper precautions be had recourse to, where we shall not be able to detect it. What is the cause of the sounds in question ? No doubt the vascular structure and peculiar circulation of the uterus act an important part in the production of these sounds ; hence, a brief inquiry into the nature of this structure would not be out of place here.

The ovum of the human female is, on its descent into the uterus, unendowed with those peculiar inherent conditions necessary for its development and the growth of the fœtus. It is therefore necessary that there should exist a connection between it and the mother, and upon this relation must depend its arriving at maturity, the cessation of which not only checks its further growth, but actually puts an end to its existence. This consideration explains the necessity *first*, of an external organ capable of connecting the embryo with the mother ; and *secondly*, of an internal conformation adapted to this external organ, and possessing structural peculiarities necessary for its temporary existence. The organ most essential for the growth of the embryo, by the vascular connection which it sustains between this and the parent, is the *chorion*, which varies in different animals in its means of effecting this object, existing either in the form of a vascular membrane, with a villous collection of vessels on its surface, and corresponding villi on the uterus, as we see in the *solipeda*, and more distinctly in the *swine*, assuming a cotyledonous form, as observed in the cup-like bodies of the ruminantia : or expanding into the most perfect form of placenta, as exemplified in the human subject. In the human ovum during the first weeks of impregnation, we find a number of very delicate vessels running on the whole outer surface of the

chorion, and exhibiting a flocculent and vascular appearance ; after which period, these vessels are observed to extend over a surface relatively smaller as the pregnancy becomes more and more advanced. In the second month, this general flocculent vascularity having diminished much in the extent of surface over which it was observable, the decidua, or uterine membrane which covers it externally, becomes united to the floating extremities of its vessels, and by such union at this vascular part the placenta is formed.

The placenta thus formed, bears a greater proportion in size to the fœtus and uterus in the early than in the latter months of utero-gestation. In the second month it extends over a space nearly equal to one half the surface of the inner wall of the uterus ; in the third, it continues to decrease in extent, but evidently increases in thickness, becoming more compact in structure ; and, in the last month, it covers about one-fourth of the inner wall of the uterus. It is generally of an irregularly circular, but occasionally of a slightly oval figure, subject, however, to variations.

If we examine the uterus of a woman who has died immediately after delivery, we shall find at some part of the inner wall of the uterus a raised uneven patch, smaller or larger as it is more or less perfectly contracted. This patch consist of a membrane of projecting extremities of vessels, some filled with grumous clot, while others are gaping and empty. This is the portion of the uterus to which the placenta is attached, and these the mouths of the vessels by which the connection between the uterus and the placenta is sustained. There is marked difference of structure existing between that part of the uterus to which the placenta is attached and the remainder. The placental portion consists of a congeries of vessels, tortuous, ramifying, and expanding into cells or sinuses, whilst the remaining part exhibits the parenchymatous structure of this organ, with merely occasional vessels interspersed. This extreme vascularity is confined to a circumscribed portion, changing, not gradually, but abruptly, into the ordinary uterine texture. In the neighborhood of the ligaments, at the lateral portion of the uterus we find a fuller distribution of vessels, even when the placenta is not attached, as the principal vessels which connect the uterus with the maternal system pass into it at these points. Hence, upon this distribution of the uterine vessels, and the transmission of blood through them, the phenomenon in question, *i. e.* the *souffle* depends. Though, it may be produced by the passage of the blood through the arteries at the lateral part of the uterus without the placenta's being attached to it; yet, of this fact we have not the most certain proof. For if we examine a woman shortly before delivery, we will only find the *souffle* present while the uterine circulation is connected

with that of the placenta, and ceases when the vessels which serve to sustain this connection are no longer pervious; otherwise, when after delivery, the uterus is empty and perfectly contracted, or, the fœtus having died *in utero*, a complete obstruction in this system of vessels is produced, in which case not the slightest vestige of the placental *souffle* can be discovered. The *souffle* is periodical, alternating with intervals of cessation, and corresponding in every case with the mother's pulse at the wrist, with which it is synchronous. Hence, this phenomenon depends, *first*, *upon the transmission* of blood; *secondly*, on this blood being transmitted *through arterial*, not venous tubes; and *thirdly*, on these tubes being situated *in the uterus as described*. The manner in which the *souffle* is produced yet remains a subject of discussion. Laënnec regards it as depending upon spasm. Dr. Corrigan ascribes it to the currents produced by the passage of blood from a narrow orifice into a wider tube, which is corroborated by the researches of Dr. Kennedy, of Dublin.

There are many sounds, which with one unaccustomed to stethoscopic phenomena, might lead us into gross error, either from the resemblance they bear to the *souffle* or the effect they have in concealing it. The respiratory murmur is sometimes conducted from the lungs across the thoracic to the abdominal parietes, as well as the sonorous *râle*, which resembles somewhat the *souffle*, is occasionally conducted in the same way. But these sounds correspond in pregnancy with the respiration, whilst the *souffle* is synchronous with the pulse at the wrist, or in other words, we ought to count three placental sounds to one respiratory or sonorous *râle*. The intestinal murmur or noise produced by the passage of flatus from one portion of the intestines into another might also lead us astray unless the examination be very careful, at the same time feeling the pulse at the wrist, and whether the sounds we hear correspond with it. But the sound most likely to deceive us, is that which occurs in the abdominal aorta or its great branches denominated by the French *bruit de soufflet*, which so nearly simulates that of the true placental *souffle*, that the most skilful observer is apt to be deceived. In this the pulse affords us no assistance, both being synchronous with the heart's action. *Bruit de soufflet* is confined to a small circumscribed spot immediately over the aorta, while the true *souffle* in pregnancy is heard indifferently over the whole abdomen. *Bruit de soufflet*, when it occurs, generally arises from aneurism, hæmorrhage, hysteria, or nervous states of the system, by its concomitant symptoms; and in the latter cases, to use the words of the great Laënnec, "when the bellows' sound exists in the aorta, particularly the ventral portion of it, there is always a marked state of disorder in the nervous system, viz: agitation and anxiety, faintings more or less complete, and produced by the slightest

causes, and an habitually quick pulse." (Laënnec, by Forbes, 2d edit., p. 698.) Pronouncing decidedly in cases of doubtful pregnancy, before the quickening period, has always been looked upon by practical and scientific men as a hazardous undertaking; but, where we detect a placental *souffle*, which in a great majority of cases is practicable at a considerably earlier period, that phenomenon, in conjunction with the other evidences, will tend greatly to establish the presence of this state. The co-existence of the pulsation of the *fatal heart* with the placental *souffle* decides the question of the existence of a *fœtus in utero* beyond even a reasonable doubt. What can possibly guide us with more accuracy in our search after a *fœtus in utero* than the first and last sensible evidence we have of its material existence, to-wit: the circulation, as proved by the action of that organ, which has been so poetically described by the illustrious Harvey, as the "*primum vivens, ultimum moriens?*"

In exploring the abdomen of a pregnant female at the full period, whose child is alive, we shall detect over a surface, more or less extensive according to the position of the fœtus, and disposition of the fœtal and maternal organs, a pulsatory sound, generally much more frequent than the pulse of the mother, and exhibiting the characteristic marks of a distinct and independent circulation. The space over which the action of the heart of an adult is heard is comparatively small; but in this respect, that of the fœtal heart's action is quite different. If we take a new-born infant, and apply the stethoscope carefully to it, we shall find that it is audible quite distinctly at the right scapula, and of course all over the chest; and by pressing the arms of the child against its chest, and applying the instrument on the arm, we shall be able to detect it plainly on the left and generally on the right arm; in some cases it extends down the back to the loins, and even to the buttocks, and occasionally it is perceptible over a considerable part of the abdominal surface. The part, however, in which we hear it most distinctly, is, the cardiac region; its character being that of a quick double pulsation, miniature imitation of the adult heart, though proportionally smaller and wanting in a great measure its impulse, but clear, quick, and abrupt, offering as distinct a double pulsation, and possessed of characters rendering it almost as easy of discrimination as that of the adult. In the fœtus, the chest is comparatively much smaller, from the fact that the respiratory organs do not at this period require its development. The fœtal heart is much larger, and its chambers of much greater capacity in relation to the thickness of their walls; and, as Laënnec has it, "the extent of the heart's pulsation is in the direct ratio of its thickness and weakness, and consequently, inversely, as to its thickness and strength;" however, this applies more particularly to the ven-

tricles. The lungs of the fœtus being compressed and carneous from their solidity, serve to conduct and transmit the sound much better than if they were distended with air, as solid bodies are the best conductors of sound. We have every day examples of this in solidification of the lungs, whether hepatized, as in pneumonia, or much compressed by effusion into the chest, in both of which cases the sound of the heart is transmitted more distinctly and extensively. The thymus gland may also no doubt, assist in this respect. The sounds emitted by the contraction of the heart, and passage of the blood into the auricles and ventricles, when the pulse is above seventy or eighty, are difficult to discriminate from each other, even in the adult subject, more particularly when the walls of the ventricle become thin from disease ; but when the pulse becomes quick, as in the fœtus, this becomes impossible, unless in a few cases. In the fœtal heart we find little if any impulse, but the sound of the pulsation is audible over a much greater surface than in that of the adult. The walls of both ventricles are in every case nearly of equal thickness and capacity in the fœtus and new-born infant, the disproportion taking place subsequently. This may possibly be explained on the well-known law in physiology, that a *muscle becomes developed in proportion to the degree of force and action it is called upon to exert, and vice versa :* until the closing of the arterial duct, the right ventricle has to assist the left in accomplishing its task, *i. e.* propelling the blood through the whole system ; whereas, afterwards, this entirely devolves on the left ventricle, while the exertions of the right are merely necessary to propel the blood through the pulmonic circulation.

M. Mayor, of Geneva, first detected a sound of a double pulsatory character, which he attributed to the fœtal heart's action ; however, but little attention was paid to it until M. Kergaradec's observations came before the profession, which established the fact beyond doubt. However, Dr. Johnson, of Dublin, Dr. Waller and others of equally high reputation, have never been able to detect the fœtal heart ; but this proves nothing, for the fœtal heart may be even detected by auscultation, and yet *they may never have observed it.*

The pulsation of the fœtal heart is much more frequent than the maternal pulse, its ordinary proportion of beats, or rather double beats, being from 100 to 140 in the minute, sometimes slower and at other times much more rapid. This variation in the fœtal pulsation may depend upon a variety of causes, some of which are inherent—depending upon the fœtal organism, the true nature of which, is necessarily beyond our reach ; while others depend upon the muscular action of the *fœtus in utero*, and those which directly operate on the fœtal circulation, as uterine

action, particularly when long continued, as in labor. But there is nothing more striking than the effect which certain impressions, whether mental or bodily, made upon the pregnant female, appear to be communicated to the fœtus. Sudden fear or joy, when the circulation of the mother becomes affected by them, produces a very decided, although not always corresponding, effect upon the fœtal pulse ; hæmorrhage, and even copious venesection, produce a similar effect. Dr. Kennedy speaks of a pregnant female, who came into the Lying-in Hospital, at Dublin, January 16th, 1830, with acute pleuritis. She was suffering greatly from general irritation and difficulty of breathing, with a hard and full pulse, beating 140 in the minute. The fœtal heart's action was perceptible over a large space, beating about 180 in the minute. The patient was bled to the amount of eighteen ounces, whereupon her pulse became softer and more frequent, beating 150 ; the fœtal pulsation also appeared full and strong, only beating 150. The blood was permitted to flow till about twenty ounces were taken ; the maternal pulse now beat 170, but the fœtal heart only beat 92. Syncope being produced, the fœtal pulsation was observed to vary, one minute beating 92, then to 100, and to 120 ; when the patient recovered her pulse beat about 130, and that of the fœtus ascending to 135 ; it continued to vary two or three minutes for half an hour, when it ceased altogether.—(Obstet. Auscultation, p. 109.)

The woman was delivered of a dead child. Hence we conclude that there is a very sensible influence or sympathy between the maternal and fœtal circulations ; and that this connection is not necessarily a proportionate correspondency with the pulse. It has been proven by Reuss that the death of the *fœtus in utero* does not depend upon the want of a due supply of blood in the fœtus, the heart and vessels of the latter having been found gorged with blood, in a case where the mother had died of hæmorrhage at the seventh month, after having been drained, as it were of the last drop. Hence, the removal of the blood from the mother from hæmorrhage or any other cause, acts, not from the want of a due supply of blood to the placenta, but by the absence or the want of the renewal of that part of it which is necessary to excite the brain and heart of the fœtus to healthy actions and sympathies ; thus, acting as a sedative upon the fœtal circulation, and its heart's action, exciting it to inordinate action, or lessening it in force and frequency. In this way, therefore, we may explain the death of the *fœtus in utero*. I have examined several fœtuses that died *in utero*, and invariably found the heart and great vessels loaded with a dark blood, as well as the sinuses and vessels of the brain. Hippocrates taught that the loss of blood was a certain means of destroying the fœtus *in utero*, and he even frequently had recourse to venesection

with a view to produce abortion, [Hippoc. sec. 5th, chap. 31] yet the observations of Mauriceau, Dr. Rush and others, go to prove, that blood may be abstracted from the mother to almost an incredible extent, without destroying or even injuring the fœtus. To the truth of which, as a general thing, every practical physician must bear testimony. Yet I have known the life of the fœtus to be sacrificed to the too free and injudicious use of the lancet; but as to its utility in difficult parturition, as well as in many other conditions of utero-gestation every practitioner will readily acknowledge. The fœtal heart's action is in some cases much stronger and more distinct than in others. D'Outrepont speaks of a female who had borne five children, who, in the last months of utero-gestation not only felt the pulsation of their hearts, but even heard it as often as she was alone and in a tranquil frame of mind, and everything quiet around her.

The fœtal pulsation also assumes a very singular character in many cases, *i. e.* a metallic resonance, resembling the *tintement métallique*, or sound perceived in cases of empyema, with fistulous communication of the lungs. M. Dubois has met with one case where the fœtal pulsation was of this character, and M. Caseaux has exhibited two cases at his clinic in Paris. But in most cases it resembles the ticking of a watch, and can be heard over a surface of about three or four inches square in the inferior part of the abdomen, sometimes in the right, and other times in the left iliac region, and even in the centre, extending completely across the uterine tumor. In advanced pregnancy, it can frequently be met with more distinctly at a point mid-way between the umbilicus, and the anterior superior spine of the ilium. It is sometimes observed at this point in early pregnancy, and oftener over the pubis, and it is even not unfrequently detected much higher in the abdomen, nearer the umbilicus, at this period. But the audibility of this pulsation is materially influenced by the position of the fœtus; for, as the region of the heart of the fœtus approaches more to the parietes, so will the sound be more audible, and *vice versa*. When we find the head of the child at the inferior part of the uterus, we shall find the fœtal heart by exploring the lower part of the abdominal tumor; but when the head is placed superiorly, we shall be obliged to seek for it at a part of the uterus near its fundus. But the usual position of the *fœtus in utero*, is, the head rests at the brim of the pelvis, the occiput at the os uteri and the chin pressed against the chest, lying with its limbs doubled upon its abdomen, its arms across the chest, the back towards the side of the mother, and of course, its limbs and front towards the other, thus, bringing its shoulder and side forwards; hence, it is evident, where we find this position generally observed, *i. e.*

the shoulder or part of the back coming in contact with the walls of the abdomen, that we shall not find the pulsation at the central, but rather at the anterolateral part of the abdomen, a little to the right or left of the median line, over the ramus of the pubis, according to the side at which the child is placed. It is a matter of facility to detect the fœtal heart in the right iliac region, when the child is placed with its left side towards the abdominal parietes at this part. Where a great quantity of liquor amnii exists, and the uterus much distended, the fœtus does not approach as near to the abdominal parietes, consequently the sound will be less distinct. This is the reason why the sound is not heard in the earlier months; for from the fifth to the sixth month the growth of the fœtus bearing an increased ratio to the liquor amnii, the heart's action by degrees, becomes more distinct. This phenomenon is generally not to be detected until the period of quickening, from the fact that at this period the uterus has risen out of the pelvis, and allows of our coming more immediately in contact with that part of it where the embryo is contained. However, as early as the fourth month by close attention and considerable perseverance, we may succeed in detecting the fœtal heart; when this is the case, we shall observe that the quickening period has come earlier than usual. From the quickening period, the fœtal heart's action increases daily in strength, and becomes more readily detected. The extent of surface over which it is audible also increases in the same ratio.

With reference to the *value* of the fœtal heart's action as an evidence of pregnancy, we cannot hesitate for a moment to pronounce it a *positive evidence of pregnancy*, from the following considerations, to-wit: first, from the facility and certainty of its detection after a certain period, renders it *positive in all cases*, and particularly in those cases where great accuracy in diagnosis is required; second, in point of delicacy, as well as accuracy, it is, at least a vast acquisition to the accoucheur in forming his conclusions; third, when detected, it proves an *independent* circulation from its not being synchronous with the maternal pulse at the wrist, which proves incontrovertibly the presence of a *fœtus in utero*.

Dr. Ramsbotham remarks: "The presence of the fœtal pulsation is a positive sign of the existence of pregnancy."—(Syst. Obstet. p. 87, Amer. Edit.) Dr. Bobson also regards it as good evidence. If its presence proves so much, what does it absence prove? It does not prove that the female is not pregnant for the following reasons: *first*, in the early months of utero-gestation we cannot detect it from its depth, and the quantity of liquor amnii; *second*, when the fœtus is dead, as a matter of course, it ceases; *third*, the fœtus may be exceedingly feeble, so much

so, as to make its detection impossible. Although the fœtal pulsation approaches nearer to what we term *positive evidence* of pregnancy than any of the numerous evidences we have referred to heretofore, yet, there are many possible sources of deception in exploring for the fœtal pulsation. The pulsations with which it is most likely to be confounded are, the iliac arteries, the abdominal aorta, or even the maternal heart, where its action extends a considerable way over the abdomen. But the peculiar double beat of the fœtal heart will serve to distinguish it from those of the iliac arteries ; the pulsation of the aorta affords us much more embarrassment by rendering the fœtal pulsation inaudible from their greater sound and impulse, especially when the pregnancy is at a very early stage, and the heart's action extremely feeble ; but, in general, this can be obviated by drawing the uterus a little to one side with the hand. Where we suspect the fœtal heart's action of being confounded with that of the maternal heart, we can generally satisfy ourselves by tracing the sound from the fundus of the uterus into the maternal cardiac region, and should it possess the peculiar impulse and double beat, being synchronous with the pulse at the wrist, particularly if it grows stronger the nearer we approach the cardiac region, we can, without much hesitation pronounce it the maternal heart. The action of the abdominal muscles, the contraction of the uterus, if the patient be in labor, the pulsation of the temporal arteries of the individual making the examination, or the peculiar irritability or nervous habits of the female, will frequently afford us much embarrassment.

It is also well for the accoucheur to be able to distinguish the *souffle* and *fœtal pulsation* from the *funic souffle* in his explorations for the *fœtus in utero*. The funis of the human subject consists of one large vein, which conducts the blood from the placenta to the umbilicus of the fœtus, and two arteries, which conduct the blood from the iliac arteries of the fœtus through the umbilicus to the placenta. Or as M. Baudelocque has it : "Cet ordre n'est cependant pas immuable, puis qu'on n'a trouvé qu'une seule artère dans plusieurs cordons."—(Vol. I., p. 165.) A quantity of gelatinous matter, contained in cells, gives it consistence, and to a certain extent prevents the bad effects of pressure upon the cord. Over its proper cellular membrame or elongation of the chorion, there is also to be found a reflexion of the amnion. The fœtus is at first attached by its abdomen, very closely, to the part of the uterus where the placenta afterwards develops itself, with scarcely any intervening funis. This organ gradually extends itself until at the time of birth, when it is generally about two feet long, and about five or six lines in thickness, flattened in the early months, but towards the close of gestation it assumes a more

rounded form. However, Dr. Maygrier, in his Mid. Illustrated, (p. 116,) mentions a case where the funis was *five* feet, and another where it was *six* feet long. Dr. Kennedy met with a case where the funis did not exceed *eight inches* in length.

We have a constant transmission of blood through the umbilical arteries, along the funis propelled by the conjoint actions of the ventricles, accompanied by a distinct pulsation or *souffle*, corresponding to that of the fœtal heart, existing as long as this distinct circulation of the fœtus in utero continues, and ceasing at the death of the fœtus, or its expulsion from the uterus by labor.

This *funic souffle* in particular positions of the funis in the uterus, can be distinctly observed by applying the *stethoscope* externally. This pulsation is equal in force and frequency to that of the fœtal heart. These umbilical vessels possess individual vitality and the power of contraction, depending on a spasmodic or inherent narrowing of their coats.

Professor Chaussier discovered a peculiar *nervous filament* in the funis, which gives us great reason to believe in the possibility of this *funic souffle* being produced by the spasmodic narrowing of the umbilical vessels. This view of its cause is corroborated by the experiments of Hunter, Lobstein and Osiander. The *funic souffle* is generally discoverable higher up than the fœtal heart, between the left ramus of the pubis and the umbilicus, corresponding in frequency with the action of the fœtal heart. Drs. Hans, Hohl and M. Dubois regard it as impossible to distinguish the *funic souffle* through the abdominal parietes, believing the pulsation to be, in all cases, merely the pulsation of the fœtal heart.

The foregoing evidences are applicable more particularly to that state of utero-gestation, usually denominated *Simple Pregnancy.*

In my next paper I shall speak of those evidences which distinguish from each other those conditions termed, *Compound*, *complicated* and *pseudo-pregnancy.*

[To be Continued.]

ART. II.—*On the Proximate Cause of Fever:* By Dr. H. BACKUS, of Selma, Alabama.

FROM the earliest records of medicine to the present time, no subject has more thoroughly engrossed the attention and research of the medical mind than that of the proximate cause of fever. No other subject in medicine has been deemed more complex and mysterious, and upon no other has a

greater diversity of opinions been held. It is not our purpose to examine the points of difference in the opinions or speculations of our predecessors and contemporaries upon this question, but rather to seek their points of agreement, to analyze their recorded observations and experiments, and see if the time has not arrived, when, by a rigid application of the inductive method of investigation, it may be stripped of its complexity and reduced to its simple elements.

To render our argument clear and unequivocal, to thoroughly dissect the subject and expose its constituent parts, we advance the following positions:

I. That all exciting and predisposing causes of fever, produce a state marked by the terms debility, prostration, and congestion.

II. That congestion, venous congestion or an increased retrograde pressure of venous blood in the capillaries, is competent to the production of all the phenomena, as flux, hæmorrhage, vomiting, dyspnœa, pain, stupor, delirium, convulsions, coma, etc., etc., which attend or coëxist with fever.

III. That venous congestion precedes and coëxists with fever.

IV. That venous congestion is competent to the production of fever.

V. That venous congestion is a constant post-mortem appearance, whether there be recognized organic change or not.

I. *All exciting and predisposing causes of fever produce a state marked by the terms debility, prostration and congestion.*—"*Debilitating* causes of predisposition are the most numerous of any. So we might expect from the fact that constitutional strength generally implies power of resisting disease. The weakness which renders the body liable to disease, is that especially which enfeebles the heart and impairs the tone of the arteries; it is often accompanied with an unusual susceptibility of the nervous system, which increases the liability of the body to suffer. The following are the chief of this class: Imperfect nourishment—impure air—excessive exertion of body or mind—want of exercise and sedentary habits generally—long continued heat—long continued cold—habitual intemperance with intoxicating liquors—depressing passions of the mind, such as fear, grief, and despondency—excessive and repeated evacuations, either of the blood or of some secretion—previous debilitating diseases." (William's Principles of Medicine, pp. 23-28.)

"Malaria and the influences which produce continued and exanthematous fevers, seem to have the same effect as external cold, but it is not so easy to explain how they operate. The cold stage of these diseases exhibits, in a high degree, the marks of intropulsive congestion; and it is well known that in ague, the congestive enlargements of the liver and

spleen are among its most remarkable phenomena. The congestions remaining during the febrile stages of fevers, seems to be the chief causes of their inflammatory complications." (*Ibid.* p. 152.)

"The direct effect of most of this class of causes, [infections] is depressing, and when they are strongest and prevail most, the resulting disease is one of depression, adynamia, asthenia, or prostration of the vital powers. These causes as exhibiting a noxious property opposed to life, are therefore commonly called *specific poisons*. But there is the antagonist principle of vital resistance in the system, which leads to various processes of reaction, which may be exhibited in different degrees, according to the relative strengths of the poison and of this resisting power." (*Ibid.* p. 67.)

"As the most proper commencement of a systematic treatise of surgery, we shall begin by describing a state commonly known as *prostration*, collapse, or shock to the nervous system; by which terms we signify that general depression of the powers and actions of life which immediately follows any severe injury, such as a compound fracture, or gun-shot wound. Causes: great and sudden extremes of grief, or joy, or fear, or cold; large doses of any active poison, such as arsenic, or sulphuric acid, or tobacco; the sudden impression of miasmata, or of morbid poisons, as the plague, great loss of blood and mechanical injuries." (Druitt's Surgery, 1st chap.)

"The causes of *congestion* are, therefore, 1st. Those which act by primarily depressing the organic nervous influence; such as advanced age; the continued or prolonged impression of cold, mental anxiety, and all the depressing passions and moral emotions; prolonged sleep, mental and physical inactivity; miasmal, contagious or infectious emanations; various vegetable, animal, and gaseous poisons; and the rapid loss of the natural electrical tension of the frame. 2d. Those which mechanically impede the return or circulation of the blood itself, or which change its quantity and quality, either locally or generally, as excessive heat; general plethora, produced either by too full living, or by the suppression of the natural or accustomed discharges, interrupted circulation through the heart, the lungs, liver, etc., a long-retained posture by debilitated persons; the use of unnecessary ligatures and tight lacing; improper and unwholesome food; contamination of the blood by the absortion or introduction into it of noxious mineral, vegetable, and animal substances, or gaseous fluids; and changes taking place in its constitution, from the interrupted secretion and elimination of hurtful matters from it, these latter causes affecting the vital manifestation of the vessels and nervous system. 3d. Those causes which exhaust the irritability or vital tone of the vessels, by previously exciting

them above their natural state of action." (Copland's Medical Dictionary, vol. 1, pp. 468-9.)

"It appears from the preceding pages, that weakened action of the heart is the effect, directly or indirectly, of the remote causes of fever. This, therefore, is a link of the chain of causes and effects extending from the remote causes to the symptoms of fever. This conclusion is confirmed by the fact long since ascertained,* that all fevers are preceded by weakened action of the heart." (Cooke's Pathology and Therapeutics, vol. 1, p. 344.)

It will be observed that the above extracts are exhaustive of the exciting and predisposing causes of fever; and also that they are repetitions, in great part, of each other. This latter fact indicates that the terms debility and congestion have a signification in common, or that they are marks of a common state; and this view is strengthened by the consideration that the same pathological phenomena, as flux, dropsy, etc., are ascribed to each of these terms as the proximate cause. As these terms occur more frequently in medical writings than any other, when the cause of pathological phenomena is under consideration, (a fact, in itself of great significance), it is of the first importance that we have a clear conception of their real import. Much confusion arises from not looking below a name to the state of which it is a mark; as for instance, when it is said that debility is the cause of dropsy or fever. Now the ancients ascribed all dropsies to debility, as their cause; and Cullen, Brown, and Rush ascribed all fevers to the same cause; but it was never clearly apprehended how debility or diminished action could be the cause of fever or increased action; and although all felt, as is evinced by their writings, that there was a connection, yet the difficulty of comprehending that connection as cause and effect was so great, that the doctrine never attained to a generally received theory. But if debility implies congestion, the difficulty may be in great part removed, since the state marked by the term congestion is the known cause of dropsy—as may be verified at any time by experiment—and it is an admitted or recognized fact that an increased quantity of blood (within certain limits) in a part, (which state is marked by the term congestion) increases the action of that part.

This is illustrated in the familiar fact that a certain degree of congestion of a glandular organ, increases the action or secretion of that organ; but if the congestion passes a certain degree, it diminishes its secretion or action. The same is true of the blood vessels. A certain degree of congestion of the heart excites that organ to increased action, as is illus-

* Boërhaave's Practical Aphorisms, 563. Cullen's First Lines, 34, 46. Darwin's Theory of Fever, Supplement, 1, 1, 6. Rush's Works, vol. 3, pp. 3, 4.

trated in hypertrophy; but if it passes a certain degree it diminishes its action, as is seen in dilatation. This increased quantity of blood in a part, is also marked by the term debility, and implies a relative deficiency of power on the part of the vessels, or a yielding of the walls of the vessels to the outward pressure of blood; a relatively (debility being a relative term) increased pressure of blood to the returning pressure of the heart or blood vessel. The following extracts will help to illustrate this point, and it will be seen that although one is applied to the capillaries and the other to the heart, yet their language is similar, and doctrines identical:

"The causes of congestion, always mechanical, may be divided into two great classes, which we often find conjoined. 1st. Those causes that act by obstructing the return of the blood through the veins. 2d. Those that act by enfeebling the walls of the capillaries and veins, so that they are no longer able to withstand the outward pressure of the contained blood." (Erichsen's System of Surgery, p. 44.)

"The causes of dilatation are: 1st. Deficient power of the heart, whether congenital or acquired, in proportion to the system. 2d. In general terms, all obstructions to the circulation, whether situated in the orifices of the heart, or in the aortic, or pulmonary system. Dilatation of the heart is a purely mechanical effect of over-distention. Blood, accumulated in its cavities, exerts a pressure from the centre towards the circumference in every direction; and when once it surmounts the resistance offered by the contractile and elastic power of the parietes, these necessarily yield and undergo dilatation." (Hope on the Heart, p. 293.)

The terms debility and congestion, then, imply an antecedent changed relation, are marks put upon an accumulation or increased quantity of blood in a part, a relatively increased pressure of blood to the returning pressure of the heart or blood vessel. It is not correct, therefore, to say that debility is a cause of congestion, since the term cannot be applied until after the change has transpired; it is a mark put upon an already existing pathological state, not a cause of that state. Names do not *precede* states, or changes, but are marks put upon them *after* their occurrence, and it is to this changed relation that we must look, this accumulation or increased pressure of blood, when the terms debility and congestion are used, if we would have a clear and correct conception of their real significance, and not to the terms themselves as the cause of anything.

These terms, then, imply a changed relation, an *increased* pressure, and the importance of clearly understanding this fact, may be illustrated as follows: it might be admitted that a certain degree of congestion of the brain, for instance, would produce convulsions, or coma; but then it

might be replied that *other* causes, as the pressure (pressure being a known cause, as has often been verified by experiment) of a tumor, a spicula of bone, etc., produce the same effect, and therefore, congestion is not the *only* cause, so that the theory is insufficient for all cases. In reply, if the term congestion is a mark of increased pressure, it agrees with the known causes, and therefore the theory is perfect. In the brains of Epileptics there are sometimes found organic changes, tumors, etc., which are said to be causes of the paroxysms ; but as these alleged causes are permanent, and the attacks occasional, it is evident that they are only coöperating causes ; they do not exert sufficient pressure of themselves to produce a paroxysm, but require a slight degree of congestion or increased pressure of blood, to determine its occurrence. This state of the circulation, this pressure of blood, is a *varying* state; hence the *occasional* occurrence of the paroxysm.

II.—*Venous congestion or an increased retrograde pressure of venous blood in the capillaries, is competent to the production of all the phenomena, as flux, hæmorrhage, dyspnœa pain, stupor, delirium, convulsions, coma, etc., etc., which attend or coëxist with Fever.*—" In one rabbit I tied the jugular veins on each side of the neck. When it was at liberty, it ran about, cleaned its face with its paws, and took green food. Its respiration was reduced to sixty-eight inspirations in a minute, which is about half the natural number. After four hours it ran about as if nothing had happened, and eventually recovered. When it was killed and injected, I found on each side, three anastomosing veins, passing from the anterior to the posterior part of the jugular veins, and conveying the blood from the head to the heart; but the vertebral vein had remained whole, and become enlarged, and it passed on the forepart of the vertebra, from the head to the space between the fourth and fifth cervical vertebra, where it entered the vertebral canal. In a second rabbit, I tied the jugular veins on each side of the neck as before. The animal's respiration became slow, but it ate green food, ran about, and was difficult to catch; but for five days after it appeared dull ; its ears had drooped. On the seventh day it was seen to be convulsed, and frequently rolled over. Its voluntary powers were lost, as well as its sensation, in a great degree. On this day it died. On examination, a clot of blood was found extravasated in the left ventricle of the brain. Hence it follows that apoplexy will occasionally result from an obstruction to the return of the blood in the jugular veins, and this I have known to happen from enlargement of the glands in the neck of a boy." (Sir Astley Cooper's Experiments. Guy's Hospital Reports.)

"The fact, then, which is beyond dispute, of the frequent preëxistence of local engorgements and distention of the capillary circulation, gives

support to the hypothesis that, (in certain cases at least), the issue of blood results from pressure, whereby the blood in substance is urged through passages naturally impermeable by its red particles, but now mechanically dilated in consequence of the *vis à tergo*. Although the dilatation cannot be made sensible to the eye, this seems the simplest and most obvious explanation applicable to some forms of idiopathic hæmorrhage, and to the secondary species of that which is symptomatic. That blood may be thus exhaled, independently of any disease of the vessels themselves, we know from experiments made on animals, and from the observation of what sometimes occurs in the healthy human body. Boërhaave produced hæmorrhage into the intestinal canal of a living dog by placing a ligature on the vena porta. An extreme turgescence of the whole venous system in one of the effects of sudden strangulation. Dr. Yelloly accordingly found such turgescence conspicuous in the bodies of five criminals who had recently suffered death by hanging; and in two of these instances, blood in considerable quantity had exuded from, and coagulated upon, the mucuous membrane of the stomach." (Watson.)

"The adequacy of venous obstruction to produce dropsy, is well illustrated by some experiments of Lower. He tied the jugular veins of a dog, expecting the animal to die of apoplexy; instead of this result, the face and head of the animal became much swelled with œdema. He then tied the ascending cava; ascites, and anasarca of the lower extremities were the result. Disease affords numerous examples of dropsy and flux from venous obstruction." (Williams' Principles, p. 182.)

"A ligature drawn round any part of the body, so as to intercept the communication of the great vessels and the heart, may cause that part to perish. But the effect of the ligature is not the same in all cases; and it does not always produce mortification in the same way. You apply a bandage round the arm before you bleed a patient, to make the veins of the forearm become distended, the object being merely to stop the circulation in the superficial veins. If you take it off at the end of a few minutes, the hand is at once just as it was before the ligature was applied. If you were to leave it on for twelve hours, the whole hand and forearm would become swollen, and would remain swollen for some time after the bandage was removed. The swelling in such a case arises from the congested state of the veins, and from the consequent effusion of some of the serum of the blood into the cellular membrane. If the ligature around the arm be still tighter, so as to obstruct the circulation to a greater extent, but without arresting it altogether, the same effect is produced, namely, serous effusion, which may continue for some time after the cause which produced it is taken away. The first effect, then, of a liga-

ture which obstructs the circulation without arresting it completely, is to produce serous infiltration of the cellular membrane, and an œdematous swelling. The different kinds of dropsy depend on the same principle. But let us suppose that a ligature is applied in this manner round a limb, and allowed to remain, so that the impediment to the circulation continues. A low sort of inflammation is set up, the œdematous swelling and the tension are aggravated, and this may terminate in mortification." (Brodie's Lectures, pp. 59, 60.)

"Parts may be killed by pressure. The mode of death here is nearly the same as when parts are killed by ligature. The difference being simply this: the pressure is like a ligature applied to a broad surface, operating not on the arterial and venous trunks, but on all the small vessels and capillaries." (Ibid. p. 61.)

"The effects of pressure have often been observed by experiments on animals. If the cranium of a dog is trepanned, and pressure performed on the *dura mater* to a certain extent, the animal shows signs of great uneasiness, and is affected with general convulsions, if the pressure is increased the convulsions cease, the breathing becomes stertorous, the animal torpid and comatose; if the pressure is diminished, the breathing becomes more free and the convulsions return; and if it is entirely removed, the animal soon completely recovers. The principal causes of pressure are congestion, effusion, etc." (Cyclopedia Pract. Medicine, vol. 1, p. 316.)

"I say the blood may undergo important alterations in its *quantity*. It may exist in too great abundance throughout the body; and it may exist in too great abundance in certain parts only of the body. These states have been recognized for ages. Sometimes they are called respectively general and partial *plethora;* sometimes general and local *congestions* of blood; people speak also of irregular *determinations* of blood to different organs; and, of late, the term *hyperæmia*, first invented by M. Andral, in France, has been imported into this country, and much adopted here. All these words and phrases mean in truth the same thing; and their frequent recurrence in medical works is, of itself, sufficient evidence of the frequency and importance of the conditions which they express. If we comprehend rightly this subject of plethora or congestion, we shall be prepared to understand some most important morbid states, of which it seems to be in many, if not in all cases, the earliest approach—the initial step. Inflammation, hæmorrhage, dropsy, all acknowledge and imply a previous condition of congestion." (Watson's Practice, p. 41.)

"In the human subject we have an experiment on a large scale in dila-

tation, or valvular disease of the heart; the necessary effect of which is to increase the pressure of blood throughout the venous system. The phenomena here presented must consequently embrace those already exhibited as having been produced by partial experiments, while many other or new phenomena are added. To briefly enumerate them, as recorded by different writers, there are flux, hæmorrhage, diarrhœa, vomiting, constipation, pain, dyspnœa, cough, expectoration, angina, asthma, headache, stupor, delirium, convulsions, coma, apoplexy, paralysis, hæmorrhage from the nose, the lungs, the stomach, the bowels, the uterus, the bladder; black vomit, jaundice, inflammation, ulceration, mortification, injection of the mucous membranes, enlargement of the liver, spleen, etc.; discolorations of the surface, irregularities of the pulse, disorders of secretion, of vision, of hearing, etc., etc."

"The diseases of the heart exert a very marked influence over the whole economy; nor is it in a narrow or circumscribed circle that these morbid reäctions are produced; on the contrary, how numerous are the sympathies which the central organ of the circulation creates in the rest of the organism! It is in consequence of their *multiplicity*, and the difficulty of referring them to *one* perfectly settled cause, that we have decided to study the general symptoms in a purely analytical order. The engorgement of the venous system is one of the extraordinary phenomena pertaining to diseases of the heart." (Aran on the Heart, p. 100.)

"The symptoms affecting the system in general result from retardation of the blood in the venous system." (Hope on the Heart, p. 358.)

III.—*Venous Congestion precedes and coëxists with Fever.*—That venous congestion coëxists with fever, is a legitimate inference from the fact that it is competent to the production of all the phenomena which attend fever. "And if the local inflammation, which can be ascertained to take place during fever, is inadequate to explain the characteristic typhoid symptoms, it is equally in vain to seek an explanation of these symptoms, as some have done, in the mere circumstances of irregular distribution and congestion of blood. Even the peculiarities of that form of fever which has been described under the name of congestive, are not to be explained by the mere circumstance of internal congestion, the existence of which, in the vessels, and especially in the veins of internal parts, in these circumstances, is admitted. For although congestion or stagnation of blood within the cranium may be held to be a sufficient cause of stupor, yet we are so far from regarding congestion in the great veins leading to the heart as a sufficient cause for deficient action there, and consequent feeble pulse and cold skin, that we have already stated the accumulation

of blood in the great veins to be apparently the chief cause of the increased action of the heart, or the reäction, in the more usual form of fever. In the cases, therefore, where the congestion in the great veins fails to excite this reäction in the heart, some peculiar cause must have operated to prevent the heart from being usually excited by the application of the unusual quantity of its natural stimulus ; *i. e.*, the circumstance of unusually great and permanent congestion of the great veins, in the commencement of fever, is in all probability the effect, not the cause, of a peculiar sedative influence affecting the vascular system in these cases ; such an influence naturally leading to accumulation in the great veins, for the same reason that determines the accumulation there after death. That accumulation of blood in the great veins is not *per se* adequate to account for the phenomena of any form of fever, appears distinctly from the fact, that no form of fever follows the congestion there in cases of suspended animation in syncope, or from extreme cold, or submersion in water."* (Alison's Outlines of Pathology.)

" Where a congestion is extensive it has constitutional as well as local effects. In proportion as blood accumulates in a part, it leaves the rest of the body with less than its proper share, and the limbs and surface generally, may show various symptoms of weak circulation and want of blood. Thus with considerable congestion of the lungs, liver, or brain, the surface is pallid and chilly, the pulse weak and small, the extremities cold ; there is a peculiar feeling of languor or weariness, and all the functions are indifferently performed. Such an effect on the system may be produced artificially, by applying a tight bandage around both thighs at once, or even both arms in a weak person ; the limbs beyond the ligature become congested, leaving a deficiency of blood in the rest of the body. The extreme of this condition is the cold fit of an ague, in which extensive internal congestions are the most pathological changes. As in this example, so with other extensive congestions, more especially if suddenly induced, as by cold, a reäction may ensue, causing quickened pulse and circulation, hot skin, and other phenomena of fever. Where this reäction is vigorous, it may fulfill its object in sweeping back the congested blood into the circulation, and thus restoring the balance. Where the reäction is weak, it will fail to remove the congestion, but constitutes a low feverish excitement, often remittent in type, with depraved functions, foul

*The term congestion is a mark put upon an increased quantity, an increased pressure of blood in a part. It is a mark of a pathological phenomenon, not a cause of it ; and the state marked by the phrase '*Venous Congestion*' is an evidence of the existence and extent of this increased pressure of blood. It is there seen to be general, and the phenomena which shall appear in any given case, will depend upon the suddenness of its occurrence, the degree of pressure in different organs, or parts, and the duration of that pressure. All these conditions may vary in different organs, or cases, and in the same organs, or cases, at different times. Hence the diversity. We saw in the experiments detailed under our second division, that degree and duration of pressure, diversified the phenomena presented.

tongue, impaired excretions, restless nights, etc., which may proceed for an indefinite period, until a critical evacuation by sweat, urine or diarrhœa terminates it, and with it sometimes the congestion which has induced it." (Williams' Principles of Med. pp. 161-2.)

"It may appear inconsistent to speak in the present department of complications, but it is intended that only such lesions of structure shall be noticed here as result from the long continuance of a mild disease, those dangerous organic changes which are almost essential to the nature of malignant intermittent being reserved for future notice. The tendency of the simplest kind of intermittent to affect the viscera of the abdomen is very great, as is shown by the following fact: if any cathartic be given to a patient immediately after even his first fit of ague, a quantity of dark bilious matter is discharged from his bowels. During the cold stage, the blood seems to be largely accumulated in the veins of the viscera generally, and very much so in those of the aortal system, so that we find the functions of the alimentary canal and the liver disturbed early in the disease; and merely by its long continuance, even should its general character be devoid of all malignancy, serious organic affections are occasionally produced. That the accumulation of blood in the viscera during the cold stage is considerably instrumental in engendering them, is shown, not only by general reasoning of a very obvious nature, but by the fact that these morbid affections arise more frequently in the quartan, which has the longest cold stage, than in the other forms of intermittent. To this cause, the remora of blood in the veins of the viscera during the cold stage, is superadded the arterial congestion of the same organs during the stage of excitement." (Cyclopedia Pract. Med., Art. Intermittent Fever.)

IV.—*Venous Congestion is competent to the production of increased action or fever.*—It has now appeared: 1. That all exciting and predisposing causes of fever, produce a state marked by the terms debility and congestion. 2. That venous congestion is competent to the production of all the phenomena which attend fever. 3. That venous congestion precedes and coëxists with fever. We have thus *isolated* increased action or fever, from all *attendant* phenomena; and the question now to be disposed of is: Is congestion competent to the production of this residual phenomenon?

1. There being no valves in the internal system of veins, including those of the head and spine, the increased retrograde pressure of venous blood may be general, thus corresponding with the effect, or general phenomena.

2. As venous congestion is competent to the production of simple pathological phenomena, the inference is that it is competent to the production of the complex.

3. The same reason which constitutes congestion, or pressure of blood, the cause of flux, hæmorrhage, and dropsy, constitutes it the cause of increased action of fever, viz : that it is an invariable antecedent and coëxistent of each, of all.

4. Flux, hæmorrhage, dropsy, and fever, are convertible into each other ; flux, hæmorrhage, and dropsy, are produced by venous congestion ; therefore, fever is produced by the same cause. Fever as often results from a suddenly suppressed flux, as does dropsy ; and is as often relieved by producing a flux, as is dropsy, or hæmorrhage ; and, if all cases of fever are not relieved by producing a flux, alone, neither are all cases of dropsy. The reason of this is to be found in the fact that depletion is not the only remedy for congestion ; stimulants, tonics, astringents, etc., are also remedies for this state, and in certain conditions, are the appropriate remedies.

We have seen that the term congestion was a mark put upon a relatively increased pressure of blood to the returning pressure of the heart or blood vessel. The obvious indication of treatment, then, is either to diminish the pressure of blood, as by depletion, or increase the returning pressure of the solids, as by stimulants, tonics, etc., or both. We may be said to do both when we give blue pill to increase secretion, and follow it with quinine to increase the returning pressure of the heart or vessels ; between the two an equilibrium is established. There is a *point of agreement*, then, in the action of stimulants and depletion, upon which point of agreement, their efficacy depends. Reäction *tends* to remove congestion or equalize the pressure of the blood and the vessels, but failing in this, the increased arterial *vis à tergo* or pressure coöperates with the antecedent retrograde pressure of venous blood to increase the pressure in the capillaries, thus converting what were before called *passive*, into what are now called *active* diseases or phenomena. In dilatation of the heart, which is a chronic ague, and in dilatation with hypertrophy, which is a chronic fever, this is illustrated.

" Obstruction in the right auricle, whether from this or any other cause, presents an obstacle to the return of the venous blood, and therefore causes retardation throughout the whole venous system. Nor is this all ; for the retardation is propogated through the capillaries to the arterial system, and thus at length returns in a circle to the heart. In this way is explained what at first sight appears an anomaly ; namely, that the left cavities are sometimes rendered hypertrophous by an obstruction

in the heart situated behind them in the course of the circulation, as for instance, when the left ventricle is rendered hypertrophous by a contraction of the mitral orifice." (Hope on the Heart, p. 252.)

It is extremely interesting to observe the influence of impeded or arrested flow of blood in the *veins* upon the *arteries*. I have already alluded to the experiment of applying a ligature on the inferior extremity of the frog in which this is seen under the microscope. The phenomenon deserves to be noticed more particularly. If we tie a ligature around the frog's leg, the web being spread under the microscope, we instantly see the whole circulation, which was equable, or nearly so before, become *pulsatory*. If we tie the ligature a little more tightly, the globules of blood are observed to oscillate even in the arteries, and to proceed and to retrograde, at each systole and diastole of the heart." (Marshall Hall.)

The causes of hypertrophy and dilatation are often the same, though acting upon different principles. Whatever stimulates the muscular action of the heart may produce the former affection; whatever has a tendency to distend the walls, may produce the latter. Now no stimulus is greater probably to the muscular fibres than the pressure of the blood within the cavity they surround; and no cause tends more strongly than this to produce distension." (Wood's Practice, Vol. 2, p. 167.)

V.—*Venous Congestion is a constant post-mortem appearance whether there be recognized organic change or not.*—If an increased retrograde pressure of venous blood in the capillaries is the cause of fever and the attendant phenomena, the state marked by the phrase "*Venous Congestion,*" should be a constant *post-mortem* appearance; and that it is such appearance, is universally admitted. Strange, passing strange, that a state so common, so palpable, so competent, as we have seen it to be, to the production of fever and the attendant phenomena, should be so universally disregarded, should be so universally deemed a mere effect, a matter of course.

"It was a happy thought of Glauber," says Sir John Herschel, "to examine what every body else threw away;" and as this *post-mortem* appearance has been universally thrown away, it may be a good idea in us to submit it to analysis; but we have already done this; we have found in it a solution of the whole matter—a present and sufficient cause of all the phenomena in question. The anatomist has subjected the body to minute dissection, the microscopist has peered into its molecular arrangement, the chemist has analyzed its blood, in search of the mysterious cause of fever; while almost the first incision of the scalpel brings it clearly and palpably into view.

ART. III.—*A History of the Diseases of Craven's Creek, and its vicinity, from* 1848, *down to the present time:* By JESSE PEEBLES, M. D., of Lowndes Co., Mississippi.

My object in this article, is to give a succint account of what has come under my own observation, including the details of certain diseases of which I have not seen any accurate description, a notice of their modifications and complications at different times and seasons of the year, and a recommendation of certain remedies, which an ample trial has proved to be efficient in the treatment of those diseases.

The locality of which I write, is situated in the southeastern part of Lowndes county, Mississippi; is bounded on the north, three miles distant, by Dry Creek, and on the south, three miles distant, by King Cage Creek; both these streams run from east to west, and between them run Craven's and Nash's Creeks, having the same general course that the other creeks have. They all empty into the Bigbee River, which is about five miles distant, and which runs from north to south, and forms the western boundary. The eastern boundary is formed by a range of hills and elevated table lands, four miles distant, which commences its gradual ascent about a mile west of this, and at other points equidistant from the river, thus making the base of the range to run parallel with the river, and nearly on a straight line. From this line to the river there is very little descent. The whole scope, six miles long by an average width of three, is remarkably level. The swamp proper is a sandy alluvial soil; is moderately rich, and subject to overflow. The benches, which have an average altitude of about twenty feet above the surrounding swamp, have generally a stiff yellow lime soil, what is commonly called chocolate land, and are more fertile than the lower lands. The swamps extending up the creeks are of the nature of the river swamp, but are richer and less subject to overflow.

The country from the base to the summit of the hills forms an undulating inclined plane; the soil is like that already described, except that it generally contains less lime and is not so productive. It is one of the oldest settled localities in East Mississippi, and consequently the most of it is or has been in cultivation, and the face of the country has undergone but very little change in the last ten years. Alas! the change in the population has been sad indeed. Death came; and the old, the young, the noon of life, the gay, the gifted and the beautiful have gone before the ruthless Destroyer.

At present, the proportion of the white to the negro population is about one to four, and the whole numbers about fifteen hundred.

During the year 1848 there was no epidemic nor any thing remarkable

in the sanitary condition of the country. It was a wet year, and during July, August and September, the rains were frequent and very heavy. From April to October, the diseases were such as are common at that season of the year, viz : intermittent fever, chills, etc., though in a majority of the cases there was more than the ordinary amount of cerebral disturbance, and in two instances there was active congestion of the brain, requiring venesection, cupping, etc., in addition to the ordinary antiperiodic treatment. There were also, what is not unusual here during the summer months, several cases of dysentery among children, *i. e.* high fever and frequent green, ropy, mucous dejections. These cases always yielded to an active purge, followed by the free use of quinine.

During the following winter there was no sickness, except the ordinary catarrhal affections of the season ; the weather was not unusually wet, but was generally warm for the season.

The spring of 1849 opened very early, but on the 16th of April there was a killing frost, followed by lighter frosts up to the 20th, which destroyed almost the entire crops of cotton, corn and wheat, and all the young fruit. Rains set in early and continued regularly up to about the 10th of August. Up to the middle of that month the country was comparatively healthy, but from that time till October there was a very unusual amount of intermittent fever, generally mild, though a few cases were complicated with serous diarrhœa. This year I treated one case of congestive chills, that of Major H., aged about sixty-five years, who was taken with a chill August 18th, and fell in walking across his room. This was about nine o'clock in the morning. I saw him about one o'clock, P. M. He was bathed in perspiration ; very cold ; incoherent ; had no pulse at the wrist, though it was discernible in his feet and his temporal arteries. Gave him two hundred drops of laudanum ; applied a mustard plaster to his spine, and hot rocks to his extremities. Reaction very soon commenced. He took large doses of quinine and recovered. I have not seen a similar case since.

During the fall there was some pneumonia, of the ordinary type, except in one instance, and that case during the early stages presented no unusual symptom, except deafness ; but the fever continued three weeks after all symptoms of inflammation of the lungs had ceased to exist, and when there was apparently no local disease to keep it up. This was the first case of the kind that I had seen. It was the first case in this locality, and in its mildest shape, of that disease which has since, though modified and under different names, typhoid pneumonia, typhoid fever, etc., so terribly scourged many parts of this State and Alabama.

There were also during the fall two cases of fever followed by general

paralysis. The first was a negro boy, Alfred, aged about twelve years, who had two paroxysms of fever for which he took quinine, etc. On the third day after his fever left him he complained of inability to use his legs and staggered in walking, the paralysis gradually got to his superior extremities and became almost complete. He recovered rapidly under the use of strychnine.

In November I treated two cases of dropsy that appeared to be the result of intermittent fever. They both got well under the moderate and continued use of quinine and the extensive use of sal. Rochelle.

The winter and spring of 1850 were very wet; in January we had a large freshet, and in April a larger one. The earth was saturated with water till almost June. The summer was very dry and an immense amount of timber died, particularly on the flat lands. In January and February we had an epidemic tonsilitis. Many cases were complicated with inflammation of the parotid and sub-maxillary glands, generally terminating in resolution, though in a few cases there was extensive suppuration. Some cases were attended with an eruption resembling scarlatina, and which made its appearance from the second to the fourth day of the disease, and wherever this eruption made its appearance, the patients invariably suffered pain and swelling about the knees and ankles during convalescence. There were several cases of the fever and eruption without any local inflammation. It was an adynamic form of disease; there was in every instance a tendency to death by asthenia. Only one case proved fatal. Mrs. E., aged about twenty-eight years, remarkably stout and healthy, awoke on the morning of the 9th of February, with sore throat, and during the afternoon complained of painful deglutition, but continued up and attended to her domestic matters. On the the 10th she was worse, and remained in bed part of the day. At night she became delirious, which alarmed her family, and I was sent for early on the morning of the 11th, and found her sitting in a chair by the fire. She was then rational, and said "that she felt very well, and that there had not been any thing the matter with her except a slight cold." But she had a strange cadaverous look. I insisted on her lying down immediately; she did so, and on examination I found her pulse was 140 beats to the minute, and weak. There was slight swelling of the tonsils, not enough to interfere in the least with respiration, but sufficient to make the act of swallowing painful. There was, however, considerable external swelling about the angles of the jaws and along the course of the sterno-mastoid muscles; her skin was a little below the healthy temperature. She continued about the same through the day, and at night was again delirious. On the 12th there was no alteration, except slight diminution of the external swelling; was again delirious at night.

On the 13th I found her sitting up in bed and insisting that she was better. She had even been walking about the room, but she was evidently sinking; her pulse then beating 150 to the minute, and very weak. I noticed, for the first time, a few patches of the eruption on her breast, and during the day it made its appearance on other parts of her body. There was never any fœtor of her breath nor any indication of gangrene whatever. She seemed to sink from the primary impression of the disease, and died early that night, the 13th.

Again, this year there was a killing frost late in April, and in this month I treated two cases of typhoid pneumonia, both negroes, and both died. I was called early to both cases, and saw at a glance that they differed from simple pneumonia ; in neither was the inflammation of the lungs extensive ; in both it was the lower portion of the left lung that was inflamed and both were expectorating freely, but independent of that the extent of the inflammation was not in itself sufficient to cause any very great perturbation of the system ; yet both cases were *very low* from the time that I first saw them.

Jenny, a negro woman aged about twenty-two years, had had pneumonia in January, and on the 15th of April was taken with pain in the side, and on the 16th I saw her. She had cough, pain in her left side, crepitation in the lower part of her left lung, was expectorating freely, though her sputa was but very slightly tinged with blood, and her pulse was 140 to the minute ; her tongue was almost black and very dry. A blister relieved her pain, but made no alteration in her pulse and was repeated without benefit. She continued to cough and expectorate to the last. A week before death she had severe albuminuria. She died on the 26th.

The other case was a negro man who had not had any previous sickness. In addition to all the symptoms detailed in Jenny's case, he had suppuration of both parotids. The duration of his disease was about the same as hers.

About the first of May there were three cases of typhoid fever reported in the neighborhood. I did not see them, but understood that they had *catarrhal symptoms* at the commencement of the fever. They all died.

During the summer months there was but little fever, but there were frequent cases of acute serous diarrhœa, or rather serous cholera, characterized by frequent and copious discharges, thin as water, perfectly transparent, and about the color of pale urine. An enema of ʒii tr. opii never failed to check these discharges, and no other treatment was required.

During November and December, there was a good deal of pleuropneumonia, mostly among negroes, and in a majority of the cases about

the fourth day of the disease the patients were suddenly and violently attacked with the form of diarrhœa that prevailed during the summer months, and almost immediately upon the onset of diarrhœa all symptoms of inflammation of the lungs would cease, and though the diarrhœa was suddenly checked these cases all terminated in death. It made no difference whether the diarrhœa was suddenly checked, moderately restrained or let alone ; the result was invariably the same. In every instance there was from the beginning a very quick pulse, much quicker than the extent of the local disease was sufficient to account for ; it was often on the first and second days as much as 140 to the minute. There was another remarkable and constant symptom, a rapid wasting of flesh ; three days always made a wonderful alteration in a patient's appearance. From the time that diarrhœa came on, and sometimes sooner, there was profuse perspiration, which continued to the last.

During December, we had flux for the first time in this vicinity. It was of a mild form, and two or three active purges was sufficient to cure the worst cases.

The first three months of 1851 were generally healthy ; there was a large freshet in February, and frost in every month in the year, except the three summer months. In January there was a fatal case of typhoid fever reported in the neighborhood, but I did not see it. From that time till April, there were a few cases of pneumonia, and in the latter month and May together, I treated a very protracted case of *typhoid pneumonia* in a negro boy, about ten years old. He had subacute inflammation of his left lung for about six weeks. At an early stage of his disease he had a singular form of rash, dysentery and sordes ; in fact, his gums grew down over his teeth till they almost hid them ; then became dry and hard at their edges, and were finally sheded. About the third week he took phlebitis and his right leg presented every appearance of phlegmasia dolens. Towards the termination of his disease he had suppuration of the left parotid. He got well.

There was but very little of the ordinary summer sickness this year. In May and June there were six or eight cases of typhoid fever reported to be in one family, and about half the number died. I did not see them; but in July I had within a short distance of the place, and for the first time, a fair, unmixed case of what is meant in this article, by the word typhoid fever. Mrs. J., aged about 30 years, after several days lassitude, was taken on July the 18th with pain in the head and limbs and slight fever, which came and went irregularly, up to the time that I saw her, which was the 25th. She then had very little fever, her pulse did not exceed 80 to the minute, and her skin was soft and moderately pleasant,

but she was *stupid*, not comatose or disposed that way, but her sensibilities seemed blunted; she was indifferent to every thing, and was slow to think and to speak; her countenance was peaked and indicative of great perturbation of her system; her tongue was rather dry, slick, and *very much contracted;* there was a little gurgling in her bowels but no tenderness, and at that time no diarrhœa; her lungs were entirely free from disease; she had been thought to have intermittent fever, for which she had been saturated with quinine. On the 30th, diarrhœa came on and continued irregularly, and with occasional tympanitis, for about two weeks. The duration of her disease was about six weeks. There were three other cases in the family, and all recovered. These cases were in the hills. During September and October there was, in the same neighborhood, an epidemic catarrh, attended in most cases with inflammation of the frontal sinuses, which was intensely painful; generally lasted six or eight days, and terminated in a profuse discharge of bloody, puruloid matter from the nose; relief to the pain was always instantaneous on the eruption of this discharge. There was at the same time, in the flat lands near the river, and mostly among children, an epidemic of what I considered to be inflammation of the petrous portion of the temporal bone. The attacks generally came on at night, commenced with severe pain in the ear, which was followed in twelve or twenty-four hours by swelling around and under the ear, and along the sterno-mastoid muscle. It terminated, in four or five days, in suppuration—sometimes discharging from the ear, sometimes pointing behind it, and sometimes in front.

In November, typhoid pneumonia broke out on the hills in an epidemic form, and several died of it, though the per cent. of mortality was really small, for there were very many cases. I saw in one family, at one time, nine cases, and all taken within forty-eight hours of each other. This was in December, and at that time there was scarcely a family in the neighborhood that had not more or less of its members down with the same disease. To some extent it differed from the typhoid pneumonia of the previous year, was more acute and dynamic and whenever death took place, I believe it resulted from the extent and severity of the local inflammation alone. The attacks generally came on very suddenly, with shivering, pain in the side, and cough, which were followed in from one to twenty-four hours by profuse bloody expectorations, amounting in some cases almost to hæmorrhage. In any of the bad cases, when the patient coughed, the blood could be seen to sprangle from his mouth. The profuseness of the matter expectorated was always in proportion as it commenced early in the disease, and it was always a great natural relief. I distinctly recollect one case, a negro fellow, who, for about thirty-six

hours, breathed seventy-two times to the minute, expectorated enormously, and whose pulse never exceeded eighty-five to the minute. Most of the cases had flatulence and severe colicky pains.

In this epidemic, the tongue differed from anything that I had seen. At the outset it was about as red and rough as a well-burnt brick, and nearly as dry; it gradually became black, and sometimes cracked and pealed off in flakes. Sordes on the teeth was also a common symptom, and some cases had all the complications common in typhoid pneumonia, viz: suppuration of the parotids, dysuria, albuminuria, and conjunctivitis. In this, as in every epidemic where the disease is acute, there were many instances of recovery from very desperate conditions.

[To be Continued.]

ART. IV.—*Critical and Speculative Researches on the fundamental principles of Subjective Science in connection with Medical and Experimental Investigations, with remarks on the present state of Medicine:* By BENNET DOWLER, M. D.

"*There is no lie which many men will not believe; there is no man that does not believe many lies; and there is no man who believes lies only.*"—STERLING.

"*Physical discoverers have differed from barren speculators, not by having* NO *metaphysics in their heads, but by having* GOOD *metaphysics while their adversaries had bad.*"—WHEWELL'S PHILOS. INDUC. SCI.

"IT is," said Goëthe, "with poetry as it is with actions; it is bad when they have to be justified." Perhaps this maxim is not altogether applicable to speculative philosophy, because philosophy, not prose is the antithesis of poetry, and therefore, not amenable to its code. Moreover, among readers there is a class who may not be averse to investigations which lie beyond the narrow circle of mere text-books—investigations which relate to the foundations of science itself including the fundamental laws of intellection. Without pretending in the present paper to enter upon the formal examination of any of these laws, it may not be unprofitable to ramble through and glance at some of their relations not only among themselves, but also to the common platform of knowledge. The poetry of which Goëthe speaks as standing in need of an apology and explanation will get but few readers, because such compositions must meet with immediate appreciation and sympathy, or they must fail altogether in their aims. But this rule applies neither to the propositions re-

lating to the speculative nor to the experimental sciences. These address themselves neither to the imagination nor passions, but to the understanding and reason, and require a sustained and laborious effort in order to determine their import or validity. Abstract reasoning, an element in all investigation, is, after all, the guide to practice. Patient and habitual intellectual exercise is required to achieve any great thoughts, coherency, or conclusiveness. Trusting in the self-flattering theory, that this paper will not be rejected unread as a tarnished dream of metaphysics; that it will be found more or less practical in character, and that the inherent obscurity of some of the topics alluded to, in connection with the necessity of brevity which will not allow of full elucidation, will be borne in mind by candid readers not as justifying a bad but an immethodical ratiocination.

If science advances at a slow pace it is not so much owing to a want of facts as it is to a deficiency in their logical interpretation and induction. Nothing contributes so much towards the accomplishment of this latter desideratum as a knowledge of the instrument of all knowledge, that is the mind, its processes and its laws ; and yet no study is more neglected. It is important to the physician, the lawyer, poet, orator and statesman to investigate the laws of the mental frame and its principles of action, in order to be the better able to influence and direct it in the affairs of life, health and disease. It is remarkable that the mind should apparently be averse to study and know itself and its faculties. An aversion readily seeks an excuse, and exclaims : Methaphysics ! speculation ! transcendentalism ! theory ! Yet, for the most part this is an experimental study as much as any other.

As intermarriages in the same families *(inter se)* tends to mental, physiological, and physical degeneracy, while, a cross with different families and nations has a contrary effect, so the routinist who constantly reasons within the narrow limits of an isolated speciality on the *inter se* in-and-in system, may dwarf the intellect, giving it a circular rather than a forward progression, while on the contrary, a cross in the fundamental principles and methods of the different sciences and professsions may be mutually advantageous. The natural tendency of these first principles is towards unity, the lines of the abstract and the concrete converging to a common centre. There is an affiliation, though not identity, between the transcendental and the experimental, the ideal and the actual.

If the following remarks fail in a practical point of view, the fault is in the writer and not in the topics to be considered, difficult and abstruse as they undoubtedly are. It has been affirmed, though with some exaggeration, that every desideratum is the half of a discovery, and that all

clear ideas are true. However this may be, the reverse will not hold good, as there are many truths which can only be seen "as through a glass darkly," being known only "in part." Clearness of thought and clearness of language are not always conjoined or equal in degree.

Many of the theoretical principles which pervade the encyclopædia of the medical sciences are, to a great extent, transcendental probabilities, that is, they are not necessarily impossible even though they may not yet be verified by experience; or they are mere hypotheses yet within the limits of possibility, awaiting reception, rejection, or modification by the same criterion. Physiology, pathology and therapeutics are never found without provisional assumptions which have often served as points of departure for the working experimentalist, being therefore the precursors of discovery in not a few instances. Thus, if there had been no speculation, no hypotheses advanced concerning the *modus operandi* of medicinal agents, much that is now practical might have remained unknown. Thus, the theory of the primary action of medicine through direct nervous sympathy, consent of parts, etc., has been disproved or at least better explained, experimentally, by the theory of absorption into the circulation, and so of other hypotheses or ideal references and provisional assumptions. Blessed is the man who invents an ingenious hypothesis, for it may end in discovery. Goëthe's ideal of the law of metamorphosis in plants (in one of his poems addressed to a lady) has been applied with great success to the whole realm of animated nature. Oken, and later Owen, have worked well upon the ideal skeleton, and Cuvier's ideal was his guide to his *à posteriori* or practical. Indeed, it may be said of most writers who depreciate speculative science (the sole aim of which is to eliminate practical results with certainty) that they are the very men who abound most in theoretical views, which, however, they take for granted, dignifying their hypotheses by calling them practical.

The longest life is too short to justify the expectation that any individual, whatever may be the extent and power of his intellect, can comprehend, retain, and apply all of the facts and details of the existing sciences. The investigation of scientific methods in general should not be undertaken with the vain hope of excelling in the hierarchy of universal knowledge, but with the laudable purpose of acquiring the general principles necessary to the successful prosecution of a special study and vocation in life:

> "One science only does one genius fit,
> So wide is art, so narrow human wit."

Count Bournon, says Dr. Whewell, published in London, in 1808, three quarto volumes on a single mineral or rather on one of the many

combinations of a single mineral, the carbonate of lime, giving its crystallography, representing its crystals under seven hundred forms, fifty-six of which are essentially different. (Hist. Induc. Sci., m. 232.) Haüy has given about one thousand diagrams of crystalline formations. Thus, a lifetime might be spent in dissecting a single species of the mineral kingdom. Now, the crystallization of a compound, for example, one of the preparations of iron, (*fer sulfuré parallélique*) which, according to Haüy, presents one hundred and thirty-four faces, whether it be the hexagonal or any other form, displays only one aspect of a mineralogical fact or experiment. If a man could in his whole lifetime succeed in mastering all of the crystalline forms of metals, and having done this, should form a system of mineralogy upon the inductive method, his facts being true, his conclusions true, will he have achieved the true science of minerals? Not at all. His researches relate to the externalities or exteriorations of minerals, or to their mathematical crystallography rather than to the physical and chemical constitution of the substances themselves. He learns only the figures on the coin, not its intrinsic value. Isomorphism may show that similarity of crystalline form consists with several or different chemical compositions. Is not the experimentalist who fondly thinks he has reached the *all* of a fact, without any theoretical biases whatever, often quite mistaken? Facts require translation. All could copy the Rosetta stone; none but Champollion could point out how to read and understand it aright.

A materia-medicist might study the physical and chemical characters of the medicines in use, might determine their specific gravity, hardness, form, and combinations, might thence form a system, without thereby illustrating or establishing a single therapeutic principle.

Talent for observation, sagacity in experiment, genius in discovery and coherent, scientific generalization do not necessarily meet in equal degrees in the same individual.

Baconianism is but another name for the now accepted system of experimental philosophy, notwithstanding the claims of Hippocrates, in this behalf, date two thousand years earlier than Bacon's time.

Bacon's theory of experimental philosophy is clearly announced by him, but his attempts to apply this theory are among the most charlatanical, clumsy and imperfect on record.

In the following much admired and most generalized statement of the theory of nature, he says: "Man, the servant and interpreter of nature, understands and reduces to practice just so much as he has actually experienced of nature's laws: more he can neither know nor achieve. [*Homo, naturæ minister et interpres, tantum facit et intelligit quantum de na-*

turæ ordine re vel mente observaverit; nec amplius scit, aut potest." (Nov. Org.)]

His three rules for the "raising of axioms" are very just, and may be briefly expressed: "1. An adequate natural and experimental history is to be procured as the foundation of the whole. 2. Such history must be digested and ranged in proper order—tables and subservient chains of instances are to be found in such a manner, that the understanding may commodiously work upon them. 3. Genuine and real induction must be used as the key of interpretation."

These rules, which apply to the medical as well as the other sciences, he attempted to carry into effect by making and recording experiments, and herein his warmest friends and eulogists admit "that his materials are fanciful in matter, vague in statement, gratuitous in evidence, exhibiting a complete departure from the severity of the inductive method."*

Bacon's practical philosophy was, therefore, no better than his ethical and judicial practice as Lord Chancellor.† He was the greatest teacher in theory, but one of the worst practitioners of it among experimental philosophers, as the sequel may more fully indicate.

The Baconian rules above recited, good as they certainly are, with certain exceptions which will not now be noticed, involve at almost every step theoretical processes, and not a few chances of error. The first rule requires facts or experiments. Now, these may or may not be pertinent, correct, significant and truly classified facts. Facts may be true and yet be valueless from inapplicability or misapplication. Here then, even in the history of facts there is not only speculative judgment required, but danger of erring at the very onset. In the next place, there is the same risk of not classifying and arranging each fact, (particularly such as relate to medicine) according to its natural place or affinity in the group to which it truly belongs, while the risk increases whenever the interpretation of and the induction from the whole are undertaken so as to harmonize with all the parts, and exclude exceptions. Exceptions, so far from proving the general rule, tend only to disprove it. Hence, it is in physiology, pathology and therapeutics, which are eminently experimental sciences, that much, though decreasing uncertainty still prevails. This uncertainty, which recedes before the brightness of scientific discovery, becomes the more perilous when disguised by the imposing designations termed practical, experimental, certain. Experimentalism becomes practical only when reasoned.

**Lib. Use. Knowl.*—An account of Bacon's *Novum Organon Scientiarum.*

†Pope characterized him as

"The greatest, meanest of mankind."

Bacon was born 1561. He died 1626.

Thus it will be seen that even Bacon denounces the received Copernican system of astronomy which is founded on empirical observation, the very method which Bacon adopts; as he maintains that the earth is motionless, and that the threefold motion ascribed to it is not only "a fiction," but "an encumbrance and a serious inconvenience," as if other planets, suns and stars, infinite in number as infinite in space, could not in anywise be incommoded or endangered by carelessness, accidents, explosions or collisions "in solar walk or milkyway," an infinity spinning eternally round one of its minute fragments.

Medical experience would lead to as much certainty as astronomical observation, were the facts of the former as easily comprehended as the facts of the latter. The motives to know how to secure health, to cure disease, and to prolong life are paramount; and were their conditions and laws as accessible to the understanding as those pertaining to the planetary bodies they would doubtlessly be found equally fixed and certain. The approach of a new planet yet unseen is within the reach of astronomical calculation, but no one can ascertain the approach of an epidemic in like manner.

In physic as in physics, the talent for observation and generalization are not (as already observed) always united in the same individual. Flamsteed, astronomer royal, was a practical observer far superior to Newton. The latter, in a letter to Flamsteed, asking for the use of his astronomical tables, says, "I have made no observations myself, as all the world knows." Yet Newton, from Flamsteed's observations, tables, and vast catalogue of stars, solved the cosmical law of matter celestial and terrestrial, and gave the world incomparably the greatest discovery ever made before or since; and yet Flamsteed called Newton's generalizations "crotchets." The one observed. The other reasoned.*

The intuitions of abstract reason, *à priori*, or synthetical judgments are few but forcible. Although the *à posteriori*, analytical method is the only one generally recognized in the experimental school, yet the former, which chiefly relates to the foundations of all knowledge, enlightens the path of the latter, while at the same time, in virtue of its self-luminosity it sees beyond in certain directions.

The intellect which knows itself, its faculties and powers, which determines with accuracy its sensuous relations to outward nature, can the

* They finally became bitter enemies. Flamsteed charged Newton with and prosecuted him for, stealing his astronomical manuscripts. They called each other vile names, "puppy being the least."

From evidence which has transpired chiefly since Sir David Brewster wrote the Life of Newton, it would appear that the latter was, in more than one instance, far from showing himself that faultless monster which this biographer represents him to have been. (See *Nation. Cyc.*, Arts. Flamsteed and Newton.)

more certainly assure itself that the senses teach truly within certain limits; that practical methods and phenomenal laws are freed from illusions, and that impregnable barriers are placed on the outposts of experimentalism, which, as yet, prevents advance to ultimate facts or causes, not to mention the nature or essence of things, in themselves independent of mere properties.

The mind's intuitions or first principles it is forced by its constitution to take for granted without explanation, just as in the intuition that space, which has no cause, antecedent, succession or experimental proof, is necessarily infinite, being a simple intuition excluding every other idea but that of itself—a self-revelation.

Thus, self-knowing, consciousness, intuition and reasoning *à priori* are developed in affiliation with the sensuous or experimental. Yet, neither the understanding nor its inherent self-light are derived solely from without or through the senses.

The great thinker, Descartes, (born 1596, died 1650) who undertook to renounce or regard with scepticism all the vast knowledge he had gained from education and books, so as to investigate *ab initio*, admitted as his starting point but one certain principle, namely, "I think, therefore I am." He then proceeded to reason his way back into the realm of science and out of his voluntary exile in the dreamy spheres of scepticism. But the greatest sceptic may admit, as he is virtually forced to do by the laws of his own constitution, at least one general theory, namely, that reason and the senses are adequate to teach him truly to a greater or lesser extent the laws of mind and matter. In medical inquiries, probabilities are more abundant than certainties. A knowledge of all the elements and conditions of any disease, or any case of sickness, would enable the pathologist to trace and to foretell its invariable course and termination as well as to estimate the influences of a method of treatment. The prognostication of an eclipse or of a death would be equally easy and certain, if the knowledge of the essential laws of each phenomenon were equally comprehended. The uncertainty is in us, not in the events.

In the medical, as in all the sciences, truth ever harmonizes ; one truth contradicts not another. In therapeutics there is in a given case but one method of treatment that is the best. But it by no means follows that our artificial classification of remedies as belonging to different or rather opposite classes is a correct one. Brandy may coincide with the lancet, opium or quinine with purgatives in curative action under the proper conditions.

The beginner in medical science sees with dismay a multitude of facts which he cannot refer to the proper laws without great difficulty, if at all.

As the horizon of his knowledge expands, the number of these laws is diminished ; they are merged or rather converged into simpler and more general principles. Thus as chemistry advances the number of elements diminish. Practical medicine, less fortunate, is still doomed to further laborious explorations in order to determine the validity of some of its principles with greater certainty. Yet, in pursuit of these laudable finalities, it is too often embarrassed and retarded by those of its own household, such as the unreasoning, the routinists, the secret sceptics, and the puling philosophers who would paralize all attempts at rational treatment of disease under the pretended antagonism between medicinal agents and the "*vis medicatrix naturæ*," "*Nature in disease*," "*God in disease*," and other myths. The worshippers of this faith look upon drugs as meddlesome, if not profane interferences, downright polypharmacy and "damnable heresy." If art be supposed to conflict with nature, medecine, like Pharaoh's chariots, must drive "heavily," the wheels of science having been thus taken off. In the art of physic, surgery and obstetrics, as in every other art, nature helps those who judiciously help themselves.

The assumed antagonism between God or Nature in disease and active medication is a refinement in charlatanism, less dangerous it may be, than rash and excessive medication, but less likely at the same time to lead to final improvement, as the history of medical discovery testifies. The active and rash medication of quacks are often so many significant experiments.

It is true that some wise men have called their speculations, "*Nature in disease*," and have justly eulogized Nature's healing powers, but with such qualifying clauses and provisos that their works might just as well have been designated *medicine in disaese or the art of physic against nature, or disease against disease*, for nature is disease as well as health. Toothache, arm presentations, hare-lip, cataract, cholera, fever, etc., are by nature as well as robust men, beautiful women, and symmetrical babies.

The doctrine of final causes, difficult, metaphysical and often unsatisfactory as it is, has been, nevertheless a very useful study. It attempts to unfold the intention, end, or use contemplated by nature. Bacon did not recognize this as a branch of the natural sciences, but difficult as the subject is, it becomes highly useful, conducting to discovery, and in all cases wherein the design of nature can be traced, the mind is filled with delight and satisfaction on recognizing this luminous principle.

Final causes, though little referred to by Kant, are justly characterized by him as the principle of conformableness, intentionality, finality or fitness, the looking to the end : "It consists, in nature, in the representation that all the laws thereof, made known to us by experience, however va-

rious, coincide, for a conception that contains the grounds of their existence, and which is termed the object of nature."

The doctrine of an end or final cause is a very fruitful principle in physiology and pathology, because it is true, and because it is often discoverable, and when discovered, it affords the highest of all intellectual rewards which the investigator can receive, namely, the pleasure of perceiving the adaptation of means to an end, by the All-wise Creator. Hereon is based Natural Theology, of which the human body is, perhaps, the best illustration.

Kant, whose philosophy is almost exclusively subjective, admitted, as already mentioned, the doctrine of final causes in objective science, as in his luminous definition the best ever given of an organized being, namely, "An organized product of nature is that in which all the parts are mutually ends and means." Again, he says: "It is well known that the anatomizers of plants and animals, in order to investigate their structure, and to obtain an insight into the grounds why and to what end such parts, why such a situation and connection of the parts, and exactly such an internal form, come before them, assume, as indispensably necessary, this maxim, that in such a creation nothing is *in vain*, and proceed upon it in the same way in which in general natural philosophy we proceed upon the principle that nothing happens by chance. In fact, they can as little free themselves from this *teleological* principle as from the general physical one; for, as on omitting the latter no experience could be possible, so on omitting the former principle, no clue could exist for the observation of any kind of natural objects which can be considered teleologically under the conception of natural ends." This theory of purpose or finality was the great working principle in much of Cuvier's successful researches into the animal kingdom; what he called the principle of the conditions of existence.

His cotemporary, Geoffroy St. Hilaire, however, refused to attribute the principle of intentionality to Nature, yet he was the great apostle and expounder of uniformity of plan and type in all organic beings, including those denominated monstrosities. In the latter he traced connection, equilibrium, analogy, affinity, harmony, uniformity. This is enough!

Cuvier, in his Animal Kingdom, terms final causes the principles of the conditions of existence and maintains with astonishing success, both theoretically and practically, that "the analysis of these conditions often leads to general laws, as clearly demonstrated as those which result from calculation or from experience. He went on the supposition not only that animal forms have *some* plan, *some* purpose, but that they have an intelligible plan, a discoverable purpose."

That every effect must have a cause, is not so much an experimental as it is a transcendental truth. Experience in this, as in the infinite divisibility of matter is not quite competent to teach this generalization or intuition. It may appeal to analogy, which is not demonstrative. It may rely on empirical observation which generally, not always, detects causes or rather antecedents; but these antecedents can be traced in the ascending series only a short distance. The best ætiological explanation quickly stops at what are hypothetically called ultimate facts, but which, with superior intelligence, would doubtlessly appear as effects of anterior or remoter causes. But for the most part the simplest facts in medical ætiology, as the causes of epidemics, etc., remain in impenetrable obscurity. Indeed, the laws, not the causes of epidemics, and of many diseases, are the only proper objects of investigation. Newton knew the laws, not the cause of gravitation. He inclined to the opinion that the cause was nothing else than the continuous and immediate action of the Divine Will.

Experience would, indeed, virtually go far towards showing that many events have no causes whatever, because none are known to its methods; nay, it might be said that antecedent phenomena are wanting so far as human observation and research can go. If experience teaches that evident causes exist in some cases, it also teaches equally that none are known to exist in other cases, that is, none can be clearly recognized. The very nature of experience excludes that which it cannot know experimentally. It ill becomes the experimental philosophy to assume as facts such phenomena or causes that cannot be established by its own method of proof.

Strictly speaking, no one can prove by experience (to take the strongest analogy it allows), that he himself will die; yet analogy in this particular case will afford proof that two hundred years will not have elapsed before this event will have occurred, notwithstanding the abundant experimental proof in the sacred writings that many have lived in former times four or five times longer. Here is experiment against experiment.

Apart from empirical observation neither anatomy nor physiology indicates that life might not be prolonged indefinitely. If there be an invincible chain of causation limiting the duration of life on an average to the third of a century, the experimental philosophy knows it not. Vegetable life is apparently immortal. Trees live an indefinite period defying old age, perishing only by accidents, such as lightning, fire, insects, and storms.

Decapitation, evisceration, cancer, consumption, and the like, cause death, as experience teaches; and in such cases the cause of death is

supposed to be explained. But experience teaches also, that a person may, though rarely, drop down dead without any appreciable alteration being discoverable by the most careful examination. Analogy would seem to teach that a lesion of the most aggravated kind must have caused the fatal effect in the latter as well as in the former cases. But the experimental method not only fails to show that the cause was proportioned to the effect, but it fails to show that any cause whatever existed. The violent obvious effect ought to have an equally violent obvious cause—the one being the measure of the other. And here a transcendental reference is resorted to for explanation.

A wound, a boil, an aneurism, a cancer, a perforating ulcer in the intestine or air tube is a materialistic, concrete alteration. But that which transcends the means of experimental research is referred to unseen or to dynamical or functional change, and the man who insists on experimental proof for everything will admit, nevertheless, the infinite divisibility of matter, dynamic, alterations, etc., which are wholly transcendental. Something is an experimental, nothing or negation is a transcendental idea.

This line of argumentation concerning the conditioned and the unconditioned, the attainable and the unattainable in experimentalism, is not altogether a metaphysical refinement, since teachers and authors dwell upon ætiology or causes as if little difficulty existed in this behalf, and now professional persons particularly require and expect, and employ a physician to explain almost everything concerning the cause and cure of diseases, etc.

But then, O how consolatory! almost any kind of explanation will suffice to satisfy them. It is often well to take the advice in the Dunciad, and not

> "Explain upon a thing, till all men doubt it,
> And write about it, Goddess, and about it."

Nevertheless, it is best for the philosopher to know that he don't know, that is, to know his ignorance in this behalf.

The most enlightened experimentalism rarely teaches more than a constant antecedence and sequence, and where it shows adaptations to ends, these are chiefly the teachings of analogy and not known to be the necessary and only possible means by which the same ends can be achieved.

It is remarkable that the only kind of knowledge which is absolutely, necessarily, and universally true, though not generally recognized as such, is wholly transcendental, not lying within, but above the experimental philosophy.

It is not pretended that in the present state of existence, the understanding can, *per se*, be developed otherwise than in connection with the body and the senses, so as to arrive at necessary intuitions, wholly independent of the sensuous school which affords the necessary conditions, means and occasions of mental growth. But the question is, may not the understanding thus progressively matured, be in its own inherent right able to discover truths and realize intuitions not only beyond, but seemingly antithetical to, the evidences derived through the senses? The argument does not rest on the *tabula rasa* of the new-born baby, nor on the disembodied spirits of the immortals, but upon things as they are or may be in their progression towards perfection. The mind of the new-born child may be a *tabula rasa* or not without affecting the argument. The savage is virtually in possession of the intuitions of the infinity of space and of duration because the opposite are not very conceivable, though he may comprehend neither decimal fractions nor chemical affinity, nor the laws of gravitation nor the innate faculties. The question is not how knowledge, any more than vitality, begins, but how far may it extend or where may it end?

Kant, the greatest apostle of this philosophy (which has been much ridiculed by sciolists)—a philosophy which he has built up with a massive logic that defies the empiricism of Bacon, maintains "that we are always in possession of certain cognitions, *à priori*, and even the common state of mankind is never without such. We can distinguish pure from empirical cognition. Experience teaches that something is constituted in such and such a manner, but not, that it could not be otherwise. If in the *first* place, therefore, a proposition is met with, which is thought of at the same time with its necessity, it is a judgment *à priori*, and if, besides this, it is not deduced from any other, and as itself, again is valid, as a necessary proposition, it is that, absolutely, *à priori*. *In the second place*, experience never gives to its judgments certain and strict universality, but only admitted and comparative, (by induction); so that properly speaking, it must be said—so far as we have hitherto perceived, there is no exception to this or that rule. If a judgment is, therefore, thought in strict universality, that is, so that not any exception is allowed as possible, this is not derivable from experience, but is absolutely valid, *à priori*. Empirical universality is, therefore, only an arbitrary progression of validity from that which is valid in most cases, to that which is so in all, as, for example, in the proposition, all bodies are heavy. Where, on the other hand, strict universality belongs essentially to a judgment, that indicates a particular source of its cognition, namely, a faculty of cognition, *à priori*. Necessity and strict universality are, therefore, sure

characteristics of a cognition *à priori*, and belong, also inseparably to each other— * * As all change must have a cause. The conception of a cause so ostensibly involves that of a necessity of connection with an effect, and of a strict universality of the rule that the conception of a cause would be entirely lost, if, as Hume did, we wished to derive it from a frequent association of what happens, with that which precedes it, and from a habit thence originating, (consequently from merely subjective necessity) of connecting representations. And without requiring such examples as to the proof of the reality of pure principles *à priori* in our cognition, we might demonstrate its indispensableness for the possibility of experience itself, consequently *à priori*. For whence would experience deduce its certainty, when all the rules according to which it proceeds were again always empirical, consequently contingent, and when therefore we could hardly look upon them as valid first principles.

"Philosophy stands in need of a science which determines the possibility, the principles, and the extent of all cognitions *à priori*. When we are out of the circle of experience, we are thus sure not to be opposed by experience. Natural Philosophy (physica) contains synthetical judgments, *à priori*, in itself. Thus, in all changes of the corporeal world, the quantity of matter remains unchanged; in all communication of motion, action and reäction always must be equal to each other. In both propositions, not only is the necessity, consequently their origin, *à priori*, clear, but also that they are synthetical propositions.

"To the critic of pure reason, every thing belongs which constitutes transcendental philosophy, and it is the complete idea of the transcendental philosophy, but still not the science itself, since it only goes as far in analysis as is requisite for the complete examination of synthetical cognition *à priori*. Transcendental philosophy is hence a science of mere pure speculative reason.

"I call the science of all the principles of sensibility *à priori*, transcendental *Æsthetick*. [The Germans use this term to denote the critic of taste.]

"Space is no empirical conception, being a necessary representation *à priori*, lying at the foundation of all external intuitions—a pure intuition—an infinite given quantity—the original representation of space, is intuition *à priori*.

"Space represents no property at all of things in themselves—is the form of all phenomena of the external senses—that is, the subjective condition of sensibility, (receptivity) under which alone external intuition is possible to us.

"Our exposition teaches the reality (that is the objective validity) of

space, in reference to all that externally as object can be presented to us, but at the same time the ideality of space, in reference to things if they are considered in themselves by means of reason—that is, without regard to the nature of our sensibility."

The non-reception of Kant's doctrine beyond the limits of Germany, is not a rational objection to its logical claims. The author of the *Principia* was long ignored beyond the limits of England. Sir David Brewster, in his Life of Newton, adopts Voltaire's statement, namely, that although Newton survived the publication of the *Principia* more than forty years, yet, at the time of his death, he had not above *twenty* followers out of England.

These extracts from *The Critique of Pure Reason*, by KANT, are given not only for their intrinsic value, but with a hope of calling the attention of medical students to the most luminous work on the more elevated points and positions of subjective science. The Kantian philosophy is almost wholly subjective, the Baconian entirely objective. The former does not oppose but advocates the latter, but it nevertheless claims and achieves what the latter rejects as impossible, because it is not objective or demonstrable by the experimental method.

An exclusively objective philosophy which allows to the understanding nothing but what was first in the senses, is an error in psychology which excludes those truths which are technically termed necessary or universal, and, which, more than any other natural endowment elevate the human intellect towards the All-knowing Being.

But passing from this digression, it may be affirmed that whatsoever there may be in medical ætiology and pathology which is not phenomenal, concrete, and representable, cannot, of course, be investigated experimentally in outward nature.

At the moment of exposure to the contagion of small-pox, there is doubtless, a change in the subject; yet, the change then effected, and all those ulterior changes which occur from such exposure up the æra of phenomenal development, are unknown to the subject and to the medical observer.

The laws of physiology and pathology are, perhaps, as already indicated, as exact, as necessarily true as those of mathematics; that is to say, a complete knowledge of these sciences; of their facts, conditions, and relations of their entire phenomena, would enable the physician not less than the mathematician, to recognize in these sciences necessary truths, instead of probabilities. Yet, such is the imperfection of our senses, of our means of exploration, that no one has attained this absolute knowledge; it may be that such knowledge is unattainable; yet, we

know enough to enable us to conclude that nothing is fortuitous ; that death or a cure in yellow fever, is a necessary result of laws as fixed as those governing the solar system. That blood-letting, calomel, or quinine will always produce a definite result under the same circumstances, and that a different effect would be a self-evident impossibility or contradiction, is as true a proposition as that a part is less than the whole. The chance, the uncertainty, the contingency is not in the science of nature, but in the science of the observer; in the limited range and in the imperfect action of his senses, by which it happens that many essential data in the plainest cases are unobserved, or elude the most diligent research.

Even the astronomer returns from time to time upon his empirical observations on the sidereal heavens, in order to inspect and correct errors incidental to his senses and experimental processes. In this way, and by multiplying his observations, the extent of possible error is limited and virtually neutralized. But unfortunately for the medical investigator, the range of possible error in his field of observation is vastly greater, how much so ever he may multiply his facts.

What is a fact, a medical fact *per se?* Virtually, it exists not in its scientific significance to the casual looker-on any more than to the idiot, until it is made an abstraction in the understanding as to its relations, teachings, and uses. Its certitude and its meaning may be mistaken, and the more so if it be of a complex character.

The chief difficulty in physiological and therapeutic experimentation lies in the inherent complex and multiform influences and conditions not fully appreciable by the observer, to which must be added the artificial influences and conditions inherent in the experiment itself. The experiment may be incompatible instead of coïnciding with the natural function sought. A vivisection may itself suspend the function or kill the animal. The drug may overpower the disease and take its place for good or evil.

Although the progress of real discovery generally tends to *solidarité* and reduces the number of theoretical principles, there is little reason to hope that the medical sciences can ever be resolved into one law of universal application so as to form all into a homogeneous unity. The apparent inexactness, instability, and uncertainty of organic science and its vital phenomena are not inherent, but incidental to our imperfect faculties and limited modes of cognizing them. Astronomy, chemistry, physics, and physiology are not the less true because savages or idiots do not understand them.

In medicine, both theoretical and practical, the physician grows weary with doubts, contradictions, investigations, experiments, and rigid logic. He wants a principle, a theory, a belief. Must he go on observing and

experimenting? must he evermore grind logic in his "dome of thought," regardless of dyspepsia, sleeplessness, and the brevity of life? Is he bound to do his own thinking and make his own positive philosophy any more than his hat or watch? Can he not accept the authority of Hippocrates, or some of his successors? as of Silvius de la Boe, who held that fever arises from acidity, and is cured by an alcalis? of Stahl, who says that disease is a bad state of the fluids attended with plethora, and is cured by evacuants? of Hoffmann's excessive or diminished tone of the fibres? of Boërhaave's weakness and rigidity of fibre, acidity, alcalinity, plethora, congestion? of Cullen's "spasm of the extreme vessels?" of Brown's sthenia and asthenia? of Broussais' gastro-enteritis?

> Say ye, who search the records of the dead—
> Who read huge works, to boast what ye have read;
> Can all the real knowledge ye possess,
> Or those—if such there are—who more than guess,
> Atone for each impostor's wild mistakes,
> And mend the blunders pride or folly makes?
> What thought so wild, what airy dream so light,
> That will not prompt a theorist to write?
> What art so prevalent, what proof so strong,
> That will convince him his attempt is wrong?
> One in the solids finds each lurking ill,
> Nor grants the passive fluids power to kill;
> A learned friend some subtler reason brings,
> Absolves the channels, but condemns their springs;
> The subtle nerves that shun the doctor's eye,
> Escape no more his subtler theory;
> Bilious by some, by others nervous seen,
> Rage the fantastic demons of the spleen;
> And every symptom of the strange disease
> With every system of the sage agrees.—CRABBE.* (Works, iii, 8.)

All the writings, thinkings, and theorizings of others, without patient personal observation and reasoning must fail at the bedside. Nature must be directly consulted. Yet to pull down without building up theories, may seem at first glance not only revolting in an æsthetical point of view, but it imposes much labor upon the practitioner, who is obliged to observe, and to reason for himself, and to compare, to estimate, to verify, and to apply the experimental labors and principles of others so far as they can elucidate special cases and maladies. To overturn error is not only a duty but a necessary step preparatory to the establishment of the truth, let that come soon or late. Error is legion, truth one. But it happens, as Goëthe says, "that each commonly defends himself as long as possible from casting out the idols which he worships in his soul, from acknowledging a master error, and admitting any truth which brings him to despair."

* Byron designated Crabbe as "Nature's sternest Poet, yet the best,"—a line placed on his tomb.

Medicine, as has been already said, does not generally advance by the multiplication of its principles but by their diminution and resolution into fewer and more general ones, the fewest possible which experimentalism and logic warrant; its evolutions do not generally coïncide with its point of departure, but terminate beyond its last generalization; it leaves behind the first principles and approximates, its desiderated finality, that is universality. It seeks to appreciate the parts in the whole and the whole in each part. Terrestrial and celestial statics and dynamics were erroneously expounded before Newton's discovery of the law of universal gravitation. Physiology, pathology, and therapy, all need similar fundamental principles, which, if attainable at all, must be sought for with zeal, courage and industry.

Having glanced over some of the regions of speculative medicine, it is proposed to descend from aërial navigation to the field of practice, where it will be found that improvements of the most encouraging character have been made in our day and generation, a few of which will be alluded to briefly, not forgetting certain retrogressive movements which would disgrace the dark ages of sorcery, witchcraft, and superstition.

Pathological anatomists and chemists have, of late, directed the medical mind to humoralism or the doctrine of the fluids, and have thereby contributed to the building up of science and to the downfall of Broussaisism, which was based on the solids, and that too almost exclusively on the gastro-intestinal mucous membrane. Changes in the blood, bile, urine, saliva, etc., are now regarded as fundamental elements in pathological science, not less important than that assemblage of alterations which constitute the anatomical characters of hyperæmia or inflammation of the mucous membrane of the intestinal canal, etc.

Each element of the blood not to name other fluids, becomes an object of special research, as an excess or a diminution of fibrin ; an excess or a deficiency of serosity, or of saline matter ; an excess of carbon, etc.

The red globules or corpuscles of the blood being regarded as living cells, minute organisms, and entities, are proved or assumed to be liable to a multitude of changes, as an excess or deficiency of red or white corpuscles. Anæmia, hyperæmia, toxæmia or blood poisoning, necræmia or death of the blood, and other conditions have been recognized.

Fortunately, a profound knowledge of analytical chemistry is not in every instance essential to enable the practitioner to judge of the general import deducible from the labored minutiæ of the pathological chemistry.

Thus either an excess of hydrocabon in the blood as in asphyxia, or apnœa in cardiac diseases and abnormities, or in cholera, etc.; or an excess of water in the blood as in certain cacheetic diseases, as each declares

itself to the eye in the physical appearance of the skin, and other tissues, as also in the consistence of the blood clot, etc. In albuminuria, etc., a boiling heat becomes an easy test, and so of some other simple processes of more or less value.

Bichât, in his *Anatomie Générale* (1800) asserted that humoralistic or morbid alteration of the fluids *(vice des humeurs)* was the real foundation for many cases of disease.

> ——"The life of all his blood
> Is touched corruptibly."—SHAKESPEARE.

The progress of Natural History with comparative anatomy during the present century, its methods and its classificatory system, have impressed themselves upon the affiliated and to a great extent identical departments of Medical science. The vast aggregation of organized beings amounting to several thousand species of the animal kingdom, viewed separately without any classificatory system, bewilders the understanding and overpowers the memory ; but when these are arranged according to anatomical and physiological identity, affinity, analogy, and finality, *four fundamental types* are found sufficient as criteria to classify all into as many sub-kingdoms as the vertebrata, articulata, etc., and the same principle of typical structure of organization musters all genera and species into the ranks, whether they pertain to the fossil or living Fauna, the remote palæözoïc or the current stream of time. There is, indeed, a luminous parallelism between the natural and medical sciences.

Organic chemistry, particularly in its physiological, pathological, and therapeutical types, characters and uses, has been, within a recent period, enriched and consolidated by the researches of Berzelius, Liebig, Mulder, Dumas, Lowïg, and many others, whose works in various languages are still more recent.

The essays of Prout, Andral, Gavarret, Becquerel, and others, were precursory to, or cotemporaneous with the revival of humoralism, and the contribution of numerous and far more complete works in which chemical and microscopic researches have illustrated the humoral pathology. The works of Simon, Vogel, Kölliker, Lehmann, Rokitansky, now form a part of every good library. Cell anatomy, cell physiology, cell pathology, cell therapeutics rest on experimental bases, more or less satisfactory.

Solidism is no longer regarded as the exclusive foundation of pathological science. Broussais' dogma, that all of the alterations of the fluids originate in inflammation of the solids, is no longer predominant. On the other hand, the ancient doctrine of humoralism was formerly not only exaggerated, but was vague and ill-defined and unproved. Modern re-

search into organic chemistry has given an approximate certainty and limit in this behalf.

The progress of medicine tends to the overthrow of badly founded theories, which are now replaced, not by any exclusive system of universal application, but by such general principles as have been tested by an unexampled amount of experimental research, whereby a golden mean as to diet, stimulants, medication, and the like has been established. Semi-starvation, syncopal blood-lettings, drastic purgatives, violent emetics, and enormous doses have, in a great degree, ceased, or have been held in reserve for extraordinary cases.

Hygienic prevention and sanitarian improvements occupy the first, and drugs the second place in modern therapy.

Physical diagnosis created by Laënnec and his successors, has given to modern pathology an illumination unknown to the past.

In general anatomy or histology, in comparative physiological, microscopic, and pathological anatomy, in medical chemistry and pharmacy, in Hygiene, in clinical and surgical practice, great discoveries or improvements have been made by the present generation. Among improved methods of investigation, the numerical method is not the least useful. It has advanced its sentinels to the outposts, to the frontiers of the unknown. Former hypotheses have been, to some extent, reduced to certainties, or disproved by vast accumulations of data whose antecedents were little or wholly unknown, and whose immediate effects or phenomena when submitted to numerical analysis and reasoning, often afford probable and even satisfactory certainty where doubt had prevailed. Numerical reasoning has done great service in vital statistics, in clinical medicine, in practical questions relating to operative surgery, obstetrics, hygiene, climate, life insurance, and many special *desiderata* to which the theory of probabilities will apply, and by which the illusions of pure chance are dispelled. It assumes that events have a definite order, the same antecedents producing the same effects; that a large mass of facts collected under similar phenomenal conditions, though individually variable, will afford on numerical analysis, similar results or averages, notwithstanding their causation or their remote antecedents may elude research. Thus the average duration of life may be ascertained. Although the *numerical* data are wanting to show with exactitude the mean duration of life two centuries ago, yet sufficient is known to prove that its duration, at present, is longer than formerly, owing, doubtlessly, to increased knowledge of the sanitary, medical, and physical means which tend to that result. Nor is it possible to say that the average extension

of human life may not be hereafter much greater than the present, should ameliorations continue to characterize the future circumstances of humanity. Small-pox, before Jenner's time, brought to an untimely grave about one-thirteenth of the world's population. The future may be still more auspicious, and the numerists upon its watch-towers will be the first and best authorities for its vital statistics. Thus numerism eliminates truths with an almost mathematical certainty, from those very facts which are commonly though erroneously considered as contingent or fortuitous.

It was intended to give, in this place, a sketch of the progress of the medical sciences from Bichât's time, (the commencement of this century) when his *Anatomie Générale* appeared as the morning star of a new day; but this article it already too long, and will be closed with a glimpse at two different tableaux, peculiarly American.

The fair face of science is scarred, deformed and blackened at the present time by more numerous and extensive quackeries than were ever before witnessed in all the abberrations of medical humanity.

Passing by homœopathy, hydropathy, and some other systems; the limits of this article will allow of but a slight glance at two charlatanries, namely, spiritualistic and Thompsonian medicine.

A few months ago, one number among the many of the journals devoted to modern spiritualism, was sent to the office of the *New Orleans Medical and Surgical Journal.* This spiritual sheet, beautifully printed at Boston, called the *Banner of Light,* contained the subjoined significant advertisements, which will be reproduced without comment, not only to show Southern readers how spiritualists practise the healing art in the North, but also to record for the information of remote posterity, these precious documents, illustrative of the therapeutics of the year 1857, should the *New Orleans Medical Journal* happen by any unexpected chance to float down the stream of time a few centuries. Who would not have thanked Hippocrates, Galen, Celsus, or other ancient writer for advertisements illustrative of the quackeries of their times? Centaurs, Egyptian magicians, and the mysteries of the Asclepiades of the remote past, "pale their ineffectual fires" before modern table-turners, clairvoyants, and trance doctors, *anno* 1857.

Before proceeding to the advertisements in one number of the *Banner of Light,* it may be proper to mention that this sheet, with many others, claims that the table-turning spiritualism which it advocates "is believed in by *three millions* of people; that the cause is God's cause; that we have the testimony of tens, yea, hundreds of thousands in evidence of the presence of those whom we have been taught were 'dead and know not anything.' We've these dead here with us now; they live, speak, clasp

our hands in their own, and tell us of their glorious entrance into a beautiful world of life and joy."

"As our circle is open to all spirits, friends in distant cities may make arrangements with their spirit friends to commune with them through our columns if they wish, and thus add a strong test to their faith in spiritual intercourse.

"Spiritualism, rightly interpreted, is the Great Word, or Truth, that was in the beginning with God, was with God, and indeed was God."

"Advertisements not exceeding twelve lines, ten dollars quarterly."

The *Banner* gives notices, "with a motive," perhaps, of various meetings, with day and hour for "speaking by entranced mediums"—"trance meetings." It abounds with individual advertisements, of which some of the briefest are the following, which are sometimes abridged, as to street, number, date, hours, fees, etc. They show the medical use of spiritual therapeutics in the North, which the *Southern mind* is either to weak or too wise to adopt in clinical medicine.

"Mrs. J. H. Conant, trance medium for medical examinations ONLY. Examinations $1 at her rooms, or at the residence of the patient."

"Mrs. W. R. Hayden, Rapping, Writing, Imprinting (letters on the arm) and Clairsympathetic Medium."

"Dr. W. R. Hayden, Physician and Medical Mesmerist."

"An Asylum for the afflicted, healing by the laying on of hands; moderate terms. Those sending locks of hair to indicate their diseases should inclose one dollar for the examination, with a letter stamp to prepay the postage. Charles Main, Healing Medium."

"Miss M. Munson, Clairvoyant."

"Mrs. R. H. Burt, Writing, Speaking, Trance and Personating Medium."

"James W. Greenwood, Healing Medium. A. C. Styles, M. D., Independent Clairvoyant; exam. and pres. $3; by lock of hair, if the most prominent symptoms are given, $2; if not given, $3, etc. J. V. Mansfield, Medium for answering sealed letters, $1.

"T. H. Peabody, Healing Medium, cures all diseases, assisted by Mrs. P."

"Mrs. T. H. Peabody, Trance Medium."

"Medical Institute having no sympathy with the legalized Medical Institution made up of a combination of speculating individuals, having no higher object than money making, etc., I have come to the conclusion that I may, as well as some other individuals, establish myself in an institution alone with my wife and boy to constitute the whole faculty, having cured more of the thousands of cases than any other physician; office connected with a store of Eclectic, Botanic, Thompsonian and Patent Medicines. N. H. Dillingham, M. D."

"Mrs. E. B. Danforth, Clarosympathetic; cleanses the blood, gives circulation, vitalizes, etc., $1 25; $1 50; $2 00.

"George Atkins, Healing and Clairvoyant Medium. By enclosing a lock of hair, the patient will receive an examination written out, with all requisite instructions. Present $1; absent, $3, payable in advance."

Homœopathy, Mesmerism, claro-sympathy, table-turningism, tranceism, and the like, supremely absurd and conflicting in and among themselves, must fail in their attempts to overthrow or long impede the march of medical science. But a different charlatanry, formidable for numbers, zealous and united in purpose, marching under a pseudo-flag of science, everywhere in this Republic, opposes its treasonable forces against legitimate medicine. Thompsonianism, now disguised under the names of Botanics, Physio-Medicalists, Eclectics, and Reformers, all unite on a common platform against the "Old School," that is to say, against the school of Hippocrates, Bacon, Newton, Harvey, Bichât, Cuvier, Rush, Franklin, not to name other experimental interpreters of the laws of Nature.

Neither the Prophet, nor his twenty dollar patent Koran (a very thin book) are now much referred to by his ungrateful successors. Some of the latter ignore both, affecting learning, journalism, and colleges; they say nothing of selling patent rights to practise in a special district nor of the oath of secrecy, but grant, in vast numbers, the highest medical degrees known in the profession.

It was proved on the trial of their founder, Samuel Thompson, for the wilful murder of Ezra Lovett, that the medicines which the accused gave to his patients were, by him, called "bull-dog, ram-cat screw auger, belly-my-seize," etc. In one of his illiterate publications, Thompson says, "It is as impossible that one of the learned professions can be a republican, as it is for ice to produce heat; they are as complete an aristocracy as exists in any part of the world." He terms the regular doctors "Anti-Christ," "The Beast," etc., and says, "I have the most certain evidence to believe that the time is coming and now is, when God will utterly supplant, root out and destroy from the face of the earth, all Doctorcraft, Priestcraft, and Lawyercraft." Wholly illiterate himself, he despised learning, particularly in a physician. Grammar was a general subject of ridicule with him and his followers. His fundamental dogmas, as announced in his book or patent right, are the following:

"Fever is a friend. No one ever died of a fever. Cold is the cause of disease. Cold is death. Nature is heat. The stomach and bowels are affected with canker in all cases of disease. Saltpetre is the worst of any poison. If a medicine be agreeable in one case, it must be absolutely so in all. All fevers, etc., can be cured in twenty-four or forty-eight hours to the extent; often sooner. All constitutions are alike. In all cases, I give No. 1, to raise as great an internal heat as I can." (Pp. 12, 11, 32, 8, 28, 14, 15, 10, 31.) He cured a woman of dropsy by boiling down all the water in her belly. Steam, lobelia, alcohol, and red pepper were his chief remedies.

Such was the founder, the Hippocrates, the father of the mordern medical sects or self-called Reformers.

Religious enthusiasm was too often prostituted to their cause in this crusade against science. The very men who read in the Holy Scriptures that the SAVIOUR cured fevers, commanding them to depart from the sick, preached the fundamental doctrine of Thompson, that "fever is a *friend*." They read or might have read that the learned Apostle Paul considered fever as an *enemy:* "And it came to pass, that the father of Publius lay sick of a fever, and of a bloody flux, to whom Paul entered in, etc., and healed him; others also, which had diseases in the island, were healed," etc. (Acts, xxviii, 8, 9.)

Let not the self-styled Reformers "forget the pit whence they were digged." They, as did their prototype, condemn as poisonous, all medicines prepared from metals or minerals, which from their pondorosity or other quality, weighed down the patient to the grave, while the up-growing vegetable or herb medicines operated in the contrary direction, causing the patient to grow upward or get better! Nevertheless, the most violent poisons are the vegetable.

The Eclectics *(of the United States)* claim for themselves, the exclusive ability, the paramount prerogative, the superior qualification for selecting and proclaiming the infallible truth, and damning the regularly educated physicians of the "old school" of legitimate medicine. These Eclectics must not be confounded with the ancient philosophers, Archigenes and his cotemporaries, who, instead of dogmatizing, professed to select from other philosophical systems, whatsoever appeared most conformable to reason and truth, which is virtually professed by all, but is practised successfully only by men of superior education, judgment and ability. There is not a fetish negro medicine-man in Congo, nor a juggling Indian medicine-man in America, who does not assume this pretended superiority of choosing or selecting the infallible truth, thereby claiming to be an Eclectic. To choose is easy, but to choose rightly is not so easy as sciolists imagine.

Let the self-styled reformers illumine the Temple of Medicine by the light of a truer philosophy, by a superior knowledge of the medical, physical, chemical, natural history, and collateral sciences; let them acquire these to a greater extent than can be found in the existing "old school," and then their consolations derived from the number of their adherents among the people, and from the legislators who charter Thompsonian-Steam-Botanic-Physio-Eclectic Colleges, will have been founded on a conscientious, useful, and real reformation, thereby inaugurating an epoch in medical history. As yet, this unscientific aggregation of practitioners

have produced no truly original work, nor any discovery of importance. Indigenous medical botany, their favorite field of research, is a rich one, in which they have gained something to the cause of medicine from their extensive experimentations; nevertheless, the fundamental dogma of Thompson, that every district of country contains the herbs necessary to cure its diseases, is as unfounded as is his utter rejection of minerals as poisonous. Dr. Dunglison, in his dictionary, defines Thompsonianism by this latter dogma, which he justly calls a "fanciful doctrine, that metals and minerals are in the earth, and, being extracted from the depths of the earth, have a tendency to carry all down into the earth who use them, while the tendency of all vegetables is to spring up from the earth, and therefore uphold mankind from the grave."

These views find a parallelism in those enumerated but severely criticised by Theophrastus, the first scientific botanist among the ancients, who says in his work on Plants, written nearly three centuries before the Christian æra, that the quacks and drug-sellers of his day, "direct us to gather some plants standing from the wind, and with our bodies anointed; some by night, some by day. They go further. They draw a sword three times round the madgragora and cut looking to the west, and dance round it; use obscene language as those who sow cumin should utter blasphemies; draw a line round black hellebore, standing to the east, praying to avoid an eagle either to the right or the left, for they say, 'if an eagle be near, the cutter will die in a year.'"

Foreigners wonder what Thompsonianism in America is. They cannot get the fundamental idea of its true character from the medical dictionaries of Dunglison & Harris; Dr. Hays' account in Hoblyn's dictionary is a trifle better, though it fails in the most fundamental parts, while the Eclectic (Thompsonian?) Prof. Cleaveland, M. D., of Cincinnati, in his Medical Lexicon, ignores it altogether. Hence it may not be improper to add a few words additional to what has been already given, in order that the rising generation may the more fully know the enemy they must encounter from this, the greatest, most dangerous, and the most persistent schism (now somewhat cunningly disguised) which true medical science has suffered in America, and which had its origin in this wise: Thompson says that when he "was 20 years old, his advantages of education having been small, he gave an herb, through sport, to a man who was mowing grass with him. The man chewed a sprig of it, and recommenced his labor, but soon stopped and said, 'you have poisoned me.' He sweat, staggered, imagined he was dying, trembled, had no more color of life than a corpse, threw himself on the ground; but we helped him to a neighboring spring and gave him water to drink, when he vomi-

ted most profusely. It was this," says T., "which gave me the first strong impression of the value of the emetic weed" (lobelia.) He quit mowing, with a motive, and from this limited stock of experimental medical philosophy, commenced the practice of medicine under a patent from the National Government, and soon formed the only completely united medical organization on large scale (including a million of adherents) ever formed in the United States, not excepting the American Medical Association.

Soon after the above-mentioned "sport" in the meadow, Thompson quit mowing grass, but was prosecuted for mowing down men, although for want of evidence of malice prepense, he was frequently acquitted with little difficulty. In the case of the Commonwealth of Massachusetts against Samuel Thompson for the wilful murder of Ezra Lovett (6th vol. Tyng's Reports) "it appears that Thompson came into Beverly where Lovett lived, announced himself as a physician, and professed an ability to cure all fevers whether black, gray, green or yellow. He (T.) came and ordered a large fire to be kindled to heat the room. He then placed the feet of the deceased with his shoes off on a stove of coals, and wrapped him in a thick blanket, covering his head." "The Chief Justice Parsons, in his charge to the jury in this case, observed, that the deceased lost his life by the unskilful treatment of the prisoner (Thompson) did not seem to admit of any reasonable doubt. Before the Monday evening preceding the death of Lovett, he had, by profuse sweats, and by often repeated doses of the emetic powder (lobelia) been reduced very low. In this state, on that evening, other doses of the Indian tobacco were administered. When the second portion did not operate, probably because the tone of the stomach was destroyed, the repetition of them, that they might operate as a cathartic, was followed by convulsion, fits, loss of reason, and death. It is exceedingly to be lamented that people are so easily persuaded to put confidence in these itinerant quacks, and to trust their lives to strangers without knowledge or experience. If this astonishing infatuation should continue, and men are found to yield to the impudent pretensions of ignorant empiricism, there seems to be no adequate remedy by a criminal prosecution."

In his *Guide to Health*, Samuel Thompson gives the following as the form of the contract between himself and the purchasers of his twenty dollar patent to cure all diseases with one remedy: "The subscriber and purchasers agree in the spirit of mutual interest and honor not to reveal any part of said information to any person except those who purchase the Right, to the injury of the proprietor, under the penalty of forfeiting their word and honor, and all right to the use of the medicine.

S. T."

The third article of the Thompsonian Botanic Society reads thus: "No member of said society practising out of his own family, shall let blood as is common with physicians in cases of sickness." (System Physic, p. 44; by S. Thompson. Boston, 1825.) "The absurdity of bleeding * * * It is the same method to cure a sick man as to kill a wild beast. There are but two great principles in the constitution of things; the principle of life and the principle of death." (Prac. by S. Thompson, 4th ed., p. 103. Columbus, O.: 1827.)

ART. V.—*Influence of Anatomy on the March of Civilization:* By J. C. NOTT, M. D., Professor of Anatomy in the University of Louisiana.

THE science of medicine is so widely separated from the range of thought of the great mass of mankind, (even of those who have been liberally educated) that few, out of our profession, have any proper appreciation of the intimate connection which exists between its various branches and other departments of human knowledge, and a still smaller number have any idea of the immense influence the medical profession has exercised on the march of civilization. Could the people and their legislators be made fully sensible of the truths I propose to set forth, they would not hesitate to award to medical colleges, the credit which they justly merit. Few States in the Union have fostered these institutions with such liberal hands as that of Louisiana, and her people are already feeling benefits which will increase from year to year. But my object now is not simply to show that we are elevating the standard of medical education from year to year, and supplying the population with more intelligent and more skilful physicians, but that while we place the science of medicine on a more elevated platform, we, at same time, elevate all other departments of knowledge with it. As my space will not permit allusion to other departments of medical education, as chemistry, botany, etc., I shall confine my illustrations simply to that of Anatomy, to which too little importance is attached by those outside of our own profession, and I might add with truth, by too many in the profession.

So intimately are all the departments of human knowledge connected together, that it becomes very difficult to estimate the full value of any one, or its influence on others, however disconnected they may seem. It

may, however, be safely asserted that there exists no science, or no department of knowledge so isolated, that it does not receive light direct or indirect from others.

It may be laid down as an axiom, that *all our knowledge is derived from the study of nature.* The mind of man comes into the world a blank, and would remain so were it cut off from the observation and study of objects of nature. Locke, in his immortal work on the understanding, was the first to develop fully the great truth that all knowledge comes through our senses, for though the mind in its progress may develope certain abstract ideas, even these, if closely analyzed, will be found to have their ground-work in ideas received through the senses. Imagine a man who had never *seen* an object in nature—who had never heard a sound—who had never felt, smelt or tasted, and ask what abstract ideas his mind could originate, that would possess form or value. In fact, all our knowledge is the result of the action of our minds on materials derived from observation of the objects and phenomena of the external world. The boasted mind of man can *invent* nothing, but *falsehood*—all science—all knowledge is but the discovery of facts of nature and their relations. What, for example, is the noble science of astronomy? It is no *invention* of man. The master-minds of Newton, Kepler, Laplace, Herschel, Leverier and others, have but *discovered* a few of the laws which have governed the celestial bodies for millions of years. So with mathematics, natural philosophy, botany, zoology, geology, chemistry, anatomy, etc.

Every proposition in Euclid is based on a law of nature. It was not Euclid that made the right angle of a triangle equal to the sum of the other two angles—or the square of the hypothenuse equal to the sum of the squares of the other two sides. When the chemist tells us that all substances in nature may be resolved into some sixty simple elements—that these may be recombined to form well known inorganic compounds; and that they combine in certain definite proportions, he only proclaims *discoveries* of beautiful laws of nature.

How stands the case with our own science of medicine? What are we daily striving at? Is it not simply to extend our *discoveries?* We seek with the dissecting knife and microscope to extend our knowledge of the structure of the human body. We seek to extend our knowledge of its functions, and the actions of morbid causes upon it; and we seek in the vegetable and mineral kingdoms for remedies to heal its maladies. Every new splint the surgeon shapes, or new instrument he brings to light, is but an appliance to carry out a mechanical principle which exists in nature.

We may then, I think, fairly assume the truth of the two propositions

thus briefly alluded to: First, That the mind of man commences its existence in darkness, and would remain so, were it not for the perceptions received through the five senses, which place us in relation with the external world. Second, All our knowledge is derived from the observation of nature, and the operation of our minds upon ideas so obtained.

If, then, it be true that the mind of man is enlightened and expanded by the study of nature, it is evident that the wide field covered by the science of anatomy must yield the richest of all harvests, and one well worthy of the great minds of Cuvier, Palas, Blainville, Geoffroy St. Hilaire, Agassiz, John Hunter, Owen, Leidy, and many others, who have not only widely extended the horizon of speculative, but of useful knowledge.

You must not commit the grave error of thinking that the science of anatomy belongs to the doctor's shop alone. On the contrary, anatomy in its more general and philosophic acceptation, is the *Science of Organization*, considered in the different living beings, from the simplest zoöphyte up to man. The series of organized beings forms an immense chain, the extent of which the accumulated labors of ages has not been able to measure. The air, the earth, the mighty deep are all populated by living beings, which, by their infinite variety of organization, form and size, attest the inexhaustible fecundity of nature. Thus while among the mammifers life is maintained by the most complicated apparatus, we find at the other end of the scale, animals, like the hydra, polyps, etc., which have been alternately ranged with animals and vegetables. Nor are the varieties in size less striking, ranging from the mammoth or whale down to the animaleulæ, a million of which the microscope discovers in a single drop of water. Even the animaleulæ, so infinitely small to our senses, may in turn become gigantic, compared to others yet to be discovered with more perfect instruments.

You at once perceive then that anatomy is the most vast of all sciences —far too vast for any one mind. Hence the necessity of dividing it up into many departments, any one of which may fully occupy the lifetime of one man.

We shall proceed to point out in succession, the connection between anatomy and a few of the more prominent departments of human knowledge, viz: mental philosophy, fine arts, zoölogy, geology, jurisprudence, political economy, ethnology, natural and revealed religion, etc.

1st. Let us inquire what connection exists between anatomy and mental philosophy? Science is one thing—religion is another, and should in the present sensitive state of the public mind, be kept as much apart as possible, and it is not our desire here to make any allusion to the soul,

or immortal part of man; but the *mind*, the *intellectual powers*, are clearly functions of that organ called the brain, and we should commence our study of the mind, by the *anatomical* structure of the organ from which it emanates. The mind is almost null in the new-born infant—it grows and gains strength on to mature age—it is deranged by disease of the brain, and is annihilated by compression and other injuries. Every thing, then, proves that its intellectual functions depend upon the integrity of the anatomical structure of the *brain*.

The metaphysician who attempts to investigate thought, and to decompose the complicated operations of the understanding, can make no solid progress without laying his ground work in the structure, and functions of the various parts of the brain, organs of sense, and spinal system. The leading mental philosophers of the past age were fully sensible of the importance of anatomy, and Descartes, Locke, Malebranche, Condillac and others, were versed in anatomy and took it as their point of departure. But it is to the labors of Gall, and those who have followed in his footsteps, that we are mainly indebted for our advance in mental philosophy. While on the one hand, I do not mean to say that the present state of our knowledge justifies us in mapping out the faculties of the brain on its surface, as Gall has done, still the merit cannot be denied him, of having given a new impetus and better direction to the study of mental philosophy. He has at least clearly proven, that certain grand divisions of the brain, preside over certain groups of functions—the anterior—the posterior parts of the brain—the cerebellum—and the medulla oblongata; although their functions are not all clearly defined, yet differ widely in those they perform. And not less important are the great discoveries of Sir Charles Bell, of the functions of the spinal marrow, and its system of nerves. A familiar illustration of all this may be given in the organs of sense—the eye—the ear—olfactory organ—the tongue, have each a nerve leading to and connected with a point of the brain, and each has a distinct function to perform; did the optic nerve connect with the brain at the point of origin of the auditory nerve, there would be no vision, and *vice versa* of the auditory nerve; it is clear, then, that one point of the brain gives sight, another hearing, another smell, another taste, etc., showing clearly that the brain is a compound organ, having many functions to perform.

And here comes in the great science of comparative anatomy, to show how indispensable it is to the proper understanding of man, and to illustrate the remark of Buffon, that if "no other animal than man had lived, we should know much less of man than we do."

In fact, most of our knowledge of the functions of the nervous system

of man, has been derived from the dissections of animals. Now man belongs to that division of the animal kingdom to which the term *vertebrata* has been assigned by naturalists. The leading characteristic of this division is found in the existence of a brain, spinal marrow and vertebral column. The vertebrata have been subdivided into mammifers, birds, reptiles and fishes, all of whom have a brain and spinal marrow.

Anatomy teaches us that man possesses the most complicated and perfect nervous system known, and by following the chain on downwards through the mammalia, birds, reptiles, and fishes, we find that the brain becomes smaller and less perfect, one organ after another is diminished or left out, until in the lowest links but little brain and little intelligence is left. If we pass on from the *Vertebrata*, through the three remaining divisions of the animal kingdom, viz., the *Articulata*, the *Mollusca*, and the *Radiata*, we shall find that not only the brain, but the entire nervous system disappear, and even the special organs of sense.

It is by anatomy, then, that the mental philosopher has been able to study and analyze the structure and functions of the nervous system. He can go back and commence almost at the worm, and by following on the chain upwards through fishes, reptiles, insects, and warm-blooded animals, discover that one organ after another is added, each of which has its special use, until he reaches such structures as the brains of Napoleon, Newton, and Locke.

I have pointed out to you one of the great results of comparative anatomy. It is the ground work of mental philosophy, and no intellect, however gigantic, could ever trace the connection between mind and matter, without the dissecting knife and microscope.

Even sculpture and painting, those twin arts, that so charm the eye, refine the taste, and expand the mind of man, are not less indebted to the aid of anatomy for the high perfection to which they have attained in our day. A certain amount of anatomical instruction is now considered as essential in every school of design. What excellence could the historical painter attain, without a knowledge of those muscles of the face which give expression to the various passions and emotions of the mind; or of those of the limbs and trunk which give expression to the motions and attitudes of living man? The most beautiful object in nature to us, is the human form divine; who can gaze upon the Apollo or the Venus, the Laöcoon, or the dying Gladiator, without the most pleasurable emotion; and they give us pleasure because they are true to nature. It may be said that the sculptors who produced these unrivalled works of art, lived in a day when dissections were not tolerated, and anatomy was in its infancy. In reply, we say, that we do not know

with certainty even the names of many of the artists, and therefore can know little of their knowledge of anatomy; but we can say, whether they did or did not dissect, they studied the external anatomy of man with a fidelity which never can be surpassed; the swell and outline of every muscle, the eminences and depressions of every bone, the motions of every joint, every trivial furrow, every superficial blood-vessel, or tendon, the irregularities of the skull, etc., were all depicted with a fidelity which can never be excelled. No one, then, will deny that anatomy is an essential element in the education of the sculptor and painter.

Need I now address myself to the intelligent jurist and ask him to turn to Paris and Fontblanque, Beck, Taylor, and other works on Medical Jurisprudence, and say how much he owes to the labor of the anatomist, to say nothing of medical chemistry? How often is the anatomist called before the coroner, or into courts of justice to determine a point of life and death. Questions of infanticide have often to be decided by the testimony of the medical man alone; in cases of death arising from blows, wounds, etc., it is the anatomist who is to decide, and upon his testimony alone, hangs, in many instances, the lives of human beings, and the momentous decision of damning guilt, or of innocence. But it would be almost an insult to intelligence to dwell upon a point so palpable as this.

Where shall I begin, or where shall I end, when I undertake to tell you of the connection between anatomy and the vast science of *Zoölogy*? What is meant by the term *Zoölogy*? It covers the entire physical history of every living being, and every fossil remain, from the animalculæ or zoöphyte up to the most gigantic animal forms. Zoölogy, then, must necessarily have comparative anatomy for its ground work. Comparative anatomy not only examines into the minute structure of each organized species, but it is its province to compare them together, to study their affinities or discordances, to arrange them into groups having anatomical relations, and to open to our view that stupendous plan by which an All-wise Providence has populated and repopulated our globe. We have only to turn back to the works of Buffon to prove how indispensable is anatomy to the science of zoölogy. Buffon was a truly great and usful man in his way, and to the charm which his genius threw around Natural History, are we greatly indebted for the impetus given to its study during the past century. No man was ever more deeply impressed with the grandeur and beauty of Nature's works; no man ever searched more diligently for land-marks by which to classify and arrange the immense mass of material over which his mind wandered; and never did gorgeous and attractive style do more in awakening the attention of others to the study of Nature's works; but after all there was something

wanting to give solidity and durability to his plan, and to elevate Natural History to the level of a true science. It was the want of *anatomy.*

When the naturalist of our day looks over the fifty volumes of Buffon, he marvels at the extent of his labor; is fascinated with his beautiful descriptions of the external forms of animals, their habits, instincts, geographical distribution, etc.; but when we seek for those solid materials out of which to construct an exact science, we must turn to the labors of Daubenton, the modest, but gifted collaborateur of Buffon, who has left us dissections of nearly two hundred species of animals, so full and accurate as to leave nothing to be desired, as far as these details extended; but Daubenton attempted no complete classification of the animal kingdom.

Comparative anatomy, by leading us to an exact appreciation of the organic resemblances and differences presented by all animals, is now regarded as the sole solid basis of their classification.

It is to Cuvier that we are indebted for the first truly scientific classification of the animal kingdom, which is founded on the organization, and the principle of natural anatomical affinities. It follows the descending order, beginning with the most complex type at the summit, and placing the most simple at the bottom of the scale. He divides all known animals into two grand anatomical divisions, viz: the *Vertebrata*, or those having a spinal column, and second, the *Invertebrata*, or those having no spinal column. The latter are again divided into *Articulata*, *Mollusca* and *Radiata*, and the whole animal kingdom is thus divided into four grand departments, which are based entirely on anatomical distinctions. If we go on down through the orders, classes, families, genera and species, we find that anatomy is still our guide, and the only means by which we can surely place each animal. It is only when well marked anatomical distinctions cease, that our doubts and disputes commence.

When, for example, you place beside each other, a bear and a dog, you discover anatomical differences which at once point to specific difference and distinct origin. But when you place beside each other the dog, wolf, fox and jackal, though they have some external differences, their anatomical structure is identical—they have the same number and form of teeth, the same number and form of bones in the skeleton, and the same internal organs; but, the comparative anatomist requires but a single bone or tooth of the bear to say that it is not a dog, or wolf, or deer—but a bear, and nothing but a bear.

If the question be asked, "Are all horses of one species or origin?—are all dogs—all cattle—all our domestic sheep and goats of one species,—nay, more, are all the races of men of one species?" We are at sea

without a compass. Anatomy ceases to guide us, and it is probable that such disputes will never be settled.

And what shall I say of the noble science of Geology, which strives to know the chronology and formation of the globe? It goes hand in hand with comparative anatomy, like sculpture with painting—in fact, organic remains are the hand-writing of the Almighty upon stone, by which he has revealed to the geologist all that we know of Preädamite history of the Earth, and it is to the anatomical labor of the immortal Cuvier, that we are indebted for our most important geological knowledge.

The following quotations, from the work of Professor Hitchcock, on "Geology and Religion," in allusion to the fossil remains of extinct animals will give some idea of the intimate and extensive connection between comparative anatomy and geology. "The fossiliferous rocks," says he, "or such as contain animals and plants, are not less than *six or seven miles in perpendicular thickness*, and are composed of hundreds of alternating layers of different kinds, all of which appear to have been deposited, just as rocks are now forming, at the bottom of lakes and seas; and hence their deposition must have occupied an immense period of time. Even if we admit that this deposition went on in particular places much faster than at present, a variety of facts forbids the supposition that this was the general mode of their formation.

"The remains of animals and plants found in the earth are not mingled confusedly together, but are found arranged, for the most part, in as much order as the drawers of a well regulated cabinet. In general, they appear to have lived and died on or near the spots where they are now found; and as countless millions of these remains are often found piled together, so as to form almost entire mountains, the periods requisite for their formation must have been immensely long.

"Still further confirmation of the same important principle, is found in the well established fact, that there have been upon the globe, *previous to the existing races, not less than five distinct periods of organized existence;* that is, five great groups of animals and plants, so completely independent, that no species whatever is found in more than one of them, have lived and successively passed away, before the creation of the races that now occupy the surface. Other standard writers make the number of these periods of existence as many as twelve. Comparative anatomy testifies, that so unlike in structure were these different groups, that they could not have coëxisted in the same climate and other external circumstances.

"There are abundant facts to prove that the climate of the whole globe was as warm or even warmer than the tropics of our day, and

the slow change from warmer to colder, appears to have been the chief cause of the successive destruction of the different races; and new ones were created, better adapted to the altered condition of the globe; and yet each group seems to have occupied the globe through a period of great length, so that we here have another evidence of the vast cycles of time that must have rolled away even since the earth became habitable.

"Among the thirty thousand species of animals and plants found in the rocks, very few living species have been detected; and even those few occur in the most recent rocks, while in the secondary group, not less than six miles thick, not a single species now on the globe has been discovered. Hence the present races did not exist till after those in the secondary rocks had died."

Another very curious result of anatomical investigations is the fact, "*that the laws of anatomy have always been the same since organic structures began to exist.*"

"It had long been known that the organs of animals were beautifully adapted to perform the functions for which they were intended. But it was not till the investigations of Baron Cuvier, within the last half century that it was known how mathematically exact is the *relation* between the different parts of the animal frame, nor how precise are the laws of variation in the different species, by which they are fitted to different elements, climates, and food. It is now well known that each animal structure contains a perfect system of *correllation*, and yet the whole forms a harmonious part of the entire animal system of the globe."

To use the language of Cuvier: "Every organized individual," says he, "forms an entire system of its own, all the parts of which mutually correspond, and concur to produce a certain definite purpose, by reciprocal action, or by combining towards the same end. Hence, none of these separate parts can change their forms without a corresponding change in the other parts of the same animal, and consequently each of these parts to which it has belonged. Thus, if the viscera of any animal are so organized as only to be fitted for the digestion of recent flesh, it is also requisite that the jaws should be so constructed as to fit them for devouring prey; the claws must be constructed for seizing and tearing it to pieces; the teeth for cutting and dividing its flesh; the entire system of the limbs, or organs of motion, for pursuing and overtaking it; and the organs of sense for discovering it at a distance. Nature must also have endowed the brain of the animal with sufficient instinct for concealing itself, and for laying plans to catch its necessary victims." This is the picture of a carnivorous animal, and the same adaptation of structure and instincts to mode of life will be found in the herbivorous and all others,

large or small, perfectly or imperfectly organized. "Hence," says Cuvier, "any one who observes merely the print of a cloven hoof, may conclude that it has been left by a ruminant animal, and regard the conclusion as equally certain with any other in physics or in morals. Consequently, the single foot mark clearly indicates to the observer the forms of the teeth, of all the leg bones, thighs, shoulders, and of the trunk of the body of the animal which left the mark.

"By thus employing the method of observation, where theory is no longer able to direct our views, we procure astonishing results. The *smallest fragment of bone*, even the most apparently insignificant apophysis, possesses a fixed and determinate character relative to the class, order or genus and species of the animal to which it belonged; insomuch, that when we find merely the extremity of a well-preserved bone, we are able, by a careful examination, assisted by analogy and exact comparison, to determine the species to which it once belonged, as certainly as if we had the whole animal before us."

Cuvier first fully tested this law of correllation of parts, in existing animals, but it remained for him to apply the same principles to remains of fossil animals. It was certain that if the laws of anatomical structure were the same in the pre-Adamite creations, as they now are, the above principles must apply equally well to the bones found in the rocks, and though often only scattered fragments are brought to light, the anatomist would be able to *reconstruct* the whole animal, and present him to our view.

The quarries around Paris had furnished a vast number of bones of strange animals, and these had been thrown promiscuously into the collections of the city. Cuvier determined here to put his principles to the test amidst this huge and confused mass of fossil remains. "I found myself," says he, "as if placed in a charnel-house, surrounded by mutilated fragments of many hundred skeletons, of more than twenty kinds of animals, piled confusedly around me. The task assigned me was to restore them all to their original position. At the command of *comparative anatomy*, every bone, and every fragment of a bone resumed its place. I cannot find words to express the pleasure I experienced in seeing, as I discovered one character, how all the consequences which I predicted from it were successively confirmed; the feet were found in accordance with the characters announced by the teeth; the teeth in harmony with those indicated beforehand by the feet; the bones of the legs and thighs, and every connecting portion of the extremities, were found to set together precisely as I had arranged them, before my conjectures were veri-

fied by the discovery of the parts entire; in short, each species was, as it were, reconstructed from a single one of its component elements."

Here, then, we have demonstrated, by the light of anatomy, that the same laws of life which now prevail in the world, have existed through a succession of creations, through thousands, tens of thousands, and probably millions of years.

Organic remains, too, teach us that there have been, in the remotest geological epochs, viviparous as well as oviparous creatures, and gemmiparous as well as fissiparous animals and plants. There were also carnivorous, herbivorous, and omniverous animals; they possessed the same internal organs as animals of our day and they were governed by the same physiological laws as those of the present day.

What a wonderful achievement is it for anatomy, that it should thus have exhausted the existing Fauna of the earth, and have followed up, step by step, the foot-prints of the Almighty, through this plan so vast in time, so gigantic in extent, and so infinite in wisdom!

Nor is anatomy without intimate connections with both natural and revealed religion. Of the latter, I have not time or inclination to speak here; but what need is there of volumes on natural religion with facts before us like those I have presented. No sane man could follow this long chain of organized beings through creation after creation, through those countless ages of time, could reflect on that plan which has been working with such harmony from beginning to end, and doubt that there is a Supreme and Overruling Intelligence, and that we are responsible to him, and him alone, for our acts in this world.

Had we time, I could easily show you to what extent even physics are indebted to anatomy. The human frame is a beautiful machine, from which the mechanic arts have received many useful lessons. It is a system of levers, fulcrums, pulleys, admirably arranged for motion and strength. The celebrated Kepler devoted much study to the structure of the eye. It suggested to the mind of Euler the possibility of achromatic lenses, and the whole science of optics has gained much from the study of the eye. Of like importance has been the anatomy of the ear, on which is based the science of acoustics.

I will conclude, by calling your attention for a few moments, to one point more, and not the least important, viz: the connection between anatomy and ethnology.

When we cast our eyes over the broad surface of our globe, and behold the endless variety of human beings which inhabit it, of every shade of complexion, of the most opposite physical conformation; and of the most diverse moral and intellectual characters; I say, when we look upon

all this, in spite of the influence of early education, we are driven to ask ourselves the question, "Are these races of men of one parentage, and are they alike capable of moral and intellectual cultivation?" The *origin of man* is a momentous question, one which has elicited much discussion among the wisest heads, and is one which I shall not touch here; but still the physical history of man has certain important connections with the view of anatomy I am now placing before you, which render it impossible for me to pass it by in science, without doing manifest injustice to the science of anatomy, which it is my duty to teach.

Of the precise epoch at which man was placed on earth, we know nothing. The short chronology of Archbishop Usher, of six thousand years, is now abandoned by every well informed writer in the church and out of the church; it may be ten thousand, twenty thousand, or one hundred thousand for aught we know. Nor can we divine how far back such well marked races as Jews, and other white races; Negroes, Mongols, Hottentots, Australians, Indians, Malays, etc., etc., first made their appearance. This much however, has been positively demonstrated through evidence derived from Egyptian, Assyrian, and Chinese monuments and records, viz., that the Jew and other white races, as well as Negroes, Egyptians, Mongols and other well marked types of man existed at least five thousand years ago as distinct from each other as they now are; and it is equally certain that these forms have, during this long period, been permanent and unchanged by climate or other existing physical causes; no race during this time has been transformed into another, whatever miracles may have been performed in earlier ages.

We not only have evidence of the existence of distinct races from the Bible, Homer, Herodotus and other early writings, but we have actual portraits of races, on the monuments of Nineveh, Babylon, and Egypt, dating back several thousand years. Nay, more, if all this were not sufficient, we have other overwhelming evidence of the antiquity of races. We have the mummies of Negroes, Jews, and other races taken from the ancient catacombs of Egypt, which place the question at once in the hands of the anatomist. We have other human remains from the barrows of Britain and Germany, from the mounds of America, from the ancient burying grounds of Mexico and Peru, from the ruins of Nineveh, from the bone caverns and other sources, all of which give to the anatomist ample material for proving the *permanency* of types through all known physical influences, and all known time.

When these different races of men are brought together and compared *anatomically*, they are found to differ quite as widely in structure, as do the species in most of the genera of animals. Compare, for example, the

skeletons of the African Negro, and the Anglo-Saxon, and I do not hesitate to assert that they differ as much as those of the polar and tropical bears—the horse and the ass—the dog and the jackal, or the lion and tiger; but we shall call your attention to but one anatomical point by way of illustration. I will select the brain, as it is the organ of intelligence, which should form the most striking and satisfactory point of distinction.

The lamented Dr. Morton, of Philadelphia, who has done more in this department of anatomy than any other man, collected and measured accurately more than one thousand skulls of various races. Since his death, his collection, which has been deposited in the Academy of Sciences, of Philadelphia, has been considerably extended, and has been re-measured by Dr. Meigs, with the same results. It has thus been ascertained that not only do the races show a great variety of forms in the external configuration of the skull, but a marked difference in the size of the brain. If we compare together the two extremes, viz., the Teutonic and North European races below the Artic, with the Hottentot of the Cape of Good Hope and the Australian, we find the former have seventeen cubic inches more of brain than the latter; if then we take the Mongol races, the Malays, the Negroes of tropical Africa, the American Indians, and other races, we shall find that there exists a gradation between the two extremes, and that anatomy assigns to each its proper position in the scale.

Nor is it less certain that the large-headed, fair-skinned races have been the rulers of the world and the depositories of all true civilization. The Mongol races, who have brains intermediate in size between the white and black races, have long since achieved a semi-civilization beyond which they cannot rise; while the intellects of the darker races have remained as dark as their complexions. From the Barbary States to the Cape, no race has ever invented an alphabet—no manuscript or tablets have been found to tell their rude history, and no architectural remain has been found to mark a bright spot on the five thousand years, during which we know they have existed.

Now, *when* did this great inequality of races begin? Of this we know nothing, but the great practical fact remains that it has existed for several thousand years, and we have every reason to believe that it will remain till these races are extinct. We have shown that both anatomy and history prove that the physical causes now in operation on earth has not been able to transform one type into another.

There are well meaning philanthropists who talk to us about the influence of education and sing hosannahs over the wonderful results which

are flowing from the colony of Liberia, and the efforts of Missionaries in civilizing these inferior races. Anatomy teaches us that it is impossible, as history has before done, until you can fall upon some plan by which seventeen cubic inches of brain can be added to that of the Hottenot, the Bushman, or the Australian, you cannot add to his intellect, or his capacity for civilization. I have examined this question with a great deal of care, and can find no facts, or evidence whatever to show that education can enlarge the brain. We know that the muscles of the arm of the blacksmith will enlarge by the constant use of the hammer, but when did thought ever increase the size of the brain, any more than vision does that of the eye, or hearing the nerve of the ear? There is no analogy between the actions and functions of the nervous system, and those of the muscular system.

The brains of royal families—of nobility, which have been educated classes for centuries, have no larger heads than the lower classes. Educated communities have no advantage in this respect, over the uneducated. The ancient Britons and Germans had the same skulls as their more civilized descendants. The heads of Napoleon, Newton, Shakespeare and Locke were as large at the age of sixteen as at fifty. In a word, there is no more evidence to prove that you can change the size and form of head in a race, than that you can change the form of the negro's foot, his pelvis, his thick lips and nose, his black skin and wooly hair. These are anatomical peculiarities stamped on him by his maker at some time unknown to us, and nothing short of the miraculous interference of the same power can change his anatomical structure. Anatomy thus teaches us a great practical fact and one which should be well understood, by the statesman, the philanthropist and the theologian. We must take the works of God as we find them, and not suffer ourselves to be misguided by Utopian schemes. As Knox quaintly said, you cannot change an Irishman into an Englishman by act of parliament.

But it is time that I should draw this rambling essay to a close. The subject is sufficiently extensive for volumes, and if I have succeeded in directing thought to the value of anatomical studies my aim is fully met.

It must be clear to any one who takes the trouble to investigate the subject, that anatomy has, from the time of Aristotle to the present day, exercised a controlling influence over many departments of human knowledge, over the progress of civilization.

Read the history of the world and you will find that medical colleges have ever been the foci around which have clustered the whole circle of natural sciences.

ART. VI.—*Rupture of the Cornea, with Dislocation of the Crystalline Lens and Capsule:* By J. COWLING, M. D., Houston, Texas.

MR. J. C., of this place, consulted me in consequence of having received, some three weeks previous, an injury of one of his eyes, by being thrown from his horse with a good deal of force, among hard clods of earth. He related that on getting up, the eye in question was full of dirt, and that some time elapsed before it could be removed. The accident was accompanied with severe pain, and was followed, in a short time, by a high degree of inflammation. He says that he is still troubled, although in a less degree, with the pain and inflammation; the patient mentions having used ice water ever since the accident.

On examination, I found the left eye a good deal inflamed, the cornea hazy, and with a wound a little above the median line of the external canthus, and close to the edge of the selerotica, projecting from the wound to the extent of several lines. Closely embraced by the ruptured edges of the cornea were the capsule and lens. Pupil dull and sluggish, with a portion removed, leaving a wound somewhat in the shape of the letter u, through which the capsule and lens had passed.

The obvious treatment of the case was to reduce the inflammation and remove the strangulated capsule and lens. For the first few days remedies for the reduction of the inflammation were used but not with that full benefit which was anticipated. The irritation, was, no doubt, kept up by the body protruding through the cornea. I therefore concluded, without waiting longer for the reduction of the inflammation, to attempt the removal of the exciting cause. But here two modes of treatment suggested themselves; either to snip the projecting capsule and lens off level with the cornea, or to remove the same by caustic applications. The first had the recommendation of at once removing the irritating body. Two objections, however, presented themselves to this mode of treatment, and were of sufficient weight to cause me to prefer the latter treatment. In the first place, in using the scissors, there was some risk that a clean cut may not have been made, and in consequence, a certain amount of force would have been applied to separate the parts which not unlikely would have resulted in removing the posterior portion of the capsule and lens; and had such a result happened, the probability is, that an evacuation of the aqueous, if not also the vitreous humor would have resulted. Secondly, there was another risk in using the scissors, namely, had the posterior portion of the capsule and lens not been firmly embraced by the edges of the lacerated cornea, they would most likely have fallen into the anterior chamber, and there cause sufficient irritation to destroy the organ. Taking the above view of the case, I commenced the treatment

by using a strong solution of argt. nit. with a camel's hair pencil on the projecting capsule and lens, having previously protected the adjoining parts by ol. oliv.

It is unnecessary to report the daily treatment, as it was but a repetition. The parts were touched every three or four days. This course was continued between two and three months, the cure being protracted in consequence of the irregular attendance of the patient, as also from his habit of free living.

The eye now presents the following appearance, all treatment having been abandoned for this last four months : Cornea clear with an almost imperceptible ridge at the seat of injury ; pupil clear and active ; the wound made by the passage of the capsule and lens is about half the size it presented at the commencement ; vision clear.

The confusion which existed in consequence of the different focal powers of the two eyes after the removal of the lens has, in a great measure, subsided, and can be still more obviated by the proper selection of glasses.

The above case may not be considered as possessing any interest, or at least not sufficient to be reported. It does however, show very distinctly that a most tender and sensitive organ may be restored, if not in all its integrity, still amply for all useful purposes, and without disfigurement, for, to a casual observer, the eye does not indicate any abnormal appearance.

Houston, October 10, 1857.

Art. VII.—*Observations upon Hernia and Obstructions in the Bowels:*—By Warren Stone, M. D., Professor of Surgery in the Medical Department of the University of Louisiana; Co-Editor.

It is remarkable, that while there is a general cry in favor of medical reforms and of elevating the standard of medical education, there should be so great a neglect of the undisputed truths and facts of medical science. There is no disease or defect, to which the human system is liable, more common than hernia ; and few so troublesome to bear, and so dangerous to life, and yet so entirely remediable ; but how few practitioners feel themselves prepared to manage it in all its difficulties.

Surgical writers have exaggerated the difficulties and dangers attending operative means, and those who are not familiar with the anatomy

concerned in this disease, find nothing to encourage them in consulting their books, and in case of incarceration or strangulation the proper and safe period for operating is often lost in ineffectual attempts at the taxis.

I will not pretend to enter into a description of the various kinds and forms of hernia, but will presume that the reader is familiar with the standard works on the subject, and only offer some practical remarks.

The common fault with practitioners, when called to a case of hernia, is in not carefully estimating the resistance they will meet with in reducing it. This can be done with considerable accuracy, by examining the tumor, and observing the general effect upon the system. When the neck is small in proportion to the size of the tumor, and the tumor at the same time is very tense, there is reason to fear strangulation. It is folly to follow any rule as to the time we may delay the operation in a case of strangulation. The bowel is in danger from the moment it is strangulated, and should be relieved as soon as possible. In a case of perfect strangulation, such will be the hardness of the neck and the tension of the hernial tumor, together with the pain of the part, and the anxiety of the patient, as to leave no doubt as to its condition, and there should be no delay in relieving it. These cases do not often occur, but when they do, they prove fatal, unless relieved immediately.

A sailor lad, about eighteen years of age, was admitted into the Charity Hospital, (when I was Assistant Surgeon) about ten o'clock in the morning, with an inguinal hernia of an hour's standing. While lying with his abdomen across the ship's yard, doing something to the sail, a sudden effort caused the bowel to descend for the first time. The tumor was quite hard ; its neck small and hard ; and there was pain and anxiety, but no vomiting, and the pulse not much accelerated. It was thought necessary to go through the usual routine of preparation and attempts at the taxis, and in this about six hours were wasted. The operation was then made, but it was too late. The bowel was perfectly black, and the circulation was not restored. Gangrene followed, and death took place two days after.

The operation, in such a case can be made very simple, more so, even than the common operation of venesection. The means recommended by Seutin will often suffice in recent cases, where the parts are in a natural condition. Seutin's method consists simply in passing the finger up to the point of structure, and forcibly thrusting it into it, either lacerating it or dilating it, so as to relieve the bowel. I have succeeded in this way, and I think in recent cases this method will generally succeed. But if this means fails, it is almost as simple to cut down through the integuments and superficial fascia and to divide the structure outside of

the hernial sack. There is no necessity for making the incision so long as is usual when the sack is opened, though it is well to make it ample. The hernial sack can be exposed without the slightest risk.

There seems to be a prejudice with many surgeons against this simple mode of operating; but its simplicity is not its only advantage. The bowel is much more easily returned when enclosed in the serous sack, than when exposed to direct manipulation. Serous membranes are easily excited to inflammation by handling.

I do not mean to say that this operation should be resorted to in all cases when the taxis fails to effect a return immediately, for in simple incarcerated hernia we may not be able often to return it without considerable preparation; but then the symptoms are not urgent and there is no danger until the bowels begin to suffer from the obstruction. The difference between strangulation and simple incarceration is so plain and marked, that it cannot fail to be observed. When strangulation exists the operation ought to be performed without delay. In cases of incarceration we can delay until the bowels suffer from the obstruction, which is manifested by vomiting, but no longer. It is highly improper to delay until the patient presents the dangerous symptoms of extensive mucous irritation, which is shown by the constant vomiting, cold, clammy perspiration, and small frequent pulse.

Perfect strangulation is very rare, and when it does occur the tumor often is not returned without the stricture is relieved either by the method of Seutin, or by the knife. But incarcerated hernia, that would be returnable if properly managed, are often aggravated into inflammation or engorgement, by injudicious management. Physicians are disposed to make strong attempts at the taxis, without preparing the patient, and, after using more or less violence without success, try some relaxing means, and then again resort to the taxis, and again failing in this, resort to some other means, and so on, until the parts become inflamed.

We can easily predict when difficulty is likely to be experienced in the reduction of the hernia, and when there is likely to be any, we ought to bring to bear every agent in our power that can relieve it in any degree. Among these means, ice to the tumor, and large cold water injections are the most useful. Tart. of antimony is a very efficient remedy, if properly used. It should be used to nauseate, but not to produce vomiting. Blood-letting also may be resorted to in robust subjects. But all medical men are fully acquainted with the best means to relax the system and to condense the gas in the intestines, and to relieve engorgement. But I would impress upon them the importance of bringing to bear all these agents, in their full force, before handling the tumor or making any forcible attempt to reduce it with the hands.

If after all these preparations, we fail in the reduction, and the usual symptoms of obstructed bowel come on, we should operate. It is true that the bowel sometimes returns spontaneously, under the influence of strong opiates, after very well directed means have failed; but I insist that it is not the exercise of good judgment to leave vital organs in distress and danger, when they can be relieved by perfectly safe means.

I have said that surgical writers have exaggerated the dangers of the operation for hernia, and it may be proper to describe or point out their exaggerations.

The epigastric artery is the bug-bear in inguinal hernia and the obturator in femoral. The epigastric arises from the external iliac at a point midway between the pubis and the anterior superior spinous process of the ilium, and runs upwards and inwards under a line marked from its point of origin to a point two inches below the umbilicus. In indirect hernia it is in no more danger than the aorta, and in direct hernia, it would require an extensive cut to reach it, whether Cooper's directions to cut upwards are followed or not. The obturator artery is only alleged to be in danger when it arises from the epigastric, and thus, it is said that it crosses the sack of the hernia, and Scarpa has represented such a case in a plate. Often the obturator artery comes properly from the internal iliac; but it does occasionally come from the epigastric. I have dissected a great many bodies, and have directed my attention to this point of anatomy, and I have never found a case where it could have been possible for the intestine to descend behind it, or in a way so that the artery could possibly have been in the way of an operation.

The intestine itself, then, is the only thing in danger of being cut, and this, in most cases, it is not necessary even to expose. But if it should be necessary to open the hernial sack, it can be done with as much safety as venesection can be performed at the bend of the arm. In the operation for femoral hernia, I have seen the saphena major vein exposed, and it is proper to examine for it, when operating for this variety. There is no doubt but many deaths are caused by hernia, and the real cause of death is not known. Several such cases have come under my observation when the nature of the disease was not discovered until it was too late to save life.

A few days since, a gentleman came to my hospital suffering from great irritability of stomach and constipation. He had had no evacuation from his bowels for five days; had vomited at short intervals during this period. He had consulted a physician who ordered him pills, but they produced no other effect than to increase the vomiting and abdominal distress. His pulse was frequent and feeble; his countenance was anxious,

and he presented the usual collapsed appearance of extensive irritation of the bowels. This was evidently a case of obstruction of the bowels, but the patient, on inquiry, threw no light upon the nature of it. I proceeded to examine all the openings where obstruction is liable to occur, and found a femoral hernia, quite small, and partly covered and obscured by an enlarged lymphatic gland. It was easily returned, and in a short time he had copious evacuations from his bowels, with entire relief; but his bowels suffered from the irritation for some time. This patient could not have survived the obstruction much longer. During my term as House Surgeon to the Charity Hospital, there was quite a number of cases of enteric irritation during the summer following an epidemic of cholera, and they presented a peculiar appearance. I had remarked it to the students, and in paying my usual mid-day visit, I observed a new patient, and he presented this appearance in a marked degree, and I called the attention of students to it, but on asking him how many evacuations he had had from his bowels, a laugh was raised at my expense, when, instead of stating that he had had a large number of serous discharges, he said his bowels had not been moved for eight days. The idea of obstructed bowel directly occurred to me, and on examination, I found inguinal hernia. The patient being much relaxed, it was readily returned, when copious serous discharges took place, and the patient was saved, with difficulty, from their exhausting effects. I asked why he did not tell me the truth with regard to his sickness? and his answer was, that he had no idea that the swelling was the cause of his vomiting.

Some do not call the attention of the physician to this disease, from feelings of delicacy. This is more particularly the case with females. A female came to the Charity Hospital a few weeks ago, in a very low state, having vomited for five days. The assistant house surgeon, Dr. Bickham, who was present on her admission, suspected hernia, and on examination, found a swelling in the left groin, and as the case was a critical one, and the house surgeon being sick, he sent for me. I operated at once, and divided the stricture outside of the sack. The bowel was easily returned. The urgent symptoms subsided. The bowels were freely moved the next day, and every symptom appeared favorable; but on the third day violent peritonitis set in, and she sank in about thirty-six hours. I was fearful that I had committed an error in returning the bowel without opening the sack, to see whether it was sound. But an examination showed that the bowel was engorged merely, and perforated by an ulcer which gave outlet to the contents and caused the uncontrollable inflammation. It is, no doubt, proper in bad cases, to open the sack so as to avoid the danger of returning a gangrenous intestine, but this

can be done sufficiently to ascertain the state of the bowels, and still have the sack surrounding the intestine sufficiently to assist in its return.

Femoral hernia, when small, is often obscured by enlarged lymphatics, and the frequent descent of the bowel seems to create a thickening of the parts. I was once deceived by this thickening and enlargement, though my examination was not as critical as it would have been, if my opinion was to have been needed in the treatment. The case was in the Hospital, but not under my charge, and my attention was called to it by the student, as I was passing through the ward. The symptoms became worse, and she was operated upon the next day; but it was too late. Soon after this, I was called to an old lady, about sixty years of age, who was suffering from violent vomiting, great pain in the abdomen, with constipated bowels. She said that she had been subject to these attacks for many years. She described the pain as always commencing just above the pubis, and shooting upwards, and it appeared very much like neuralgic pain. The symptoms of obstructed bowel, however, led me to examine for hernia. I found quite a hard oval tumor in the left groin, which she said had been there for many years. I pressed my fingers under it in the direction of the femoral ring, but was not satisfied that I had felt the intestine. I ordered Battley's sedative to be given until the pain subsided, and then a large injection of the infusion of senna. On returning the next morning, I found that the bowels had been freely moved and the patient entirely relieved. Two days after this, a messenger came to say that the symptoms had returned, and to request another visit. It so happened that I could not make the visit at the time, and advised that the Battley's sedative and the large injections should be resorted to, as before, and to notify me if she was not relieved. I heard nothing more for seven or eight days, when I was again called, on account of severe inflammation and suppuration, as I was told, in the region of the tumor spoken of above. I found that a homœopathist had been treating her since the day I was last sent for, and getting alarmed at the symptoms, gave it up. I found an extensive inflammation involving the lower part of the abdomen and upper part of the thigh, with a gangrenous spot in the region of the groin, which had just given away; and there was a large discharge of offensive matter, a portion of which, was found on inspection to be from the bowels. There had been a hernia; the bowel had given away, and through inflammation, suppuration and gangrene, the contents had been conducted to the surface, and an artificial anus was the most favorable result that could be expected.

This patient was very low, but under the free use of tonics, stimulants, and supporting diet, she rallied; the slough was separated; but she did

not improve in strength in proportion to her appetite and the nutriment that she took. On inspection, it was found that the discharge from the artificial opening appeared almost like chyme, as it passes from the stomach, and often portions of the food could be seen. I found she was ordered injections, and I became satisfied from the amount of the discharges that the intestinal canal was not entirely cut off. When the bowels were kept freely open, the discharge through the artifical anus was less; and finally, after about three months, the artificial opening closed. When I saw her, a few months since, (about four years since her disease left her) she was in excellent health, and was active for one of her age, and has had no more colics or neuralgias of the abdomen. The probability is that when I examined the tumor on my first visit, that I pushed up the bowel, or it may be that the large purgative injection dislodged it. It must have been pushed up, however, by pressing on the tumor, for I was prepared by the previous case and made a very thorough examination. The probability is that the intestine was merely incarcerated and not strangulated, and that it was opened by ulceration and not by gangrene.

Obstruction of the bowels occur from other causes, and when we cannot get at the cause and remove it by mechanical means, we must adopt the best means in our power. I will relate two cases illustrative of the good effects of large injections or purgative injections, or such as produce a strong action on the bowel. An engineer, on a steamer, when high up Red River, was taken with what he supposed to be colic, and commenced to take physic for relief, but he vomited it all up, though he continued to take it until he arrived in the city, five days after the attack. I found him in a dull, feeble state, with rapid pulse, and his symptoms were so characteristic of hernia, that I examined for it, but found none. I ordered opiates to relieve the pain and vomiting, and large injections, also mustard to stomach and abdomen. On my return about two hours after, I discovered in the chamber something like fæcal matter, and asked if his bowels had been moved, but was told that he had vomited what I saw. On further inquiry, I learned that he had worn one of Stagner's trusses some two years, but considering himself cured, had left it off for nearly a year. I examined again, but could feel nothing through the walls of the abdomen that would indicate obstruction. I suggested that an operation might find the intestine obstructed by a returned sack that had been thickened at its neck by pressure. The injections, however, had not been given properly, and I procured a force-pump, and gave one myself. His bowels received a very large quantity of simple water,

larger than I thought they could hold,* and just as I was about to desist for fear of doing injury, the patient suddenly exclaimed, "there, Doctor, it is unkinked;" and so it was, for the discharge was copious, and it was with difficulty that it was restrained, so great and extensive was the mucous irritation. I can form no idea of what the real nature of this obstruction was. I could relate a great many cases of obstruction from different causes; but they would not be instructive, as the general treatment would be the same, and the correct diagnosis can only be made on the cadaver.

A medical friend called me in consultation, in the middle of the night, to see the captain of a steamboat that had just arrived, who was supposed to be laboring under strangulated hernia. He had been vomiting four days, his bowels constipated, and he presented dangerous symptoms. On examining the scrotal tumor, I found that it was hydrocele and not hernia, and would have left it for future attention, but observing that the friends seemed to doubt the correctness of my opinion, I made a puncture and discharged the water, having no doubt in the case. The captain had taken purgatives freely, but the course of treatment was changed to opiates by the mouth, and copious tepid water injections, but with no effect in relieving the bowels, though the absortion of some of the injections seemed to be of service. He continued five days in this way, growing weaker, and he made his preparations for death. At this time we agreed to try the effect of croton oil, by injection, with a view of exciting the lower bowels strongly. Six drops were mixed in an emulsion and given. He complained bitterly of the heat and burning, but it had the effect of relieving the obstruction. This could not have been one of intussusception, for it continued so long that inflammation would have destroyed the parts; unnatural loops are formed sometimes, through which a fold of the intestine falls, and the passage is obstructed, but the bowel is not strangulated. In one case I found the free end of the vermiform appendix adherent to the brim of the pelvis, and a portion of the small intestine had fallen through. Whatever the nature of the obstruction may be, injections are appropriate, and nearly as useful, as purgatives by the mouth are injurious.

One of the most careless and thoughtless habits of physicians is, in giving purgatives in colics. It is very rare that the pain experienced, is caused by any irritating substance in the bowels, and while it is in this state of spasm and contortion, purgatives never operate; they are liable to do much harm. When all is quieted by opiates and other quieting and

* I have ascertained in the adult cadaver of medium size, that eight pints of liquid may be injected into the large intestine (not a drop of which passed beyond the ileo-cæcal valve) without producing any marked physical strain or distention, which might not apparently have been safely borne during life.—B. DOWLER.

relaxing means, purgative injections will generally be effective. When a person is taken with pain in the abdomen, and vomiting, and is obstinately constipated, it is impossible to determine whether it is simple colic or something more grave. I am satisfied that if all these cases, without distinction, were treated with opiates, bleeding, (if the patient will bear it,) warm baths, and large injections, intussusception would be very rare, and death from obstructions of any kind would be much more rare than it is.

Large distension of the bowels shortens them and produces traction, and so does active peristaltic action excited below the obstruction.

ART. VIII.—*An Essay on the Ætiology, Pathology, and Treatment of Epidemic Dysentery:* By JOSEPH B. PAYNE, M. D., of Magnolia, Arkansas.

WHEN we look back over a series of ripened years that have flown away into the dim and shadowy past, and take into consideration the vast numbers of our citizens who are suddenly snatched from the stage of action, by the desolating epidemics of our country, and in defiance too, of the most energetic and vigorous efforts of our most talented and eminent physicians, and see how unavailing and ineffectual the greatest skill is, in checking their progress, and staying their fatality, we must at once be convinced of the importance of any evidence, and facts, which may shed any light on their history. Facts, judgment, and theory have analytically been pointed at, as standing like the sacred tripod of medicine. Facts we must have, to exercise the judgment, and from inferences, certain principles are established to form a theory, the whole being embodied, we come at the science of medicine. So extensive and important is the scope of facts in the study of medicine, that in proportion to their number and import, our knowledge of the healing art can be defined sufficient or imperfect. It may even be said that the study of it would be fruitless, was it not incessantly assisted and guided by the evidence of facts and observation.

Dysentery is by no means the least formidable and direful of epidemics, and its frequent visits and fatal character demand physicians to turn their attention to its investigation. Truly, the science of medicine has, within a few years past, made rapid advances towards maturity, but

is yet in its infancy; and, much, very much, remains to be done, before it can arrive at any degree of perfection. It therefore becomes the indispensable duty of every physician to devote his mind and talent to augment and promote this all-important object, and when any observations are made, they should be communicated to the public; for, if it be nothing more than a hint, others may improve it to the lasting benefit of science and of humanity, for there are no circumscribed limits to human science, because perfection is not an attribute of human wisdom; and, however abundant and apparently sufficient our records of medical facts might be, they have been considerably increased in this New World, where a laudable emulation, heightened by the holy motives of philanthropy, has treasured up new materials from new theatres of diseases and observations, and where it is *in magnis voluisse sat est.*

Fearing that I may weary your patience with this exordium, I will return to my subject and drop into it at once *in medias res.* Having witnessed the direful and fatal effects of dysentery, the writer has undertaken, with some reluctance, to make public, some facts which he considers important, concerning it, as it appeared in Claiborne Parish, Louisiana, in the summers of 1854 and '55. Its prevalence in Claiborne Parish may be accounted for from local causes. The face of the country, although very uneven and hilly, abounds in swamps, especially that portion of the Parish where the disease raged with the most fearful violence. My observations were confined to that portion of the country bordering on the banks of Bayou d'Arbonne. This stream rises in the northwestern portion of Claiborne Parish, and flows in a southeasterly direction, until its confluence with the Bayou Cornelle, when it changes its course and flows away towards the east until it empties into the Ouachita River. When this stream is swelled by the spring rains, it is navigable with steamboats several miles above its confluence with the Bayou Cornelle, and with keel-boats many miles above this point. The banks of the Bayou D'Arbonne are very low, and on each side are extensive swamps, interspersed "now and then" with large marshes. The face of the country, immediately along the banks, is even, and the soil very rich and fertile, producing a heavy burden of cane, grass, weeds, and plants of great variety.

In the months of April and May, 1854, much rain fell and filled the swamps, and swelled their streams above the banks. The weather then changed and became uncommonly hot and dry; so it continued for a considerable length of time. The excessive heat and drought of the season which followed, partially or entirely dried up the swamps and marshes, and the putrid exhalations arising therefrom were very perceptible. Considering the circumstances that have been mentioned, it is by no means

difficult to account for the rise and progress of the dysentery in the Parish of Claiborne.

The following symptoms will leave no doubt upon the mind of the acute symptomatologist, as to the disease in question being the genuine dysentery, as is described by medical writers.

Symptoms.—Small and frequent stools, composed of bloody mucus, sometimes resembling the washings of raw meat, and frequently mixed with a large amount of mucus; severe griping pains in the bowels; violent and obstinate tenesmus; fever; nausea; vomiting; emaciation, and great prostration of strength. I will here take occasion to remark that, in no instance, have I observed the scybalæ as is mentioned by medical writers. This disease has been so often described, that it is deemed wholly unnecessary to enter into a more minute detail of the symptoms, as there was nothing peculiar in the general symptoms, more than are usually described by medical writers, only that the fever generally made its appearance first, followed up by all the dysenteric symptoms.

The pathology of dysentery has formed the subject of many an ingenious essay, and has been a fruitful source of difference of opinions among medical writers, and much remains to be said and done before it will be a settled question.

Aretæus, Galen, Celsus, Aurelianus, Cælius, and many others, their successors, supposed dysentery to be an ulceration of the intestines generally, without limiting it to any particular portion. Sydenham and Willis entertained near about the same opinion, but looked upon inflammation of the mucous membrane as being essential to the production of the disease. But the numerous dissections of Punardli, a Roman physician, proved that he had never found blood in the small, but always in the large intestines. Bonetus and Morgagni limited the disease to the small intestines. This error was committed, no doubt, from the fact that the greater number of their dissections of dysenteric subjects were cases of long standing. Cullen defined dysentery to be a contagious fever, with mucous or bloody stools, the natural fæces being, for the most part, retained, with griping and tenesmus. This erroneous definition has been a source of much unsuccessful practice in the treatment of this disease. The theory of inflammation of the mucous membrane of the intestinal canal, was the original link or proximate cause of the disease, met with many adherents, whilst at the same time it met with many dissenters. This idea Dr. Johnson combatted in his justly celebrated work on Tropical Climates, and I find him sustained by Dr. Armstrong in his admirable work on Typhus Fever. Dr. Armstrong says, "this pathological

view makes the concomitant inflammation rather a symptom or an effect of the general excitement, than the original cause of the dysentery, and a strict survey of facts will verify this opinion; for, in the first stage of oppression, when the surface is cool, so far from their being any unquestionable sign of inflammation, a diminution of arterial tone exists everywhere, and substantial evidences of inflammation only emerge with the excitement of the heart and arteries, which succeed the first stage of oppression. Dr. Johnson says, "after a long attention to dysentery, then, as it affected great numbers under my care, and also as it affected myself in person, I can confidently assert that two functions appeared to be constantly disordered from the beginning, and which were either soon produced, or were accompanied by other derangements. These were the functions of the skin and of the liver, or perspiration and biliary secretion."

Moseley and many other writers on dysentery, say that the disease is a fever turned in on the bowels from suppressed perspiration. Mr. Bampfield regarded the disease as little else than a mere inflammation of the bowels. Dr. Somers, an army physician among the troops of Portugal, advocated the same theory.

Congestion of the portal circle is maintained by some writers to be the cause of dysentery, and it was a favorite theory many years ago, with the late Professor Cooke, of Lexington, Ky. It is supposed from some unknown cause that the secretory vessels of the liver become torpid, and therefore, in consequence of this torpidity. a state of venous congestion is the result throughout the whole portal circle, and owing to the vascular turgescence of the mucous membrane, this stasis of blood finally brings about inflammation of the mucous coat of the bowels, and the result is, blood is thrown out to relieve the plethora. This popular theory gradually waned away, and fell into disreputc, but in the absence of a better one, it unfortuately has been revived of late, to explain the pathology of dysentery. But in all candor, if it be the correct theory, I must acknowledge that there are some things which I am unable to solve and comprehend.

The theory of most medical writers of the present day is, that dysentery is nothing more nor less than a simple inflammation of the mucous membranc, but not the whole of that long surface indiscriminately. So far as my observations go, I think this falls far short of being the correct theory. All of these theories are as ingenious as they have been popular, and for the most part they have been speculative, but time and observation have told upon them and are gradually blotting them out one by one. *Opinionum commenta delet dies naturæ judicia confirmat.*

It is an anatomical fact that the large intestines are well supplied with

blood-vessels, and that in those blood-vessels there is the utmost freedom of anastomosis between the arteries, and absence of valves in the veins. Also, there is a close relationship existing between the skin and mucous membrane lining the intestinal canal. I believe dysentery to be a lesion of secretion, and that lesion to be a perverted secretion, producing congestion. Let us imagine any cause checking the flow of blood to, and the secretion from the skin; naturally enough, there is a determination of blood to the interior of the body, and should there fail to be an early revulsive action, the consequence is, owing to the allied connection existing between the skin and mucous membrane, both in function, structure, and sympathy; it assumes the duty of helpmate, and receives the weight of perverted action. The morbid matter in making its exit through the mucous surface, acts as an irritant, the end of which is inflammation.

Prof. Linton, in his Outlines of Pathology, says: "That increased action in any organ or part leads to subsequent exhaustion or debility, is a well established fact; or, over-exertion of any organ induces active congestion, which passes often into inflammation."

In short, whatever is capable of driving or inviting arterial blood to a part, may become a cause of these pathological states. The major portion of the cases upon which I have been called to treat, of epidemic dysentery, the fever has usually made its appearance first, followed up afterwards by those of dysentery, and it is in this form of the disease that I have witnessed its most alarming and fatal effects. I believe that when dysentery is derived from any cause flowing from the atmosphere, that it will assume the remittent form. A dysentery having its origin from the general condition of the atmosphere differs from the intermitting and remitting fever only in the intestinal affection, and requires a similar treatment. "Of late I have been induced to look upon dysentery as a secondary affection, and fever as the primary cause.* A fever when generally and equally diffused throughout the system, though very violent, is accompanied, comparatively, with less danger, because it does not expend its force on any one organ, and is therefore less capable of producing disorganization and death in any particular part." Hence the greater malignity and danger of centripital forms of fever, or those determined to the interior parts of the body, when compared with centrifugal forms, or such as appear in a state of greater diffusion.

Lind, in his work on Hot Climates, says: "Fluxes and fevers are the distempers most fatal to Europeans on the Coast of Guinea, and the season of their appearance is during the rains, and for some time after

*Alibert, page 11, note.

they have ceased. The flux sometime appeared alone ; at other times attended the fever, most frequently followed it." He further says, " We indeed meet with few instances of epidemical malignant fever which are not in some cases accompanied with a flux." Alibert, on Intermittents, says, " At other times the febrile paroxysm is accompanied with a flux of a dysenteric character, that is, the matter discharged by the patient by stool consists of mucus mixed with blood. The discharges are attended by tenesmus and tormina the most excruciating." In the writings of Torti, we meet with an account of a malignant double tertian, accompanied with a constant evacuation of a bloody mucus, similar to that discharged in dysentery.

In the summers of 1854 and '55, dysentery pervaded the Parish of Claiborne, and a considerable number of dysenteric cases came under my care. The febrile symptoms usually run high several hours before the dysenteric symptoms set in, and continued throughout the attack with but little abatement. I could observe no remission at any time during twenty-four hours, except between one and four o'clock in the morning, when I could observe a slight remission in the febrile symptoms, at which time there would be an evident and distinct quiet of the system generally, whereupon the patient would drop into a short and quiet sleep, though only to be aroused towards the hour of morn with a renewal of all the symptoms of the previous day.

I have already drawn this communication out far beyond what I originally intended. Knowing that short and practical articles are more desirable than lengthy ones, I will at once proceed to the treatment of this disease in as brief a style as possible, and then draw it to a close.

Blood-letting may be advisable in extreme cases of plethora ; it is seldom admissible, and whenever I have desired to take blood, I have usually had my purposes answered by the application of two or three exhausted cupping-glasses over the abdomen ; but, generally, I have derived as much benefit from dry cups, and perhaps more. Aside from the subject of blood-letting in this disease, I have usually opened the treatment by administering some mild purgative, and a favorite one with me has been a combination of hydrag. cum creta and pulvis rhei ; assisted, if necessary, with the syrup of rhubarb. After the action of which, I prescribed the following mixture :

Receipe—Kreosote gtt. x; acetic acid, gtt. xx; morphiæ sulph. gr. ij; oleum sassafras, gtt. iv; water distilled, ʒi ; in teaspoonful doses every three hours, until the discharges were checked, and the bowels quieted.

If there was any visible remission in the febrile symptoms, (a subject

which I have before referred to) I usually anticipated that period with the administration of quinine. Poultices, I have used with much relief to the patient, seldom employing a blister. I have much confidence in the use of diaphoretics and diuretics; consequently, I prescribe the liberal use of uva ursi and buchu.

Such is the treatment I have followed, in passing through two epidemics, and success has crowned it in nearly every effort. I could enumerate case after case where this course has been followed, and success the result. I have seen and assisted in treating in all, more than two hundred cases of dysentery. In the October number of the *Nashville Journal of Medicine and Surgery*, (page 306,) there is a short article by J. W. Brown, on the use of kreosote in dysentery.

He says "It is not my object at this time to write an article on the nature, causes and treatment of this terrific malady, but to call the attention of your readers to the practice of Drs. McMath and Gilder, of Louisville, Arkansas. They tell me they have treated as many as three hundred cases, and in all proving perfectly satisfactory, under the use of kreosote." Their prescription, as is given by Dr. Brown, is the same as I have given it, except the oil of sassafras, which is an addition of my own, merely to cover the disagreeable smell and taste of the kreosote, which it effectually does. Now, if there is any honor due to the builder of this recipe, and its use advised in the treatment of dysentery (which there undoubtedly is), that honor is due to Professor A. P. Merrill, of the Memphis Medical College. I saw him use it both in dysentery and diarrhœa with the happiest effects. When I returned home, I informed my preceptor, Dr. Gilder, of the use of kreosote in dysentery, and we both passed through the epidemic of 1855 as above-mentioned. Drs. McMath and Gilder have since used it with unequaled success. Prof. Bowling, of the Nashville University, says, "We have seen parotitis often in typhoid dysentery, and we do not remember that any such case recovered."

I will state, that in 1854, I was called some distance from home to visit a family, three of whom were sick with dysentery. Two of them were beyond the reach of remedies, the other lingered for some time, when parotitis supervened. Suppuration came on after the usual course of inflammation, the gland discharged freely, and the patient gradually recovered.

Magnolia, Arkansas, November, 1857.

ART. IX.—*On the Mercurial Treatment of Dysentery:* By J. C. TRULY, M. D.

To the Editor of the New Orleans Medical and Surgical Journal:

THERE has been raging, (as did the Israelitish plagues), in this and some of the surrounding parishes, a disease which has taken off its hundreds, and baffled the skill of the scientific physician, as well as the strict attention of the faithful nurse, *i. e.*, dysentery in all its aggravated forms. Most usually it commences with a discharge of black, watery-looking bile, immediately followed with discharge of very liquified blood, and thirdly, by mucous discharges from the rectum, sometimes tinged with blood, sometimes with bile.

I feel myself under obligations to the intelligent Dr. Gammage, of Rusk, Texas, for his able writings on that subject, in the *New Orleans Medical and Surgical Journal*, which you so nobly edit. I have followed his treatment through, and must say that I have had unlimited success with the mercurial treatment from first to last, though I used it to a a greater extent than laid down by Dr. Gammage, because this disease seemed to require it. I have used blue mass to the extent of one hundred and fifty grains, within thirty-six hours, and during my whole course of practice this summer and autumn, have lost but one case, and that case I will point to you in its turn.

As was the case in the vicinity of Dr. G.'s locality, so it is here; that is, there has been a great diversity of treatment in the above-named disease. Mercury in all of its preparations was looked upon with horror and with disgust, as being poisonous to this disease. Though, thanks to the guardian of human life, that the prejudice against the use of this remedy is fast fading from the minds of the community at large, to the great consternation of a few who have been preaching it down from the very house tops.

I will now give a few illustrations of my practice. Mrs. Chambliss was attacked on the 17th of September. Her disease progressed until the 19th, when I was called; pulse 132, extreme tenderness of the abdominal viscera, tongue coated with a thick brownish coat in the middle, edges very red and fiery. Prescribed hydrag. cum cret., vi grs.; opium, 1 gr. in pill; give the same every two hours, until a copious discharge from the bowels takes place. She took xxxvi grs. mass, vi grs. opium before an operation occurred.

20th. Had had four actions from the pills; pulse about 100; skin soft, and not so much straining while at stool; stools less frequent. Directed ol. ricini $\frac{1}{4}\frac{3}{3}$, tr. opii. 15 drops after every discharge, to be alternated with the following pill: blue mass ii grs., quiniæ sulph. iv grs, morphiæ sulph. $\frac{1}{2}$ gr.

22d. Tongue slightly coated with a brown exudation; pulse about 120, (accelerated by the use of quinine); has been sweating finely for 24 hours. Continued the use of the oil and laudanum, with one of the pills three times per day; nothing to drink but sage or mint tea; diet, squirrel soup.

23d. Tongue cleaning off finely; pulse 90; complains of very little pain in going to stool; discharges bilious. Continued the oil with the addition of a little more laudanum; quiniæ sulph., every two hours iv grs.; morphiæ ¼ gr.

25th. All symptoms favorable; pulse 80; skin soft; scarcely any blood with the discharges; tongue clean. Directed port wine every three or four hours, and with a small dose of oil and laudanum three times per day. She recovered rapidly in a few days.

I had many other cases which were treated about the same way. Yet I have one case more to allude to before closing: Mrs. L., of a very weak constitution, was taken on the night of the 8th of September, with dysentery. I was called on the 10th, and found her extremely weak, with bloody, mucous discharges every half hour; very sore about the region of the abdomen; pulse 140; skin cold, clammy; very colliquative perspiration suffused her whole body, especially just after a discharge from her bowels; tongue very thickly covered with a dark brown exudation, edges very red. Ordered hot rocks to be applied to her back and feet. (She had taken, before I saw her, two or three doses of laudanum and camphor.) Ordered the following to be given every two hours: ℞. blue mass viii grs., opii. i gr., to be alternated with the following mixture: oleum ricini, one teaspoonful; tr. opii., ten drops; no drink but sage tea, and "nothing to eat."

12th. The blue mass had just commenced acting; had brought two dark and very fœted discharges; tongue about the same, only darker; pulse reduced to 115 per minute; skin warm and dry; complains of "burning up inside;" wants to fan herself all the time. Says "she will die for water;" no appetite; has not eat anything since she was taken; does not complain of so much soreness in the abdomen on going to stool; stools not so frequent. Continued blue mass, twice per day; continued oil and laudanum every hour.

14th. Much better to-day. Tongue clear; pulse 90; skin soft; has had dark discharges ever since I saw her last. Gave quinine iv grs., ipecac ½ gr., morphiæ sulph. ¼ gr. every two hours, with sage tea and a little squirrel soup. Continued oil and laudanum.

18th. Worse. Tongue smartly furred; pulse 120; skin cold; perspiration free and clammy; hands cold; discharges bloody and watery;

much pain in the abdomen. Gave more blue mass, viii grs. every two hours, with i gr. morphiæ. The medicine operated in twelve hours; tongue cleaned off, but was very red and dry. Gave hot salt water bath, and left her with directions to take the oil and laudanum frequently, and one teaspoonful of table salt twice per day; quinine v grs.; Dover's powder ii grs. every two hours.

21st. Extremely weak; has been discharging no blood for a day or two; operations very dark; complains of being "hot inside;" skin very cold; pulse weak and soft; no appetite; eyes very staring; so debilitated she can scarcely talk. Ordered port wine every fifteen minutes, and warm rocks to feet and back.

22d. Symptoms about like they were yesterday; got up to stool; had a dark discharge. They laid her back in bed. She asked for some soup and wine; gave her some; she said it tasted good. She asked to be turned over, and died in five minutes.

It would not be amiss for me to state that about two weeks previous to her attack, she was very much reduced by an attack of bilious fever, from which she had not regained her strength; consequently she sunk from extreme debility after an abatement of the dysenteric symptoms.

I shall now say a few words relative to the contagion of this dysentery. It is almost universally allowed in this community, even among the physicians, that this disease is undoubtedly contagious.

It has raged in such a horrid form, that some of the very intelligent physicians have almost come to the conclusion that it is not really the dysentery, but some other malady which is more destructive in its course. Some have been pleased to term it, "Piney woods cholera;" some "The plague of 1857;" some one thing and some another; but I consider these surmises groundless, both as to contagiousness and its not being dysentery, and also about its being "Piney woods cholera." If such is the case, it is far different from the "Asiatic cholera," as scarcely any of its symptoms resemble the latter; having no severe spasmodic contractions and crampings. I never saw a case of cholera without these symptoms. There are some gripings on going to stool, but no vomiting (scarcely ever), and no muscular contractions.

So far as the contagion is concerned, I consider it a perfect fallacy, strictly speaking. I believe certain locations and especially those nearest swamps, in undulated lands, are more subjected to the disease, because there is more malaria generated there. I have noticed during this summer and fall, that those persons living near a low field, planted in cotton, have suffered more from the disease. Cotton on the low lands (or, as it called here, bottom lands), this year has grown very luxuriantly

and owing to so much rain, has shed its foliage to a considerable extent. The earth is kept very damp, from the cottons continually shading it; consequently, there is a continued decomposition of this shedding foliage taking place, generating this miasma, and a great number of people are living down in the bottoms, with cotton planted all around their houses. And who, under these conditions, could expect anything but sickness, and that of the worst kinds? I have been with this disease from the first of July until the middle of October, up late and early over the sick bed, and have not restrained myself in any particular, and have never had a symptom of the disease, and I have often seen one or two members of a family have it, while the others would miss it. Even this, I think, is enough to convince any reasonable man of its non-contagiousness. I think that contagion can be applied to no other disease than those diseases which can be propagated from one to another by touch, or juxtaposition. This disease could safely be called an infectious disease, because it is undoubtedly contracted from a vitiated atmosphere; from an air which is universally contaminated with malaria, produced from a vegetable or animal matter.

Dr. Hooper says: "The Walcheren miasmata extended their pestilential influence to vessels riding at anchor fully a quarter of a mile from the shore." I have known the same to be communicated from a low marshy swamp to the distance of more than a mile, where there was no local cause nearer for its generation; and the warm summer south winds would convey it from that distance to a farmer's residence, where it was almost impossible for it to have been generated nearer; at least, there was no local cause for its formation. I give it as my opinion that it can be conveyed for five miles on a cloudy day, when there is a smart wind from its origin. I do not think it easily amalgamates with the pure atmospheric air. This is my opinion relative to the supposed contagiousness of dysentery.

Let me say to those who are opposed to the mercurial treatment in this disease, that they have never given it a fair trial. We all know that there is no specific in medicine; but we know the effects we want produced, and if a little dose does not produce the desired effect, a large one may (if the absorbents are not so impaired as not to receive the drug.) I will just say, then, if six grains of blue mass does not have the desired effect, give twelve, eighteen, twenty, forty, etc., until you get a bilious discharge from the bowels, and don't cry down such an excellent, yet untried remedy. During my practice in this disease, I have had to fight against the winds of superstition and the waves of an almost maniacal community, who are crying down the physician's calomel.

I now bring my scattered, feeble and very imperfect remarks to a close, and, if there be any yet out of the "ark of safety," still adhering to their tenacious principles of no mercury in dysentery, all I have to say is, God help them, and turn them from their darkness into the light of a better knowledge.

Hamilton, Shelby County, Texas.

ART. X.—*Remarks on Imperforate Anus:* By JAMES JONES, M. D., Co-Editor.

I AM disposed to believe that malformations of the intestinal canal prevail more frequently than many generally concede, and that they produce much unnecessary suffering, and often terminate fatally, through the injudicious and improper management, and the ignorance and neglect of midwives and monthly nurses, to whose exclusive care infants at birth are usually confided. The dread of trismus nascentium is so great in the South, reliance on the prophylactic influence of early purgation is so universal, that it would be almost useless to repudiate the inevitable and nauseating draughts in which the first efforts of deglutition are invariably exercised.

The cases to which I am about to invite attention were not successful, nor do I publish them as peculiarly interesting or extraordinary. The object is, in the first place, to demonstrate that in obstruction of the bowels from malformations, as from other causes, cathartics are both useless and injurious; and in the second, to establish not only the inadmissibility of all food in similar cases, but this interesting fact also, that, under absolute diet life may be protracted for a number of days after birth, without the sudden and alarming prostration we are generally lead to anticipate. A larger proportion of the diseases of the earliest periods of life depends on excessive and improper, than on defective nourishment, and I am confident that among the better classes, the antagonistic opinions promulgated by Gooch, have proved more mischievous than useful.

The first case in my own practice occurred in February, 1846. I delivered a lady of a fine boy on the morning of the 13th, and as she lived two and a half miles from my residence, left her in charge of a very experienced nurse, who now practises with some reputation as a midwife. Next day she informed me that the child had as yet no passage from the

bowels, notwithstanding the exhibition of olive and of eastor oil, and also of two enemata. The ehild was very restless and fretful, vomited and had a very tense belly. I took the syringe whieh had a long ivory nozzle, and nsing it as a sound for the examination of the anus and reetum, found to my regret that it would only penetrate about half an ineh, and that the anus was imperforate. I immediately made an incision with my lancet to the depth of half an inch more, and finding by the introdnetion of a eataraet needle an ineh and a half, that neither air nor liquid of any kind made its appearanee, I ealled Dr. Stone into eonsultation, who disapproved of further ineisions, and dilated that already made to the depth of an ineh and a half unsueeessfully. The abdomen beeame more tense, the child gave evidenees of great suffering and died on the next day.

The post-mortem was made on the 17th, by my friend, Dr. Lemonnier. The intestines were all greatly distended, and the surfaee of the peritoneum eovered with eoagulable lymph. The lower portion of the eolon, whieh was the part most enlarged, terminated at the promontory of the saernm in a eul-de-sae, filled with meeonium. There was a eomplete absenee of the reetum, and the end of the eolon had two little punetures made by the needle, from whieh, apparently, nothing had eseaped. If they had been larger, the eontents would have been emptied into the eavity of the pelvis.

My next ease was in eonsultation with my friend, Dr. Riehard Bein, on the 11th of Angnst, 1856. The little boy was born on the 9th, presented the nsnal symptoms, and Dr. B. made an attempt, in eompany with Dr. Hunt, to cross the river during the gale of August 10th, to relieve him by an operation ; but after several honrs' drifting about, they returned home without being able to get aeross. Dr. H. being nnwell, I went over next day, with Dr. Bein. We found the child in great distress, the abdomen very tumid and renitent, with eonstant vomiting of a yellowish thin fluid. On examination, the anus being imperforate, we proeeeded at onee to open it with a bistoury, whieh, after being earefnlly introdueed to the depth of an ineh and a quarter, gave issue to a large quantity of meeonium. The aeenmulated eatharties prodneed so mueh purgation that Dr. B. was eompelled, during the evening, to administer an anodyne mixtnre to eheek their debilitating operation.

On the 12th, we introdueed a eaoutehone tnbe to keep the passage open. The abdomen was greatly redueed, the child sueked and swallowed and we hoped that it would eontinne to improve.

On the 19th, I saw it again. The operations had eeased; the abdomen was again hard, althongh not so mueh inflated. The opening having eontraeted, we dilated it by introdneing the dressing forceps and gently

spreading the blades ; the orifice was again kept dilated with a gum catheter. The general appearance of the child was wretched ; it moaned and whined continually, never slept, rejected everything, and exhibited signs of peritoneal inflammation of a fatal tendency. By the use of camphorated mercurial ointment to the abdomen, emollient enemata and two or three minute doses of calomel, it revived slightly and had a few operations, urinating as usual.

On the 23d, I saw it again by the request of Dr. B.; and although evidently sinking, it lingered until the 25th.

On the 26th, the body was opened by Dr. Bein, myself, and Dr. F. Poland, at that time a student of medicine. The intestines were somewhat distended, and were also generally adherent by organized bands of lymph. Internally, there was much disseminated inflammation. The rectum terminated in a pouch, which being doubtlessly softened in its attachments by inflammation, pealed off entire from the pelvic surface of the perineum. I have it now in alcohol. The friends were waiting to bury the infant, and we did not examine into the state of the sphincter or of the other muscles.

It was very evident that in this case enteritis and peritonitis were the causes of death. The only question being how they should have occurred, when the operation had been successfully performed at so early a period.

My next case was yet more instructive, reflecting, in my opinion, not only much light on the sources of the severe symptoms occurring at such an early period in the majority of these conditions, but, also, establishing how long an infant can live without food. On the 16th of February, I was called to see an infant, in a family generally attended by me, which was born on the 14th. I found that it had never emptied the bowels since its birth; that its abdomen was tense, particularly on the right side, and that it suffered with violent paroxysms of pain, somewhat relieved by vomiting. I had just lost a man in the Hospital, with obstruction of the bowels, and I found the fluid brought up by this infant presenting the same gamboge and fæcal appearance as that vomited by him. I immediately explored the anus and rectum. A gum catheter passed up three inches without any difficulty. Thinking that there might be a loop of intestine included in the ligature of the umbilical chord, I opened the dressings and found everything right. The child had taken all of the usual cathartics in full doses, which, after great distress, it threw up. To these had been added the use of enemata. Nothing was to be done. It was a well developed and otherwise healthy infant, and its cries so distressing, that I gave it two or three drops of syrup of morphia occasionally, to relieve the suffering which were the evident cause of violent convulsions.

I discovered soon that the paroxysms of pain were the result of the ingesta, and after they ceased giving it anything more than a few drops of water, it suffered apparently very little more. The vomitings became more rare, and it gradually expired on the 26th, from inanition on the thirteenth day.

On the post-mortem examination, made by my student, Dr. Holloway, now of Mississippi, we found no peritoneal inflammation. The most remarkable appearance was that of a portion of the ileum, which, from its form and size, we at first took for the stomach. It was a pyriform sack on the right side of the abdomen, containing several ounces of a dark green fluid, and was the termination of the upper portion of the intestinal canal. Immediately below, also closed at its upper end, recommenced the ileum, no larger than a quill, about two inches above the caput coli, which, like the remaining portions of the tube down to the anus, was exceedingly small and empty.

Considering the small quantity of fluids received, the child urinated very well.

On the morning of November 17th, 1857, I was called, before day, to see Mrs. R$_{**}$, Franklin street, in labor. I found, on my arrival, that she had been delivered several hours previously of one child, and that she was much exhausted by her efforts to expel another. I found the membranes of the second ovum unruptured, and felt the hand and arm presenting through them. I immediately ruptured the bag, pushed up the right hand and arm, brought down the head, and in three pains it was delivered. I felt the chord prolapsed and pulseless as I pushed up the hand. The child was born in five or six minutes after I commenced to interfere, but it was asphyxiated, without the slightest pulsation about the heart, and nothing could revive it.

On the 18th, in the morning, the surviving child had passed nothing from the bowels, although unfortunately, it had taken considerable quantities of castor oil, olive oil, and all of the usual teas children are compelled to swallow. Its belly was already tumid and tense, the enlarged veins showing the intestinal obstruction. On returning, several hours afterwards, with the proper instruments, I found the child worse; it was very restless, evidently in pain, and had rejected, latterly, everything put into the stomach. It had never attempted to suck. Dr. Stone being in the vicinity, we examined the case together, and found the anus completely imperforate. A small incision made unsuccessfully with the bistoury, was repeated afterwards with the lancet, which being driven to the depth of more than an inch, was followed by a copious eva-

cuation of meconium and flatus ; the belly subsided immediately ; the vomiting ceased, and the child did not give much indication of pain; but refused to suck; swallowed with difficulty, and continued to decline.

On the 20th, a gum catheter was introduced, after extending the puncture, more meconium followed. In the evening of the same day, the child died.

On the morning of the 22d, M. Capdevielle and my son, made a post-mortem examination and brought me the intestines. The peritoneum was inflamed ; the intestines somewhat distended, and the colon very much inflamed, and filled with a mixture of meconium and of blood, which probably came from the incision I made last, in enlarging the opening. The rectum terminated, as in the case with Dr. Bein, in a pouch, on the pelvic side of the perinæum. The incision was through cellular membrane, and there was no appearance of a sphincter.

It is much to be regretted that in Louisiana practice, so much latitude has been generally allowed to midwives and monthly nurses, that a great part of the medicine given to infants within the month, is often administered without the advice of the physician, even when he is in attendance. The popular idea that trismus is the necessary result of retained meconium drives everybody to purgation, and while we may reasonably entertain doubts of its expediency in the mass of cases, there is no doubt that in nine-tenths of the fatal cases of imperforate anus, it leads whenever the operation is rather late, to fatal inflammation of the intestinal tube, by the direct action of the agent employed, and the distention from the accumulation of morbid and irritating secretions. It is also a misfortune, as I have mentioned in the first case, that from the enema passing by the piston into the head of the cylinder, ignorant nurses tell you that they have administered several injections, when not a drop has passed from the end of the pipe.

The operation should always be performed immediately after diagnosing the nature of the obstruction. In conversation with Dr. W. B. Wood, of the Fourth District, one of our most intelligent physicians, he informs me that in a hopeless case of imperforate anus, lately occurring in his practice, the obstruction having been early suspected, the child took no medicine and no nourishment, beyond a little water and sugar, did not appear to suffer, and lived eight days before it yielded to the effects of inanition.

ART. XI.—*Insane Asylum of the State of Louisiana, at Jackson:* By DR. STANFORD CHAILLÉ.

IN March, 1847, our Legislature finally passed "an act to establish an Insane Asylum in the State of Louisiana." Since that time more than one hundred and twenty-five thousand dollars have been appropriated by this State, to the laudable purpose of establishing an asylum, which should be a fit abode not only for the indigent insane, within its own limits, but also for all from every land, who, while above actual want, were yet the victims of an affliction, which the ancients, in their horror, forcibly designated the "wrath of God."

Additional appropriations will be required to complete the work so well begun; although even now sufficient has been done to enable our asylum to compare most favorably with similar institutions. During the past eight years, hundreds of homeless outcasts have found a comfortable abode, and permanent relief in its walls, and its doors are now thrown open to all who need its resources.

Too long have we been dependent upon Northern cities for many things, besides doctors and asylums. Whilst the former long since ceased to command a premium, it affords us unfeigned pleasure to be enabled to present such a report of the Jackson Asylum, as will soon cause our people to dispense with seeking relief for their insane friends, beyond the limits of Louisiana.

Thus far the intelligent public have manifested but little interest in this home institution, and even the medical profession is singularly unacquainted with its past history, present condition or anything reliable concerning it. Surely it behooves at least every physician in this State, whose duty may bear him at any moment to the maniac's side, to know whether an asylum within our own borders is now prepared to offer means of relief, far more promising of success, than he can by any possibility afford. And if this asylum be so prepared, it becomes the duty of the medical man, and of every good citizen, to be so acquainted with the fact, that they may conscientiously aid and support the cause, to which the State has wisely supplied a portion of its resources.

The information contained in this article has been diligently sought for from every *reliable* source, and is intended for the benefit not only of the medical profession, but of all interested in the insane; whether from that true benevolence of the heart, which sympathizes with every human suffering, or from that charity which, beginning at home, finds there some unfortunate object of affliction, whose wretched condition it knows not how to alleviate.

Here, by way of preface, allow me to express my obligations to Mrs.

Caroline Bowers, of the Poydras Female Asylum, and to Dr. Barkdull, Physician and Superintendent of the Insane Asylum, for the interesting communications which they have so kindly forwarded for my use.

The pretty village of Jackson is situated in the parish of East Feliciana, and is about thirteen miles from Bayou Sara, the nearest port on the Mississippi River. It is about thirty miles from the capital of the State, (Baton Rouge), from whence a public stage makes a daily trip. This, though the cheaper mode of travelling to Jackson, occupies an entire day, and is by no means so agreeable as the route from Bayou Sara, where a private conveyance can always be procured from the stables, at comparatively small price. The ride from Bayou Sara to Jackson occupies less than three hours, through beautiful woodland scenery, and over a most excellent road.

In the first report of the Board of Administrators, dated January, 1848, we read, "The land on which the Asylum is located, is within a convenient distance of the business part of Jackson, and at all times of an easy access to the same, but separated from the noise and bustle of the village, by a valley, and small stream, which renders it sufficiently secluded to protect the inmates from the annoying gaze of the idle and curious." There are about one hundred and fifty acres of land owned by the Asylum, one hundred well timbered, and the balance enclosed for the use of the patients. "The Board of Administrators consider themselves remarkably fortunate in securing a site so well adapted for all the purposes of an institution of the kind. Because of its acknowledged salubrity of climate, and central position, (being within a convenient distance of the capital of the State, and but a few miles from the great river which is the common thoroughfare of the country), and farther, because it possesses capabilities as regards landscape beauties unsurpassed by any other location of the State. The situation comprises an elevated plain covered with forest trees, and evergreen shrubs, among the former of which are many noble specimens of the glorious Magnolia, towering aloft in all the beauty of their native grandeur, whilst the smaller growth, consisting of a variety of fruit and flowering trees, of a diversity of form and hue, imparting a garden-like aspect to the place, and verifying, in fact, its familiar and appropriate appellation of '*Flowery Hill*.' The buildings occupying an elevated position, command an extensive view of the surrounding country. The field of vision, embraces within the compass of a mile, on the north and west, the quiet and pleasant village of Jackson, including the beautiful structures, and picturesque location of 'Centenary College,' and the various mansions in the neighborhood. On the south and east, we have hill and dale, wood and lawn, interspersed with rural cottages, and culti-

vated fields in their most agreeable combinations. The whole forming a landscape view seldom to be enjoyed in this section of the Union; and which must be a source of unfailing interest to a large portion of the inmates, as such scenes are sure to please the eye, and relieve the sad musings of a melancholy mind."

The buildings of the Asylum consist of two wings, each "ninety-four feet in length, and forty-eight feet in depth, three stories high, with a ten foot gallery on the south and west sides of each story. There are, in the first and second stories, nineteen rooms each, thirty single apartments, and eight large ones, the latter to be used for day rooms, or parlors, offices, etc. In the third story there are six large rooms to be used for associated dormitories, or other purposes, making in all forty-four apartments, besides a passage or hall, twelve feet wide, running the whole length of the building, in each story." These wings are the one for male, and the other for female "State patients." The principal structure, the "central building," which stands between these wings, "is four stories high, above the basement, and measures, exclusive of the projections that connect it with the wings, sixty-three feet front, and one hundred and twenty feet in depth. It embraces a *mile* of departments, for the Superintendent and his family, office and reception rooms, parlors, a commodious chapel, six dining rooms, dormitories, etc., sufficient to accommodate any number of 'pay patients,' likely to seek admission into this institution. The basement will afford secure storage rooms for provisions, and space for the construction of hot air furnaces, and such other fixtures as may be

needed. The wings, and the projections connecting them with the central building are three stories high, and the combined structure has a front of three hundred and fourteen feet. The edifice in question, so far as designed for the use of patients, is exclusively for those who pay, and it will include apartments and other accommodations graduated to their means and habits of life, from the frugal in mediocre circumstances to the luxurious of ample pecuniary resources; and connected with it is a separate enclosure of several acres in extent, which it is designed to embellish with walks, shrubbery, and other attractive objects."

The first class rooms are on the second floor, in the front of the building; and the accommodations, for neatness, size, and comfort, are not inferior to the best city hotels. A commodious dressing-room is attached to each, and all of the first class apartments, further possess the luxurious advantage of being supplied with bathing-rooms and shower-baths, with abundance of warm and cold water at all seasons. The second, third and fourth classes of rooms, which are situated in different stories of the same building, diminish in size, convenience, and style of furniture, in accordance with the rates charged. The best rooms are for $50 a month, while the poorest are for only $12.50 a month. Semi-annual pre-payments are required.

In addition to the buildings already described, there are two others for the temporary confinement of those, whose violent and unmanageable condition imperatively require it. These buildings are intended one for each sex; they are detached from the main buildings, and sufficiently distant to prevent the noise and disturbance of the raving maniac, from interfering with the welfare and repose of others. They are of brick, like all the other permanent buildings, some ninety feet in length, and contain each nine rooms, in which two or more patients may be conveniently confined. They are very secure, kept perfectly clean, and are comfortable places of lodging, being well ventilated, and warmed with hot air. An attendant is kept on guard day and night, to protect them from self-injury, and to administer to their wants.

Such are the permanent structures now completed; the consideration of the improvements still to be made is deferred to a subsequent portion of this article. In the construction of these buildings, the original design contemplated has been faithfully carried into execution. This design is thus detailed by the first Board of of Administrators: "The plan of buildings embraces, as far as circumstances will permit, all the improvements recently introduced into modern institutions for the insane. The entire plan contemplates the erection of a structure amply sufficient to accomodate with comfort and security about two hundred and fifty

patients, with the usual number of officers, attendants, etc. A number which is considered by the best authorities on the subject, as the maximum that should be collected in a single establishment of the kind.

"In consequence of the latitude and climate of Louisiana, the general construction of our building when completed will differ somewhat from the more Northern institutions for the insane—from the fact that there are but few days in the year in which fire for the purpose of warming is necessary, and ventilation in our climate being regarded as of more importance than heat. Hence, we have introduced into our design a greater number of windows, doors, galleries and spacious corridors, than are usually found in similar institutions in colder climates.

"The building being elevated, the entire plan is so arranged as to have the benefit of air from any point of the compass, thereby producing a free and natural ventilation throughout every part of the establishment. In the erection of this edifice it is our intention (and the location certainly favors the design) to avoid as far as possible everything prison-like or repulsive in its appearance, and to give the whole design that airy and cheerful aspect which is known to have a beneficial influence on the mind of the insane." An eye-witness writes, "neither windows nor doors are *disgraced* by the prison-like appearance of iron bars, or iron works of any description."

Connected with the three principal buildings are distinct yards, separately enclosed—one for the "pay patients," and one for each building of the male and female "State patients." They are, each, of three or four acres extent, covered with the natural growth of forest trees—magnolia, pine, oak—and are a delightful resort at all seasons of the year; and, in the summer, especially cool and shady.

Besides these yards, there are various other amusements and means of occupying the minds of the inmates. In the chapel, some one of the ministers of Jackson preach to this afflicted congregation every Sabbath. The chapel is easily converted into a room of entertainment, where the patients are sometimes allowed music and dancing.

Dr. Barkdull writes, "We do not require the pay patients to work, but very frequently they volunteer to do light work, preferring to be engaged rather than unemployed. But those who are supported at the expense of the State, are required (if it can be done without fear of injury) to do such light work as they can, from four to six hours each day; during the season for planting, the men are engaged in cultivating the various vegetables of this climate. We also find considerable employment for another class during the winter season, in chopping cord-wood. As we keep up during most of the winter some twenty-five fires,

it requires a large amount of wood for this purpose, all of which we get from the Asylum lands. Others are employed in getting up water to supply daily the kitchens, wash-house, etc. In the tailor's shop, female sewing-room, wash-house, etc., a number of other patients are kept employed, to the decided advantage of the State, and at the same time, to their own benefit, by giving them moderate exercise, and keeping their minds employed, thus calling them away from the various subjects upon which they are concentrated."

The Asylum was first thrown open for the reception of patients, in December, 1848, just nine years ago. Since that time there have been more than six hundred admissions. The average number of inmates has been, for several past years, over a hundred. The annual appropriations of the Legislature, for the support of this Institution, have been about $20,000.

"There are in the Asylum at the present time 124 patients; of these there are 9 male and 3 female 'pay patients.' The 61 males and 51 females remaining are 'State patients.' Among the 'State patients' are 8 free persons of color. *Two-thirds*, at least, of the inmates are of foreign birth, principally Irish and German, and these are mostly brought from the city of New Orleans. We also have quite a number of 'Creole French,' of the poorer class.

The United States Census of 1850, assigns to Louisiana, a total population of 517,762—whites, 255,491; free colored, 17,462; slaves, 244,809; total insane, 200—whites 144, free colored 11, slaves 45. By this estimate, we have one insane free colored man to about every 1600 of that class, one insane white man to about every 1800, and one insane slave to about every 5500 slaves. As appertaining to this subject, I will quote Dr. Barkdull's remarks, and may mention *en passant*, that he is a native of a Free State: "I informed you that we had eight free negroes here, as 'State Patients,' and from this you would infer that we had no slaves in this Institution. If it can be made to appear, therefore, that it is exceedingly seldom that our slaves ever become insane, I think you will agree with me that this fact is a striking commentary on the *pseudo-philanthropy* of some of our Northern brethren. Let us inquire into this matter a little; perhaps, after all, they do become insane as well as other persons, and in the same proportion, and their masters are not prompted by the ordinary instincts of humanity, or disregarding their own interests, they are not willing to incur the expense of $150 per annum, in having them provided with the means most likely to restore them to a state of sanity. I am quite sure, that any one, knowing the little value our planters place upon such a trifling sum as that, would never charge them with

being in the least influenced by such insignificant considerations. But the truth is just here, and it cannot be got around, that their great exemption from insanity is due to their situation, the protection the law guarantees to them, the restraint of a mild state of servitude, the freedom from all anxiety respecting their present and future wants, the withholding (in a great degree) of all spirituous and drugged liquors, and all other forms of excess into which the free negroes plunge in this and all other countries, to the utter ruin of mind, body, and estate. These are some of the reasons why the slave enjoys almost an entire immunity from all forms of insanity. As far as my knowledge extends in the surrounding parishes, (and it is the experience of other physicians of extensive practice,) I have never seen a single case of insanity in this State, or Mississippi, where I practised medicine for several years. But, on the contrary, it is also my experience, that free negroes, from the before mentioned and other causes unnecessary to detail, are peculiarly predisposed to insanity."

As a further commentary upon the pseudo-philanthropy alluded to, I append an extract from a report of Dr. Athon, of the Indiana Asylum, to the Legislature of that State. If not misinformed, his remarks are applicable to other Free States besides his own. Dr. A. says: "in our report of 1854, we called your attention to the expediency of making some provision for admitting and treating insane colored people. The State does not recognize the negro as a citizen, and in our opinion, he is not entitled to treatment in this Institution, under the law; but, notwithstanding this prohibition, he is a human being, and suffers alike in mind and body with his white neighbor; and, as a humane and liberal people, we should make an effort to extend the munificence of the State to provide for treating a just proportion of colored insane. We should have authority to admit them; as the law now exists on the statute books, their admission to this Institution, for treatment, is authorized by nothing but the 'higher law' of mercy, exercised entirely upon the responsibility of the Superintendent.

"In *the South*, there exists no prejudice of consequence among the people on account of associating blacks with whites, but here the prejudice is very perceptible, and while this exists, that classification, which is necessary to recovery, cannot be insisted upon."

A Southern man cannot but feel indignant when he looks on this picture and then upon that other, where so often he is the object of wholesale vituperations.

The care of our Asylum is entrusted to a Board of five Administrators, appointed by the Governor, to hold office for two years. The powers and duties of this Board are thus detailed by the law: "The Board shall

have power to make all rules and regulations for their own government, not contrary to law; to make all necessary contracts; and they shall further have the right to accept any donation or legacy in the name of the Asylum, and for its use; to sue and be sued; plead and be impleaded in all actions appertaining to the Asylum. They shall have power to elect a physician, a general superintendent, and a matron, to be employed in the service of the Asylum, with such salaries as may be determined by the Board. At every regular meeting, (which is once a month) the Board shall appoint two of its members, whose duty it shall be to visit said Asylum at least once a week, for the purpose of ascertaining the manner in which the regulations are complied with, and at each monthly meeting to report the condition of the Asylum. The Board shall furnish the Legislature, on the second Monday of each session, a detailed statement of the annual receipts and expenditures of said Asylum; a statement of the names of the insane persons in the Asylum; of the number and names of those admitted; of those deceased, and of those cured and discharged during the current year." They appoint from their own number, their President and Treasurer, and fill vacancies occuring in the Board. To the Treasurer the law assigns the duties usually incumbent on that officer.

The present able and intelligent Board was appointed in the spring of 1857, its members are—John A. Merryman, President, Bayon Sara; Chas. McVea, Treasurer, Jackson; Alfred Hazard, Secretary, Jackson; J. W. Taylor, Jackson; F. V. D. Hagaman, Jackson. Mr. C. N. Gibbons is the "Superintendent of Building," and the Board have appointed to the important office of "Physician and Superintendent" of the insane, Dr. J. D. Barkdull.

The Administrators require, by their regulations, that this officer be a well-educated physician, competent to treat, either medically or surgically, any of the inmates whose condition may require his skill. Power is given him to employ all needful attendants, and, for good cause, to discharge them. He is required to confine his professional labors entirely to the Asylum, and not to absent himself therefrom, for more than three successive hours. He is further required to keep a "register," in which he is to record detailed statistics of his patients, as well as a "prescription-book" to enter his daily medical treatment, with its results, etc. He must, every month, make a written report to the Board of the Asylum's condititon, and annually a full and detailed account of the Asylum for the year, as also for all the time the institution may have been in operation. Such are the important services required of this responsible officer.

To the Steward and Matron, the Board have also assigned numerous and appropriate duties.

In regard to the admission of patients, the law of the State is thus written:

"Whenever it shall be made known to the Judge of the District, by the petition and oath of any individual, that any lunatic or insane person, within his district ought to be sent to, or confined in the Insane Asylum of this State, it shall be the duty of the said District Judge, to issue a warrant to bring before him, in chambers, said lunatic or insane person, and after proper inquiry into all the facts and circumstances of the case: If, in his opinion, he ought to be sent to, or confined in said Insane Asylum, he shall make out his warrant to the Sheriff of the parish, commanding him to convey the lunatic or insane person to the Insane Asylum, for which duty the Sheriff shall have the right to demand the same fees as are now allowed by law for the conveyance of convicts to the Penitentiary of the State, which shall be paid out of the parish treasury, upon the order of the District Judge, and likewise, all other expenses previously incurred in bringing said insane person before the District Judge.

"The Board of Administrators shall have authority to receive insane persons not sent to the Asylum by a District Judge, on such terms and conditions as they may deem fit to adopt; and money so received shall be applied to the support of the institution.

All persons received in the Asylum as insane, shall be charged at a rate not less than ten dollars a month, unless the Police Jury of the parish from whence the insane person came, a Municipal Council, if from a city or town, or Clerk of the Court, shall certify that said person is in indigent circumstances."

And, "whenever any person arrested to answer for any crime or misdemeanor, before any Court of this State, shall be acquitted thereof by the jury, or shall not be indicted by the Grand Jury, by reason of the insanity or mental derangement of such person, and the discharge and going at large of such person shall be deemed by the Court to be dangerous to the safety of the citizens or the peace of the State, the Court is authorized and empowered to commit such person to the State Insane Hospital, or any similar institution in any parish within the jurisdiction of the court, there to be detained until he be restored to his right mind, or otherwise delivered by due course of law."

Such are the regulations for the State at large. They are sufficiently explicit, without farther comment or explanation.

For the benefit of those interested in the provision made by our municipal regulations for the welfare of the indigent lunatics in New Orleans, I beg leave to state that we have here in the city, adjoining the State

Prison, a part of a building which is designated by the horrible misnomer of a "Temporary Asylum for the Indigent Insane." Call it a lock-up, calaboose or man-kennel, if it so please you, for surely no benevolent lexicographer could so outrage humanity and the English language, as to justify our city fathers in terming this place an *Asylum*. Our "City Physicians" have each in their turn endeavored to blot out this disgrace to the city, but thus far their exertions have only served to verify the old adage, "Corporations have no souls." Dr. Deléry, the present City Physician, and an honored confrère, has kindly furnished a communication upon this subject, which, though it has lost some of the strength of the Doctor's native French, in the process of translation, may still prove of general interest.

Notes furnished by Dr. C. Deléry, City Physician, upon the Temporary Insane Asylum in New Orleans.

When Dr. Deléry entered upon his duties, December 1st, 1856, the Asylum contained fifty-six persons. One of them had been there for a year; others for several months. The insane were huddled together in the cells, where they often fought each other at night, and several came forth in the morning with their heads barbarously bruised with the blows they had received. Dr. Deléry has addressed several petitions to the Common Council, to inform them of the abuses which reigned in the Asylum.

Upon the entry of his duties, a lunatic was compelled to undergo a detention of sixty days, before he could be conveyed to Jackson. Upon Dr. Deléry's demand, the Council, some three or four weeks since, adopted a resolution authorizing the City Physician to demand, whenever he should judge proper, the conveyance to the Asylum at Jackson, of every lunatic sent to the Temporary Asylum, without the delay which heretofore they had been compelled to undergo at the said Temporary Asylum.

The City Physician complains that lunatics, beggars, and persons affected with delirium tremens only, are all sent, without distinction, to the Temporary Asylum. He deems it a violation of the laws of humanity, thus to confound two classes of persons, separated by the abyss of insanity.

He deems it of the highest importance to expedite as much as possible the conveyance to the Jackson Asylum, of the insane sent to the Temporary Asylum; for the hygienic condition of the Temporary Asylum is entirely unsuitable to the treatment of insanity, whilst these same hygienic conditions, at Jackson, offer the best advantages, besides that this last establishment is perfectly prepared for the treatment of this sad affliction.

The City Physician further thinks that there should be another asylum, similar to the one at Jackson, near the city, and for the following reasons:

1st. Since most of the insane are furnished by the city, the proximity of the place would enable their relations to visit them easily and more frequently.

2d. The expense would be much diminished, since the transportation of the insane from the city to Jackson, is very expensive.

3d. It would be a source of emulation to the Physician of the Jackson Asylum, who, instead of being isolated as now, and abandoned to his own solitary exertions, would find himself surrounded by confrères, with whom he would be enabled to exchange the lights of experience.

In the present state of things, an individual upon being sent to the Temporary Asylum, is submitted to the examination of the physician of this establishment (the City Physician), who retains him until he has acquired accurate information of his mental condition. He is sent, in virtue of the order of a Recorder, which order is issued upon the affidavit of any one.

If the individual is attacked with delirium tremens only, the doctor retains him, and only restores him to liberty, when he has perfectly recovered his senses. If he finds that the individual is afflicted with mental alienation, he fills up one of the blanks furnished by the City Attorney, and sends it to the Judge of the First District Court. The Judge then fixes a day for the examination of those designated as insane; the City Physician is summoned, is put upon oath, and swears that his affidavit (contained in the above-mentioned blank form) is true. After this, the Judge orders the Sheriff to have these insane (who have thus passed through the forms of the law) sent to the Asylum at Jackson.

This is a subject worthy of the attention of our modern engines of progress and reform, the daily newspapers, and to them I must leave it, after mentioning that, whilst on a visit to this place, in Dr. D.'s company, a poor devil, imbecile and wretched, was pointed out, who had had two of his fingers so mutilated, by a raving maniac, with whom he had been confined in the same cell during the night, that the Doctor was forced to amputate them. Let me also add what Dr. Barkdull writes on this subject: "The opinion I have formed respecting *that 'prison,'* has not been derived from anything I have gathered from the patients, or the friends of patients, but from a more reliable source than either of these, viz., from the *filthy* condition in which we receive those who have been confined there any length of time."

The foregoing details are sufficient to afford ample information in regard to all essential things appertaining to the indigent insane throughout this State who, when admitted to the Asylum, become the "State

patients." The unanswerable argument of poverty and necessity will send these in abundance to our institution.

But it is desirable that this Asylum should be patronized by the better classes of the community, not alone in this State, but also in those of our neighboring States, who are destitute of a similar establishment. This State, as already shown, has expended a large sum of money in erecting this Asylum, and no small portion of this sum has been devoted to building an edifice exclusively for pay patients, and to making such improvements as would render it worthy of their patronage. Many who have taken patients to the most celebrated Northern Asylums, have returned to this State with them; and, after personal observation, have stated that the Jackson Asylum is in no essential respect inferior to the best Asylums abroad, and, for many reasons, infinitely preferable.

Here, then, at our very door, we have a home institution, possessing advantages for the pay patient surpassed by few, if any similar establishment in this country. That all important time, the premonitory and incipient stage need not now be wasted in a distressing and perhaps fatal journey to a cold and inhospitable climate. The easy and economical traveling to Jackson will not force upon the friends of these unfortunates a long and painful separation, and the expenditure of large sums of money; and once there, they will be made to feel that they are in the land of their homes and friends; and, when they leave, feel that their patient is in the hands of an able, cheerful and energetic physician, and is surrounded by every comfort and attention which can conduce to his happiness and restoration. Thus, for from one hundred and fifty to six hundred dollars a year, a patient may enjoy here, at home, all those advantages which, during so many years, the insane has been compelled to seek elsewhere.

Let the true condition of things be once generally known, and no longer will the lunatic be conveyed by heart-broken friends thousands of weary miles, away from all that is hallowed in his memory. No longer will his mental and physical ailments be aggravated by a tedious and mournful journey to some Northern Asylum, where the cold winter winds cannot but be prejudicial to constitutions rendered doubly susceptible by disease to the unaccustomed rigor of the climate. How infinitely more to be preferred, are the equable and temperate breezes of this genial climate, which every day invites the sick man from his confinement to enjoy the beauties which flourish so luxuriantly in our prolific soil. Here, the insane need never be debarred from that *sine quâ non* to the successful treatment of chronic mental maladies—daily exercise in the open air; for the "Flowery Hill," upon which is seated the Jackson

Asylum, is almost that beautiful land of the poet, "where the flowers ever blossom, and the beams ever shine."

On the subjects of removal, and climate, Dr. Gray, of the New York Asylum, says, "Deceived by manifestations of seeming strength, few seem to realize the danger of removing acute cases of insanity a *great distance.* Several, brought during the severe winter weather from remote parts of the State, were severely frozen on the way." Again, "In a climate such as this (*i. e.* New York) it is impossible for invalids, whether such from mental or physical diseases, to take much out-door exercise during the winter months; yet, to no class of persons is a constant and abundant supply of fresh air, and the maintenance of a uniform degree of temperature in the apartments they occupy, so essential as to the insane."

It is a fact, well-known and acknowledged by all authors on insanity, that the ratio of the insane to the total population is very much less in warm, than in most cold latitudes. The cause of this is not well established, but the fact is beyond dispute. At the risk of exhausting the reader's patience, I cannot refrain from presenting some statistics to illustrate this point. By the United States Census of 1850, one man in less than every six hundred in Massachusetts, is insane; in New York, one in about every twelve hundred. On the other hand, in Louisiana, there is one to about every twenty-six hundred of total population, and one white insane man to about every eighteen hundred white inhabitants; and in Mississippi, my well-beloved native State, there is one insane to every forty-seven hundred total population, and one white insane in every twenty-eight hundred of the white population. This difference, between the Northern and Southern States, is well marked throughout the whole series. The great disproportion between Louisiana and Mississippi, to the prejudice of this State, is easily accounted for by the fact before stated, that more than *two-thirds* of our insane are not natives to the manor born. Deduct from the computation the large foreign population of New Orleans, and the statistics of Louisiana would be as favorable as those of Mississippi. In Egypt and Turkey insanity is remarkably rare; and Spain and Italy present even a stronger contrast with France, Germany and Great Britain, than is presented by the comparison made between our Northern and Southern States.

It is not strange that the medical writers of Northern climates should have endeavored to prove, that the greater number of insane in their respective countries, is attributable to the greater degree of refinement and mental culture prevailing therein; and to establish the law that insanity increases *pari passu* with civilization. We, in this quarter of the

globe, may be permitted to call in question what these writers have failed to prove; and may be pardoned for being so simple as to entertain, the hope and belief, that those means which are admitted, even at the North, to be so conducive to the restoration of the insane to health, may have something to do with protecting those who constantly enjoy these blessings from this terrible affliction. These blessings are, abundance of exercise in the open air at all seasons of the year, a mild and equable temperature, an agricultural life, and the absence of all those exciting "*isms*," so highly appreciated by *educated ignorance.*

If any reader of these pages should have under his protection some unfortunate lunatic, and doubt how best to discharge this responsibility, it may be well to remind him, that the greater the delay in subjecting the insane to proper treatment, the less the chance of recovery; that the more violent cases are usually the most amenable to treatment; that the interruption of familiar ideas and associations, and the subjection of the patient to new scenes, faces, discipline, and regular occupation, is deemed most beneficial; that no physician, however skilful, can treat the insane at their own homes, as successfully as a competent physician may in any well regulated asylum.

Notwithstanding the present admirable condition of the Asylum, for the reception of both State and pay patients, there are some improvements yet to be made, and some changes to be effected, calculated to promote still farther the original design of its founders. The Legislature will be probably solicited, at its approaching session, to make an appropriation sufficient to complete, as expeditiously as possible, these necessary improvements. As the State's agents in this matter, the Legislature has appointed Administrators of the Asylum, and the fact of their appointment should be the strongest evidence of the implicit confidence due to their representations. It is, therefore, earnestly to be hoped, that our Legislature will act with no niggardly ideas of false and ill-timed economy, but furnish the Board with the funds requisite to complete what has been begun in a spirit of benevolent zeal, commendable to every good citizen. In regard to the propriety and economy of making the contemplated improvements without loss of time, the present Physician and Superintendent thus expresses his sentiments: "From what I have been able to gather, from various sources, I am satisfied that a system of economy, entirely too rigid, consistent with the prosperity of the Institution, and the design of its founders, has been enforced heretofore. Under any circumstances, including the expense of building, etc., the institution will, for years to come, be an expense; no one ever expected it to be a source of revenue to the State. Therefore, why not at once appropriate

money sufficient to carry out fully the objects of an Insane Asylum? Pecuniary aid is imperatively required, to make all the improvements needed by the growing wants of the place, and to add such sources of amusement as all well-regulated institutions should possess. If it is true, (as all experienced Superintendents state), that few insane persons, who have been insane more than a year ever recover, no time should be lost in making ample provision for this large class of unfortunate individuals in our State. For should they be too long neglected, the State would be compelled to support them for life. Whereas, if promptly and properly attended to, they might have been cured in the majority of cases, and returned to their friends in a few months."

Upon this same subject, Dr. Stokes, so extensively and honorably known as the Medical Attendant of the Mount Hope Institution, for the past fifteen years, thus writes: "The stage or duration of the disorder, before the insane is subjected to hospital treatment, is found to exercise a most potent influence over his recovery. It has been conclusively established, that the probability of recovery, in cases brought under care within three months of the first attack, is as four to one, whilst in cases not admitted until more than twelve months after the attack, the probability of recovery is less than as one to four. Dr. Thurnam, who has recently very carefully and ably investigated the statistics of insanity, estimates the proportion of recoveries where the duration of the disorder when admitted was within three months and a first attack, to be seventy-eight per cent.; above three months, and within twelve, forty-five per cent.; but in cases of twelve months' duration and upwards, the probability of recovery dwindles down to about nineteen per cent. No point connected with the subject of insanity is more important, and none requires to be impressed more frequently and earnestly upon the public mind, than the fact that insanity, generally speaking, in its early stage, is a curable disease; that the first period of its approach is the time when treatment is most effectual; and that the want of proper management at this critical moment, and as is too often the case, the total absence of medical treatment, constitutes the true cause of the great proportion of incurable cases which has made insanity the opprobrium of medicine."

The first object worthy of the immediate attention of the Legislature, is the erection of an Infirmary, separated from the main buildings, for the accommodation of those afflicted with physical diseases. The intermixture of those suffering from corporeal maladies, with those whose minds are in a diseased and unnaturally excited condition, is most earnestly to be deprecated. One would scarcely suppose, that the condition of a man seriously sick, would be greatly improved by being surrounded by a band

of lunatics. Nor can it be supposed, that a lunatic easily excited, and often most injuriously, by the slightest cause, can witness, without prejudice to his condition, a sick man's sufferings, and often a painful death. Farther, when a contagious disease exists, the insane are subjected thereto. Thus, each act injuriously upon the other, besides interfering with that order and method so essential, both in the hospital and the asylum. Either two infirmaries, one for males the other for females, should be erected without delay, or one sufficiently large to be so arranged that the sexes may be kept distinct and apart.

Another work imperatively required, is the completion of the excellent idea originally designed, viz: the extension of the present "Central Building" ninety or one hundred feet back. This extension is absolutely necessary to furnish suitable eating rooms (for the accommodation of the male and female "State patients,") and to provide a proper kitchen, store rooms, etc., for the Asylum. The building now used for these purposes, is an old dilapidated wooden building. It is at such a distance from the main buildings, that it requires much time and labor, and is exceedingly inconvenient, to convey the meals to the "pay patients'" building, and to those who are confined to their rooms. The "State patients" who are compelled to eat there, are much exposed in damp and rainy weather. This building, in the engraving, makes quite a respectable appearance, giving one no idea of either how inconvenient and uncomfortable it *now* is, or of the ravages which time is so rapidly and unrelentingly inflicting, that it will soon be uninhabitable. The dangers from fire are rendered more imminent, and are much to be feared so long as this old "eye-sore" is allowed to stand. These are a few, among the many weighty reasons, why this addition to the permanent buildings, which must eventually be made, should not be delayed, and the present temporary frame house be razed to the ground. It now presents a most disagreeable contrast to the other elegant structures, and sits on "Flowery Hill" "like a mildewed ear, blasting its wholesome brothers."

Funds are also required for ornamenting the present yards with walks, shrubbery, and other attractive objects. But let us hope that the Administrators may have sufficient taste not to sacrifice the old forest trees to any mania for improvement. Some substitute should be made for the present palings which enclose these yards. A hedge of "osage orange" encloses the different yards of the Missouri Asylum, and I would suggest that no fence palings, or walls could equal in elegance, economy, and safety a hedge of the "Cherokee Rose," which flourishes so beautifully and luxuriantly in our soil. In a few years after planting all of the Asylum land, and the different yards might be impenetrably enclosed, thus

economically and beautifully promoting the health-giving design originally entertained, "to avoid, as far as possible, everything prison-like or repulsive in the appearance of the Asylum."

Among the improvements, not so immediately required, are the erection of workshops for the insane. All the old and well-established Asylums have found them of great benefit to the State, in curtailing their expenses. And all healthy occupations are most strenuously advocated for the welfare of the insane, by physicians throughout the world.

Some appropriation will also be required for a library. The pleasant diversion of the mind from its engrossing phantasies by the perusal of entertaining books often produces the most happy effect, and is deemed one of the most important means in the "moral treatment" of insanity.

A handsome sum might be procured, for increasing the present library, by subscriptions from the benevolent and sympathizing public of this State. And it might be respectfully suggested to any philanthropic Touro, anxious to bequeath his wealth to worthy objects, that a small portion of it would not be ill-bestowed on the "Jackson Asylum Library." Our newspaper editors should also bear in mind the poor lunatic; and in this, as in every other State, gratuitously forward their papers for their benefit.

The present law directs that criminals, acquitted on the plea of insanity, shall simply be sent to the State Asylum. The proper view taken of this subject by Dr. Kirkbridge, in his report to the Legislature of Pennsylvania, is well worthy of our Legislature's attention. The following extract will render any additional remarks unnecessary. "Even when conviction is avoided on the ground of insanity, there are certain forms of the disease that seem to require that *permanent seclusion* should be one of the results of such an acquittal. Where life has been taken or attempted, and a homicidal propensity has been shown to have existed, there can scarcely be a greater judicial farce, or a scene better calculated to bring legal proceedings into disrepute, than that an individual through whose instrumentality some one's life had been lost or seriously jeopardized, should, a short time after such a trial, be seen walking in our midst as openly as before the occurrence.

Hard as permanent seclusion—even with all the ameliorating surroundings which may be connected with it, and regarded too, not as a punishment for the individual, but simple protection to the community—may seem to bear on certain cases, still it is surely more just and truly charitable that one, who has attempted or executed deadly violence, should suffer *permanently* such a degree of personal restraint, as the safety of all others may require, than that these should always be exposed to

the risks which a recurrence of the propensity may at any time produce. Were this generally understood to be the result of the acquittal of such cases on this ground, the plea of insanity would probably be less frequently urged under circumstances in which the sound common sense of the community, as well as scientific accuracy, should alike declare it to be merely a scheme to escape the just penalty for offences against society."

Another change, which we "hope against hope" to see effected by our Legislature, is an extension in the term of office of the Administrators. The present term is two years; if extended to four, or ten years, or more preferable still, were they appointed to hold office during "good behaviour," how much more interest would they take in the permanent prosperity of the Asylum. Then, their good names would be involved in the continued progress of this Institution, and some opportunity be afforded them to gratify the laudable ambition of making themselves, by their own good works, known and honored far and wide by the friends of this unfortunate class. But what sort of ambition can a man gratify, what hope of doing good can he indulge in, when he reflects upon the brief tenure of his office, and knows that he may be superseded by bitter political enemies, who will misrepresent his exertions, and substitute anything, so it be a change, for the laudable improvements he has been laboring to accomplish? Have such examples been unheard of in the history of the charitable institutions of this State? As a Democratic voter, I may be allowed to believe, that no change will be effected in this respect, so long as political demagoguery, and the detestable doctrine of "rotation in office," (which, as Calhoun predicted, has made us a "nation of office-seekers," and corrupted freemen into *toadies,)* are prevalent throughout the land. These two causes find nothing too holy to desecrate; science, charity, philanthropy, when weighed in the balance with the "necessities of the party," are all found wanting.

Could this extension of the term of office of the Administrators be effected, we might then hope, too, that the "Physician and Superintendent" would be kept in office by them, as long as he remained worthy of the post. And since the longer he remained in office, the more worthy he would become, from the experience daily acquired, he would cease to be a "tenant at will," as he now is, and feel *secure* in his office. Will it be asked what advantages would accrue from this feeling of security? The history of every Insane Asylum, and of every physician who has made for himself a reputation in this branch of medicine, is sufficient reply. That experience which makes an extensive reputation for both the Medical Superintendent and his Asylum, is not the growth of a day nor a year. It is the result of long continued and ample opportunities,

which have been the more diligently cultivated, when security in office has brought conviction to the physician's mind, that the labor of a life time would be devoted to this cause, and upon it depend all his hopes of fame and ambition.

If these views be correct, it becomes the duty of the Board rather than of the Legislature, to carry them into execution. It is the farther duty of the Board to give a wide discretion to the power conferred on the Superintendent of "employing all needful attendants." Curtailing the trifling expenses for a requisite number of attendants, may be economy, but such economy, if once known, will surely cost them the loss of the most valuable pay patients. A standard authority says :

"It is evident that the well-being of lunatics confined in a public asylum must in a great measure, depend upon the number and conduct of nurses. It is essential, not only that the utmost discrimination may be used in the selection of such servants, but that their salary and comforts be such as to render their situation desirable. Nurses should, therefore, be distributed into *three* classes, in which they will *progressively rise* according to their behavior."

Among the attendants, the Board should allow the Physician to appoint two or more medical students, who, for their board and lodging, and the privilege of seeing the sick and studying disease, could be easily procured without farther compensation. Such opportunities as are afforded at the Insane Asylum should be a source of benefit to as many as possible of those who are to administer to the medical wants of this State. Sixteen students are allowed by the State, the privilege of dwelling in the Charity Hospital. It should be its settled policy to allow one medical student to, say every twenty-five public patients in any of its charitable institutions for the diseased, whether in body or mind.

To one more subject would I call the reader's attention before parting, hoping that he will attribute any details tedious to him to a cause in which, at least, it is to be hoped I have his sympathy, my interest in all that tends to the welfare of this State, and the progress of our home institutions.

This subject appertains to the Physician of the Asylum, and his office. But a short time since, the offices of Physician and Superintendent were distinct. The latter had charge of the business affairs and internal arrangements of the Asylum, while the former, residing in Jackson, was simply a visiting-physician, and could do little more than attend to the physical diseases of the inmates. Thus, instead of having all the regulations emanating from one mind, and all the patients under the exclusive control of the patients' medical attendant, there existed two offices with

duties often conflicting; and, as ought to have been expected, an indifferent alms-house was substituted for a well-regulated insane asylum. It is no wonder, that, under such an arrangement, the medical and general public at home should have been so indifferent to this Institution, and that it should have remained totally unknown abroad.

Fortunately these two offices have been united in the present Physician and Superintendent, Dr. J. D. Barkdull. Of this gentleman, one who has had ample opportunity to form a correct estimate, and whose good faith may be entirely relied upon, thus writes: "He is a gentleman of surprizing energy, feeling the full responsibilities of his duties; and, with his powers of endurance, I should judge him possessed of sufficient moral force and courage to direct the most wayward. I observed him in his daily rounds, and in familiar converse with his patients. His manner was characterized by humanity and uniform cheerfulness, which is so grateful to the sick and melancholy. He held the most excited and impatient in restraint, by a very mild yet most unmistakeable dignity, which at once insured obedience and respect. To him the institution owes the introduction of its present system, of combined medical and moral treatment. To him, with the coöperation of the intelligent gentlemen who compose the present Board of Administrators, belongs the high honor of discarding the use of all handcuffs, muffs or other relics of a barbarous age, formerly used there. The neatness, harmony and excellence of the internal arrangements of the Asylum are unsurpassed. This is doubtless owing to the care of the intelligent and sympathizing wife of Dr. Barkdull, who treats the friends of the patients as honored guests."

This flattering testimony to the Doctor's merit is doubtless well-deserved. Certainly, he should be all of this, to fill skilfully and conscientiously his responsible office. And to fill it thus, is the solemn duty owed by the Administrators to God, humanity and themselves. Let them so fill it, if they hope eventually to hear the cheering words, "faithful servants, enter into the Kingdom of thy Lord." Granting that the Administrators have properly discharged this duty, there are two topics worthy of their consideration: the duties required of this officer, and the compensation for their performance. Their attention is invited to their own regulations, specifying these duties *on paper*. But what adequate idea do these give of his annoyances, vexations, disappointments—of his anxious days and disturbed nights—of the feeling of responsibility which weighs upon him who has that only divine part of man, the reason of so many human beings depending on his skill and labor? Can any office be conceived of, which requires for its proper dis-

charge so many of the highest attributes of a man, and nicer knowledge of the human system?

And how much is paid for all this?

Boarding, lodging for himself and family, *if* it does not exceed *three*, (for each member above that number, a *moderate* charge will be made by the Board), and over and above this generous allowance; he is paid (shade of the great Hippocrates!) $1500 per annum.

Formerly, when the Physician did not fill the office of Superintendent also, when he resided and practised his profession in Jackson, merely *visiting* the Asylum, this salary may have been a proper remuneration for his services. But now that his duties have been more than doubled, and he is compelled to devote *all* his time to the Asylum, there is manifestly a sad disproportion between the labor and the compensation.

Fame says that the members of the present Board are all intelligent, progressive, liberal-minded gentlemen. May their proper appreciation of this subject justify the report! And may the pleadings of truth and justice prevail, however humble and unknown the source from which it proceeds! If they desire to inspire others with confidence in their choice, they must first prove *their own appreciation* of his ability to perform the responsible duties assigned him.

Are the highest professional attainments, daily discharged in the most onerous duties, to be compensated by a salary which a good negro overseer, or a dapper retailer of spools and ribbons may easily obtain? Is it becoming this wealthy and prosperous State to take advantage of a man's necessities, of one who, however competent, may be forced by the pressure of want and indigent circumstances, to accept any honorable occupation which will barely sustain himself and family? Is this the interpretation of Louisiana's escutcheon? If so, let us blot out the self-sacrificing mother, nourishing her offspring from her own beak, and depict in its stead, the famishing son begging bread of his father, and receiving a stone!

If a competent Physician and Superintendent has been found, it is to the interest of the wretched lunatic, that he should be retained as long as he can be induced to remain. A man of high intellectual attainment and lofty purpose may find strong inducements to continue in the Asylum, for he will be prompted by philanthropy, professional interest in the insane, and the opportunity of making for himself a wide-spread reputation. But these inducements, unfortunately, neither feed, clothe, nor educate a man's children; and he should have not only the means of doing this much, but also of accumulating something as a resource for an honorable old age, prematurely brought on by harassing, but useful exertion.

Justice gains strength in the minds of some by the force of precedent, and the unanimity of opinion. If these be needed here, they can be furnished. The State pays our principal Quarantine Officer $5000 per annum. Dr. Thorpe is well worthy of this office, and the salary is by no means too large a remuneration for his services. But, surely, no man competent to form an intelligent and impartial opinion, will say that the responsibility, labor, or professional skill required in the Physician of the Insane Asylum is less; and every medical man (Dr. Stone among others) whom I have asked what would be a proper compensation for the Physician of this Institution, has, unhesitatingly replied, that if the office be permanent, and the officer a competent man, (both of which should be the case) the salary should not be *less* than $5000 a year. Such are my sincere convictions not as an individual, owning a medical diploma, and who, for this reason would arrogate undue importance and demand exorbitant salaries, for all, who, whether worthy or unworthy, claim the unhonored prerogatives of this too cheaply obtained privilege; but as a man, feeling for my fellow-men, and anxious to see that right and justice rendered to them, which I demand for myself.

In conclusion, the language of the first Board may find an appropriate place: "Thus, it may be seen that a commencement has been made in the erection of a charitable institution which, when completed, (and now but little more remains to be done) will be found equal to the exigencies of the State, not unworthy of her growing wealth, and destined to remain as a noble monument to the philanthrophy of her people. And Louisiana may be justly proud in being the first among the Southwestern States, in extending the mantle of her charity to shelter and protect this unfortunate class of her population; and by so doing, she affords a noble example to her adjoining sister States—which cannot fail to have its influence in the cause of afflicted humanity."

ART. XII.—*Anomalous Lactation:*

DR. BENNET DOWLER:

Dear Sir: In compliance with your request, I give you the notes of an interesting case, that occurred in my practice; the only case of the kind I have ever witnessed—that of a grandmother suckling her grandchild. The lady who is the subject of these notes is well known to several eminent practitioners at Natchez, as an accomplished and intellectual woman, devoting much of her time to literary pursuits.

Being on a visit to Natchez, during the month of September, 1855, I was called upon to act as accoucheur to Mrs. M., aged twenty-one years. It was a primipara case, and the labor, though severe, was not protracted, resulting in the birth of a daughter.

The mother, after making many fruitless efforts to nurse the child, gave it to its grandmother, who had been present during the labor, and appeared to feel every pain as acutely as her daughter. She endeavoring by every means to pacify the babe, at last applied it to her own breasts, and strange to say the milk began to flow abundantly. In a few days the mother recovered sufficiently to be able to assist in nursing it.

Having occasion to visit Natchez again in September, 1856, I found the grandmother suckling the child, assisted as before, by its mother, and the infant thriving.

About two months since, they came to this city, with the intention of making it their future home. Shortly after their arrival, I visited them, and found them occupied as I had last seen them—both nursing the baby, now two years old, having grown a remarkably fine and healthy child, seeming to prefer the milk of its grandmother to that of its own mother.

The lady is a native of Virginia, about thirty-eight years of age, mother of two children ; the youngest a son, now twenty-one years of age.

A few days subsequent to their arrival in this city, they had the curiosity to ascertain their weight. The mother and grandmother were of the same weight—ninety-two pounds each. The baby weighed twenty-four pounds.

Respectfully,

JAMES S. EGAN, M. B. T. C. D.

New Orleans, November 20, 1857.

Art. XIII.—*Typhoid or Enteric Fever:* By R. L. Graves, M. D., San Antonio, Texas.

There seems to be as great a variety of opinions at this time, as to the true pathology and treatment of typhoid fever, as there ever was. I will, therefore, give what little experience I may have obtained in reference to this disease.

I have never seen a case of typhus fever according to my understanding of that disease; but I have met with four very tedious cases of typhoid fever during this summer and fall, in this city. The first case

was a negro boy, aged about eighteen years. When first called to see him, I found him quite stupid, with very little fever; but his abdomen was very hot, and I learned he had been loose in his bowels, and had been complaining of heat and cold. His pulse was quick and small; his tongue was of a brick-dust appearance, and his skin of an ashy complexion. I prescribed, ℞—calomel xx grs., Dover's powder xx grs. Mix, and divide into five powders; one to be taken every three hours; warm fomentations to the abdomen. I called again to see him in the evening, after he had taken several doses of the powders; found his bowels quiet, and his fever somewhat abated. I then prescribed castor oil, and as soon as it operated, put my patient on sulphate of quinine, and small doses of morphia. On the next morning found him no better. I then prescribed calomel and antimonial powders in small doses, at intervals of three hours. On my return the next day, I found the medicine had given a good deal of relief to his head, that had been quite hot; but his fever still continued with an increased heat of the bowels, and a decided prostration of the whole system. I then put him on sulphate of quinine and Dover's powders; but before night had to abandon the powders, as they were running off from the bowels. I gave some astringent enemata, and soon after an anodyne clyster. My patient was given some gum-water, and permitted to rest for the night.

On the morning of the fourth day, at nine o'clock, I found the fever somewhat abated, as usual in a morning. I then determined to keep his bowels open with the castor oil mixture, as below, and gave gum-water freely, and permitted the disease to run its course. I therefore prescribed, for that day:

℞. Castor Oil, ℥i.; Spirits Turpent., ℥ss.; Mucilage Gum Arabic, ℥iij.; M. Tinct. Opii. ℥i.

Of this, I gave half a tablespoonful every three hours, and applied warm fomentations to the abdomen. On the next day, the fifth, I called to see him; found his pulse much better, I mean fuller, and his bowels had been operated on only twice; his fever had remitted as usual. I gave gum-water freely that day and night; gave pills of blue mass with morphia. On the next day, the sixth, I saw but little difference in his case, except his head was much hotter and he appeared more stupid. I then cupped him on the nape of the neck, and shaved his head in front, and applied cold cloths to the head; his feet were put into hot mustard baths; continued the castor oil and turpentine mixture, and gave gum-water freely. On the seventh, I found my patient no better; his head was then shaved over the entire front, and I applied (after scarification) cups on both temples, and treated the case as the day before.

On the morning of the 8th, I found the negro a little better, but the head was hot as before, but somewhat relieved; his eyes were red; his hands tremulous; his tongue had cleaned off andpresented a sleek appearance. The bowels had ceased to purge, but were tympanitic, with more heat than before. I then prescribed ten gtts. of spirits of turpentine to be given in gum arabic emulsion, three times a day, and gave gum-water freely; applied cold cloths to the bowels, and had them frequently changed; also cold cloths to the head, and warm bricks to the feet. On the 9th, I found him pretty much as before; continued the treatment as on the day before. On the 10th, there was but little difference in his symptoms. Continued the treatment on the 11th; I found the tympanitis had passed off, and the heat of the abdomen very much abated; the tongue had become more moist; but the fever still continued. I then prescribed gum-water and rice-water, and permitted him to remain on that treatment for ten days more, giving him a dose of oil, or an enema to open his bowels regularly, and when necessary, giving an astringent enema to keep the bowels from moving too much.

On the 21st, I found the fever had ceased; I then paid particular attention to his diet, and in five weeks he was able to sit up.

The next case was treated pretty much as the other, but I did not give as much calomel or sulph. quinine. He recovered in about the same time.

The next case I treated without sulph. quinine, except by enema, and with but very small quantity of calomel. This was a boy, five years old, my own child; the calomel was given at the commencement in ten grain doses for two days, and spirits of turpentine in large doses, as I was under the impression this was a case of worms; but as the fever continued, I became satisfied it was a case of typhoid fever. The child, however, passed a leech on the ninth day after he was attacked, and on the seventeenth day the fever gave away. For five days he was in a comatose state, and did not open his eyes or speak.

The next case was my own child, seven years old. This case I treated without calomel or sulphate of quinine; gave, I think, one or two doses hyd. cum creta and Dover's powder at the start, but saw no good from either. I became satisfied that calomel and sulp. quinine aggravated the disease, and put my boy on the turpentine and castor oil mixture, and gave him gum-water, and lime juice, made into lemonade. This I gave him whenever he desired it; also sago, jellies, and beef tea, and ventilated the room freely, and on the fifteenth day his fever ceased. His cheeks were red on the three days following, but only for half an hour each day. On the nineteenth day, about one o'clock, he took a little roasted apple; is entirely clear of fever, and is convalescent.

I found brandy or wine to increase the fever. I am now satisfied that sulph. quinine, calomel, or stimulants, are injurious in the treatment of typhoid fever, unless you use stimulants after the fever has subsided—and then, it should be the best wine, and be given in emulsion of gum-arabic.

I have been in this city four years, and have only seen the four cases of typhoid fever above reported. I practised five years at Lake Providence, Louisiana, and never saw one case of typhoid fever, but quite a number of cases of typhoid pneumonia.

PROGRESS OF MEDICINE.

ART. I.—*Etiology of Yellow Fever.* (Discussions in the French Academy of Medicine.) Translated and Condensed from the *Archives Générales de Médecine*, for October, 1857: By JAMES JONES, M. D., Co-Editor.

THE generation of yellow fever, always a subject of interest in regions liable to its invasion, has acquired a new importance from its late fatal and extending epidemic influence, and the recent general effort to establish a uniform code of Quarantine Regulations. The positive and authoritative tone assumed by several of the distinguished leaders in these discussions, in affirmation of the contagious character of this disease, will doubtless recall to the recollection of many, the unwise and useless effort in our late Quarantine Convention, to pronounce yellow fever never contagious *per se.* From what patent of orthodoxy this inter-municipal association acquired authority to condemn the opinions, and discredit the experience of a large portion of the scientific world, will doubtless, hereafter be voluminously shown.

The doctrines here promulgated by Trousseau and Bouchardat have not appeared for the first time. They were maintained in the last century by Lind, Humboldt, and Fodéré, and in the present by Burnett,

Pariset and Berthe, and are identical, in many respects, with what I have myself taught as a Professor of the Practice of Medicine in the University of Louisiana.

The miasm of yellow fever is specific. In the face of our very imperfect knowledge of the real nature of morbid poisons, and of many conflicts of antagonistic opinions, we have no more positive evidence of the actual generation of this miasm by local causes than of any other productive of maladies confessedly non-paroxysmal and non-recurrent. The germs, sporules or ova of this zumé are, probably, in certain localities, ever present. They may be forced into sporadic action, a brief, limited and barren life by circumscribed local causes, aided by peculiar morbid susceptibility. Again, by more general influences, they may exhibit a development capable of producing all of the phenomena of endemic fevers, mature, potent, fruitful, and only capable of reproduction or multiplication, and as in a transitory stage of heteromorphous or successive generations. Lastly, by some extensive climatic constitution, or by some unknown law of their own disposition to reproduction, we have the full and perfect maturity of these germs, sporules or ova, that perform a part in every zumé fruitful, reproductive, epidemic, infectious, contagious both far and near by every mode of propagation attributed to communicable maladies. Phytology or zoölogy without fermentation will sustain this a hypothesis. The periodical epidemic and endemic prevalence of cholera, of plague, and of less kindred diseases, like measles, pertussis and small-pox, will offer examples and analogies from general pathology. Craigie, a strong non-contagionist, speaking of these presumed atmospheric conditions, which have, however, never been established, and of which the phenomena can be better explained by the periodical reproduction of certain organisms, says: "This fact indicates, I conceive, a sort of *cycle* or periodical revolution in the train of atmospheric phenomena, which, at the end of certain periods, assume the same position and combination in regard to each other. It appears that yellow fever becomes epidemic neither in consequence of intense temperature alone, nor humidity alone, nor filth, nor the presence of foul docks and wharves, nor desiccated marshes, nor decomposed cabbages or coffee, nor even charred ship-holds; but in a certain condition of the atmosphere which returns at very uncertain intervals, and of the recurrence of which the *circumstances now enumerated are indications.* The rapid decomposition of vegetable and animal matters is to be regarded not as a *cause of fever, but as an effect of the febriferous state of the atmosphere*, which thus not only displays its insalubrious influence on the human race, but on the vegetable world, and on dead animal and vegetable matter." Substitute for the periodical revolution of

atmospheric phenomena, periodical phases of the evolution and reproduction of zymotic organisms, and we have a more intelligible rationale of all that follows.

Nominated in 1851, Chairman of a Committee of the American Medical Association, to report on the relations of Bilious-Remittent, and Yellow Fevers, I have never devoted one hour's attention to the duty, having no cognizance of any degree of consanguinity between these two diseases. Had the author of that commission suggested an inquiry into the relations of bilious-remittent, and of intermittent fevers, as members of the same family, it would have been more practical if not more useful. Had he wished to trace the relations of bilious fever, with bilious-intermittent, bilious-pleurisy, or any other congeners by that morbid element, it would have been equally consonant with sound pathology. I acknowledge affinities in the etiology of bilious-remitnt and of yellow fever. For this last is neither a malarious disease, a paroxysmal disease, a country disease, a bilious disease, nor a recurrent disease, but always in its regular forms quite the contrary, besides being occasionally infectious and contagious. I take this occasion to thank my old fellow-student, Professor Richard Arnold, of Savannah, for his late essay on this very question, which he has treated in all of its aspects with his usual ability.

The following extracts are taken from the discussions of the Academy of Medicine, at Paris, during the sessions of the 8th and 15th of September, 1857 :

" At the first meeting, M. Depaul read a report from M. Girardin, on an important work of M. Dutroulau, on Yellow Fever, containing the results of the author's observations during the five annual visitations of the epidemic at Martinique and Guadaloupe, from 1851 to 1855.

" Dutroulau describes carefully the anatomical characters of the disease, and attempts to show that the alteration of the blood is the principal lesion. After a careful study of the degrees, symptoms and diagnostic characters of yellow fever, he enters upon the etiology. Its essential cause, says he, is a *specific miasm*, very distinct from marsh miasm ; its mode of transmission is alike established both from infection from the sick and through that of localities. The Commission recommends a vote of thanks to M. Dutroulau, and the publication of his memoir.

" M. Londe found in this memoir some inexact assertions. No one, in his opinion, doubts at the present day, the paludal origin of yellow fever.

" M. Trousseau combatted M. Londe's statement as to the paludal origin of yellow fever, relying for support on his own experience and that of

M. Louis, which dated from the period of their mission to the South of Spain. He cited Gibraltar as a place exempt from all paludal emanations, in which, as in other cities very elevated in their position, and enjoying similar exemptions, yellow fever prevailed with great violence; while at St. Roque, a mile and a half from Gibraltar, and where intermittent constantly prevails, there has not been a single case. M. Trousseau does not deny that yellow fever prevail most frequently where we encounter paludal influences. He wishes to prove that the disease does not require localities for its development, placed in such conditions.

"After some observations of M. Bouchardat, who maintains that yellow fever commences generally in a marshy locality, and afterwards transmits itself readily from one man to another by the power of a *specific malady*, the report was adopted."

"Sept. 15.—M. Londe renewed the discussion on yellow fever, to which he ascribes a paludal origin. In support of his opinion, and without invoking the testimony of Chervin, who was on the Commission with Louis and Trousseau, at Gibraltar, he cited the opinions of a number of English physicians who resided in this city, and who attribute the epidemic of yellow fever to local causes.

"M. Rufz testified that during a long residence in the Antilles, he had seen the yellow fever many times. In his opinion, Chervin had committed a great error, in wishing to establish the identity of the yellow and of malarious fever. If these are analogies, these are also discrepancies between these diseases. The domain of yellow fever is, in truth, more limited than that of intermittent. Yellow fever prevails only on the sea-side; malarious fever in the interior of a country; yellow fever attacks only a certain class of persons under certain conditions—Europeans in the Antilles; malarious fever attacks everybody. Contrary to malarious fever, yellow fever leaves nothing behind it, and cannot return a second time. If yellow fever is contagious, the contagion is peculiar. He has never observed a case of the disease propagated by contact. Arriving at the question of transmissibility, M. Rufz presented some very interesting remarks in demonstrating that this disease has never extended itself beyond the sea-shore.

"M. Trousseau took the floor to answer M. Londe, and to examine the reasoning of M. Rufz. Referring to the epidemic at Gibraltar, he did not deny that conditions of insalubrity exist in every city. But there is a wide difference between insalubrious conditions and malarious conditions, in which last, Gibraltar was far from being placed. The sewers of this city, where they desired to locate the causes of the epidemic,

had been just well purified at the moment the epidemic appeared; while for the fourteen years previous, when they received no care, there had been no pestilence. M. Trousseau did not, therefore, admit the malarious origin of the yellow fever at Gibraltar.

"M. Rufz, continued M. Trousseau, finds certain analogies between malarious and yellow fever; he has seen, during the prevalence of epidemic yellow fever among the unacclimated, severe fevers of an intermittent type, develop themselves among children born since the last epidemic, and among residents, and yield to the sulphate of quinia; but this is only confined to malarious regions, and is as true for all acute diseases, as for yellow fever. In Algeria, thoracic affections often exhibit exacerbations of a tertian or double tertian type. If M. Rufz has observed intermittent symptoms during the first two or three days of yellow fever, that is to be referred to the region. In Gibraltar, a non-malarious country, M. Trousseau has seen nothing similar.

"M. Trousseau then took up the question of contagion, with a great show of authority. If people do not credit the contagion of yellow fever, it is because they always seek to compare one contagion with some other contagion; the contagion of itch with that of small-pox, that of dysentery with that of putrid fever. But contagion assumes a great number of forms; some contagions strike but at a distance, others propagate only by immediate contact. Borrowing his examples from fecundation and generation, M. Trousseau explains contagion and its various forms by some remarkable analogies; he concludes by saying that he finds no identity between yellow and malarious fevers, and that the non-transmissibility of this disease in our own country does not prove in any manner that it may not be contagious in others."

ART. II.—*Diarrhœa and Dysentery:* By DR. CAZALAS. Translated from the *Gazette Médicale de l'Algérie*, of Sept., 1857: By J. P. BARBOT, Apothecary.

[DR. CAZALES, Physician-in-Chief, in the Hospital of the Military School, at Constantinople, is contributing from time to time a series of interesting statistical papers, illustrative of the sickness and mortality of the French army, during its late operations in the East. In the following article from the *Gazette*, he investigates two of the principal diseases which prevailed among the troops:]

DIARRHŒA AND DYSENTERY.

Diarrhœa is sometimes a disease in itself, and sometimes the symptom of another disorder of the system. On this occasion, I will consider it in its first condition only.

When diarrhœa constitutes a disease, it is due to a morbid condition of the large intestine, characterised by more or less copious alvine dejections, frequently accompanied by straining and pain, and always freer and looser than normal, but without any sanguineous exudation from the mucous membrane of the bowels.

Dysentery is also an affection of the large intestines, characterised during life by the following symptoms: pain more or less severe, in the abdomen, a frequent and sometimes constant desire to go to stool, attended by straining, tenesmus, and discharges, small in quantity but always, painful, and more or less tinged with blood exhaled from the intestinal surface of the large intestines.

I have treated of these two affections under the same head, because their relation to each other is so intimate, that it is, in fact, very difficult to separate them in practice; they are seated in the same portion of the organism; because diarrhœa is at all times passing into dysentery, and dysentery into diarrhœa; because, in reality, one differs from the other only in the absence or presence of blood in the stools; because, in fine, it is impossible at the bedside of the patient to state positively where diarrhœa ends and dysentery begins.

The following table exhibits monthly statistics of the patients treated for one or the other of these diseases, and will at once give the reader an idea of their frequency and severity in different seasons.

Although this tableau only exhibits cases of diarrhœa and dysentery either simple or sufficiently severe to predominate over other symptoms, it will show how severe and frequent they were from the beginning to the end of the campaign; for they make up one-fifth of the total of admissions, and more than one-third of the deaths.

Form of Intesti-nal Disease.	1855.												1856.								
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	January.	February.	March.	April.	May.	June.	July.	Total Intestinal Diseases.	Total Deaths.	Relative proportion of deaths to patients.
Diarrhœa..	120	220	111	102	30	139	146	198	99	152	107	176	106	85	69	33	50	26	7	1976	766	1 to 2.5
Dysentery.	26	65	33	27	2	32	67	94	20	55	20	42	18	13	4	1	2	1	1	622	215	1 to 2.4
Total.....	146	285	144	129	32	171	203	292	119	207	127	218	124	98	73	34	52	27	8	2498	981	1 to 2.5

But in order to give a better appreciation of their importance in the

pathology of "*l'Armée d'Orient*," I will give a synopsis of the cases under my treatment, and exhibit their symptoms, and relations to one another, or their connection with other diseases.

In 4,114 patients admitted, I have found:

Diarrhœa and Dysentery, either simple or not complicated in any marked manner	406	times.
Complicated with Angina	2	"
" " Ophthalmia	3	"
" " Hemeralopia	1	"
" " Icterus	10	"
" " Ascites	1	"
" " Anasarca	10	"
" " Albuminuria	6	"
" " Bronchitis	30	"
" " Pleuro-Bronchitis	16	"
" " Pleuro-Pneumonia	3	"
" " Pulmonary Tuberculoris	8	"
" " Frost-Bite	98	"
" " Scurvy	452	"
" " Scurvy and Anasarca	21	"
" " Scurvy and Intermittent Fever	85	"
" " Intermittent Fever	170	"
" " Intermittent Fever and Frost-Bite	7	"
" " Remittent Fever	138	"
" " Remittent Fever and Frost-Bite	16	"
" " Typhoid Fever	54	"
Total	1,537	times.

Whence we see, first, that out of 4,114 patients, 1,537, or more than one-third, were suffering at the time of their admission, with diarrhœa or dysentery, either alone or in connection with other diseases.

Second, That out of 1,098 deaths, 359, or rather more than one-third, were caused by either of these diseases, either simple or complicated.

Third, That out of 1,537 cases of diarrhœa or dysentery, 406 only were not complicated with any other disease in a notable manner; and that 1,131, or more than two-thirds, were connected with other more or less severe affections.

Fourth, That out of these 1,537 cases of diarrhœa and dysentery, there were 359 deaths, or one in 2.4.

The preceding tables clearly show the frequent prevalence of affections of the bowels, and fully justify the assertions of Desgenettes, Fournier and

Vaidy, that "dysentery * causes more ravages in an army than *typhus fever, yellow fever* or the *plague*." We might add, than cholera even; for cholera only carried off from the "*École Militaire*," which was the special Hospital for cholera patients, 658 patients, while 981 died of chronic diarrhœa and dysentery, without taking into account the part they bore in deaths produced by other diseases, in which they were an important symptom.

Affections of the lower portion of the digestive tube—setting aside those periods when cholera and typhus fever raged as an epidemic, have constantly predominated in frequency and mortality; and we may affirm that by reason of their permanency, diarrhœa and dysentery are, in all armies in their campaigns, the most dreadful and most devastating.

We rarely met with diarrhœa and dysentery in an acute form. When first brought to our charge they were already chronic at their second, third or fourth relapse or return, (or complicated with other diseases, particularly scurvy.) As long as they were simple and acute or not serious, they were treated in the regimental infirmaries or in the "*ambulances*."

Simple diarrhœa was, in the "*Armée d'Orient*," so general a morbid symptom, that I have no hesitation in saying that in its acute form, it preceded about all the other diseases, and that very frequently the other diseases terminated in a chronic diarrhœa. I have scarcely found five hundred out of these four thousand one hundred and fourteen patients—that is one in eight—that did not show as their first symptom or their first ailment, diarrhœa of greater or lesser duration, or of more or less intensity; and, on the other hand, a majority of the somewhat protracted diseases, of whatever nature, but particularly scurvy, would be complicated with diarrhœa often very obstinate, and which frequently became the direct cause of death.

Dysentery itself is, perhaps, never a primary disease in camps; it has for a prodroma a diarrhœa of more or less duration, which is more or less obstinate, and often recurs several times. It is most generally due to a transition of diarrhœa into dysentery, brought on either by cold, too hearty a meal, bad liquor or any other determining cause; and it frequently happens, in our hospitals, that what is diagnosed as *diarrhœa* one day, becomes a *confirmed dysentery* the next; and just as this latter disease almost constantly begins by acute diarrhœa, even so does it terminate, (should it be prolonged), before the death or cure of the patient, into a diarrhœa more or less obstinate, more or less difficult of cure.

* All these authors confound chronic diarrhœa with dysentery.

This frequency of diarrhœa at the beginning and close of other diseases, is not otherwise a fact occurring to the soldiers of the "*Armée d'Orient*" exclusively; it is observed in all armies in their campaigns, particularly in warm climates; it is due to the soldier's manner of living, to nostalgia, to the insufficiency or bad quality of their food; to his fatigues and privations of all sorts, to his constant imprudences, the dampness of the ground on which he is obliged to sleep; to his marches by day, and duties by night and day, and the vicissitudes of the atmosphere from which he can seldom protect himself sufficiently.

Acute diarrhœa is never, and acute dysentery is very seldom, the direct cause of death. Chronic diarrhœa was always the more frequently fatal of the two, for it was rare that in chronic dysentery the dysenteric symptoms, which are tenesmus and bloody evacuations, continued until death.

The anatomical lesions in acute dysentery consist in inflammation and ulceration of the large intestines. In chronic diarrhœa and dysentery they have the same seat, and are characterised by recent or old ulcerations, of greater or less extent, generally deeper and more numerous as they approach the inferior portion of the rectum; more or less of an indurated thickening of the sub-mucous cellular tissue. In ninety-two post-mortem examinations on subjects from the Crimea, without taking into consideration the disease which had been the direct cause of death, we found in sixty-eight cases, in a greater or lesser degree, the lesions of chronic diarrhœa or dysentery, viz: inflammation, recent or old ulcerations, œdematous thickening and induration of the sub-mucous cellular tissue of the large intestines.

When primary acute diarrhœa was attended to from the first, it was cured, most generally, in a few days, by rest or diet, either alone, or in connection with farinaceous gruel and a few drops of laudanum.

Were this disease—of which, almost all the soldiers in a campaign suffer, which is so trifling in appearance and so serious in reality; inasmuch as it impairs the constitution, and renders it more liable to other diseases—were it, I say, treated on its first appearance by the methods I have mentioned above, I can safely say, that the number of severe disorders and deaths would be diminished one-half. But, unfortunately, soldiers generally do not complain of diarrhœa until it has reduced them, or, unless sharply attacked.

When diarrhœa was attended by bilious symptoms, and these were the most frequent cases, an emetic or an emetico-cathartic at the outset, followed by diet and laudanum, would almost always bring about, in a short time, a certain and permanent cure.

In chronic diarrhœa, even when complicated with scurvy, I have gene-

rally found astringents of all kinds more injurious than beneficial: the relief they afforded was generally more transient than durable, more apparent than real; and they were only calculated, in a majority of cases, to mislead both the physician and the patient as to the gravity of the case, in inducing them to consider the cure as affected as soon as the symptoms of the disease had disappeared.

Tonics were generally ill borne. The best, was good wine. I would give it, carefully and incessantly watching its effects, until I saw that the patient could take it without suffering therefrom any morbid derangements in the digestive functions.

Ipecac, in alternative doses, and seidlitz water, in small doses, repeated now and then for three or four days, generally produced good effects.

Opium, steadily administered, either alone or in combination with doses of ipecac, or sulphate of magnesia, was, of all remedies, the most happy and successful in its results.

But none of these therapeutical agents produced any permanently successful results, unless aided by a proper system of diet and hygiene; also, a fresh water or steam bath to cleanse the skin and restore its normal secretions, applied warm, at once next the skin, in order to maintain on the surface of the body an uniform and gentle heat, were always very useful auxiliaries; but diet, I cannot too often say it, and repeat it—diet was the main, the most important point in the treatment of chronic diarrhœa. Without an extreme persistence in a dietetic course, there was no possibility of effecting a cure.

Just as soon as the patient began to eat too freely, and had too soon began to use *gras* (*i. e.* meat), he would relapse. Whenever diarrhœa patients had sufficient control over themselves, to restrict the false appetite which is often experienced in intestinal affections, and would be content to take no more than one-fourth of their ordinary ration, for some time—they would generally get well; but just as soon as, to gratify their hunger, they would go beyond this limit too early, they would most generally relapse.

The *régime maigre* consisting of rice and vermicelli soups, boiled rice, or arrow-root—the use of which, at the request of *M. l'inspecteur* Baudens, had been generously permitted to the Constantinople hospitals as exceptional cases, bread and wine, according to the digestive powers of each patient; light food, such as baked apples, prunes, eggs, etc., etc. These must be, for weeks and even months at a time, the food of patients suffering from chronic diarrhœa. I have seen a great number of cures, which might almost have been called miraculous, procured by this regimen, and the system of medication mentioned above; but how often

have I not seen men, whose cure would have been easy by proper medication and diet, fall victims to a greater or less number of relapses, almost invariably produced by errors in diet.

The treatment of acute dysentery offers nothing special; it consisted, as I have stated elsewhere,* in the use of emetics, purgatives, enemata, cataplasms, hip-baths, and opium, either alone or combined with ipecac.

When the disease is of the chronic form, it must be treated in the same manner and with the same rigid attention to diet as chronic diarrhœa, as its anatomical lesions are like those of diarrhœa—a great alteration in the structure of the large intestines.

ART. III.—*Experimental Researches on Normal Albuminuria, in Man and Animals:* By DR. CLAUDE GIGON, Physician to the Hospitals and Prisons of the City of Angoulême.

"I am a simple Pioneer in this mine, in which the truth lies so deeply buried."†

Translated for this Journal from L'Union Médicale, of October 13, 1857: By M. MORTON DOWLER, M. D., New Orleans.

UP to the present period, the existence of albumen in the urine, has been considered as a pathological Phenomenon; and, indeed, for a long time its appearance in the renal excretion was thought to be restricted to cases of albuminous nephritis or morbus. Brightii observation, however, subsequently disclosed the fact, that this phenomenon is of much more frequent occurrence than was at first suspected. It was found to exist in the urine of hydropics,‡ in which the disease results from compression of the large venous trunks, and especially from compression of the vena cava; in cases of cantharidian cystitis; in the melanuria, or black urine; of scarlatinous œdema; in the prolonged and violent paroxysms of asthma; in the slow asphyxia which is seen to take place in organic affec-

* General and Practical Considerations on the Treatment of Dysentery.—*Metz.* 1845.

†*Bacon, sa vie, son temps, sa philosophie, etc., etc., par Charles Rémusat; chez Didier et Compagnie*, 1857.

‡ By making an adjective discharge the double functions of substantive and adjective, giving, thereby, a pertinent designation of both patient and disease, the French language avoid an awkward and inelegant circumlocution prevailing in our own noble language, and which might be easily avoided by adopting the legitimate manner of the French. While the French say, *cholérique, cholériques; phthisique, phthisiques; hydropique, hydropiques; anémique, anémiques; ictérique, ictériques, etc., etc.* In our language we have no more concise equivalent expression than *cholera patient, or patients; phthisical, hydropic, anæmic, icteric patient or patients.* There are no reasons whatever that can be assigned why we should not legitimize in English the calling of our cholera patients *cholerics*, our phthisical patient, *a phthisic*, our icteric ones, *icterics*, etc. Indeed, that "soul of wit," brevity, has compelled us already to speak of our *dyspeptics* and our *hypochondriacs*; and if a patient in French can be considered a *phthisique*, there is every reason for setting him down in English as a *phthisic* and spare effusion of words.—TRANSLATOR.

tions of the heart; in certain cases of pregnancy; and in certain cases of debility attendant on relaxation of the tissues, as in children of a lymphatic temperament, or that have been debilitated by disease, etc., etc. But up to the present time, however, so far as I know, no one has recognized albuminuria as a physiological and normal phenomenon in man.

The opinion of the prince of French chemists on this subject may be deduced from the following: "the excrementitious fluids are the *only ones* in which we realize the total absence of albumen." (Dumas, *Chim. Phys. et Méd.*, *p.* 346.) Berzelius, (*Traité de Chimie. t.* vii., *p.* 390,) says "Albumen is sometimes found amongst the constituent principles of the urine; and, in reality, it is very often found in this fluid; *but it is always in consequence of a morbid condition*, or, at least, of a debility." The mere opinion of two such illustrious men, has, for a long time, caused us to hesitate in the publication of our experiments.

Several years since we had suspected the general existence of normal albuminuria in man; but, in the absence of conclusive proofs, we abandoned the idea. Some time since, however, having undertaken some physiological researches, which we hoped soon to be able to submit to the Academy, we were led to renew our study of the renal secretions, and under the following circumstances: having employed chloroform as a reägent for the detection of iodine in the urine, we witnessed at each experiment, the formation of a thick white abundant precipitate, which often masked the appearance of the phenomena of coloration. We then bethought ourselves that this precipitate might be normal albumen—such as at a former period, we were persuaded we had discovered; and, in order to verify our idea, we undertook a great number of experiments, which we now propose to make public.

We began, for the purpose of comparison, by taking ten grammes of the white of eggs, and the same quantity of the serum of blood—substances, especially the former, which may be considered as representing albumen in its greatest state of purity. We made solutions or, rather, dilutions of these substances in two porcelain mortars, with two hundred grammes of distilled water to each, and five grammes of the contents were placed in small experiment tubes, closed at one extremity; twenty drops of chloroform were added, and, after a few seconds' agitation, it formed at the bottom of the tube an abundant and compact precipitate. We then submitted the same albuminous solution, successively, to the action of heat, nitric acid, creosote, alcohol, tannin, nitrate of silver, bichloride of mercury, the neutral acetate, and the subacetate of lead—and by all of these reägents there were obtained all the special reäctions pertaining to albumen. The experiment was then varied, solutions (blood-serum

and white of eggs) containing one-five-thousandth part, by weight, of the white of eggs being used. In this state of extreme dilution, all the reägents that disclosed albumen were without apparent action on the fluid; so that a person accustomed to the usual means of investigation, would not have failed to pronounce the non-existence of albumen in this fluid. Meanwhile, six grammes were poured in a test tube, and afterwards, twenty drops of chloroform. No reäction was at first observed. The chloroform, being heavier than the water, descended to the bottom; but the fluid being shaken for two or three seconds, it was allowed a few minutes repose, whereupon there was observed at the bottom a white, abundant, compact precipitate, which cannot by any possibility be anything else than albumen, since the latter was known to have been placed in the fluid, and since distilled water yields no precipitate to the action of chloroform. No doubt can therefore rest over the result. Hence, it appears, plainly, that chloroform is a reägent vastly superior to all other known reägents, in testing the presence of albumen—a fact hitherto unknown.

From what we have said, it will be seen that, accordingly as we operate with dilutions of one-twentieth or of one-five-thousandth, the results will be very different. In the first case, there will be all the hitherto known reäctions proper to the presence of albumen, with that of chloroform superadded; whilst, in the second case, all the hitherto known reäctions are wanting, the chloroform alone declaring the presence of albumen. With a view of determining the relative power of the several reägents in testing the presence of albumen, I instituted the following experiments: The white of eggs and the serum of human blood were diluted in distilled water to such a degree that the albumen existed in the several solutions in the proportions of 1–20, 1–100, 1–500, 1–1000, 1–3000, 1–5000 and 1–10,000. The different solutions were then successively treated with the following reägents which are here placed in the order of their power as disclosed on experiment:

1. Chloroform.
2. Creosote.
3. Tannin, (5 grammes to 50 grammes of distilled water.)
4. Subacetate of Lead, (10 grammes to 100 do.)
5. Nitric Acid.
6. Nitrate of Silver, (2 grammes to 75 do.)
7. Bichloride of Mercury, (2 grammes to 75 do.)
8. Neutral Acetate of Lead, (10 grammes to 100 do.)
9. Rectified Alcohol of Pharmacy.
10. Heat.

By the foregoing it will be seen that chloroform is the most reliable reägent, and the most sensitive, to the presence of albumen, and discloses it when existing in extremely small quantities, seeing that by chloroform albumen is precipitated when dissolved in 10,000 times its weight of distilled water, and even more. When the chloroform is dropped into such dilution, the presence of albumen is not at once declared; but by the process of shaking, it is soon made to appear at the bottom of the tube,

being in the form of an alabaster-white, compact, precipitate, when the albumen is abundant; and of a pearly appearance when the quantity is more minute, as for example, when it is at the 1–5000 dilution or more. If to the liquid in the tube which contains the albumino-chloroformic precipitate, a certain quantity of nitric acid be added, the precipitate is not dissolved; but if the supernatent fluid be decanted, so that there be left nothing in the tube save the white precipitate of which we have spoken, and a great excess of nitric acid, the following results occur: The chloroformic precipitate of the 1–20 dilution is not dissolved; the precipitate of the 1–100 dilution is incompletely dissolved; but if the above precipitates be heated to a moderate degree, much below the boiling point, they are completely dissolved. On the other hand *all the precipitates*, from the 1–500 dilution up to the 1–1000, are completely dissolved without the aid of heat. Thus, then, it will be seen, that nitric acid dissolves normal albumen with or without heat, a fact to which we shall recur hereafter.

[To be Continued.]

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.

From the Medical Purveying Office, United States Army, New Orleans.

NEW ORLEANS, LA., Lat. 29° 57′ 30″ North; Long. 90° West; Altitude of Barometer above the level of the sea, 35 feet.

1857.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
MONTHS.	Max. of Obs.	Min'm of Obs.	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
October..	7 A.M. 17th. 30.380	2 P.M. 8th. 29.856	30.138+	2 P.M. 4th. 80	7 A.M. 30th. 63	72.38+	Several 79	Several obs. 57	69.77
November	7 A.M. 26th. 30.500	2 P. M. 7th. 29.810	30.159+	9 P.M. 7th 82	7 A.M. 21st. 55	66.95—	2 P.M. 7th 80	7, A.M. 20th. 39	61. 1
Dec. 17..	10 p. m. & 11 a. m. 30.500	2 P. M. 6th. 29.987		7, A.M. 9th. 77	7 A.M. 13th. 59		2 P.M. 8th 78	7 A.M. 11th. 48	

1857.	HYGROMETER.			PREVAILING WINDS.	RAIN.	
MONTHS.	Max.	Min'm	Mean		No. of Days.	Quant'y
October..	2 P.M. 4th 76	7 A.M. 21st 53	60.09	North, Northeast and East.	7	5.10
November	2 P.M. 6th. 76	7 A.M. 20th 35	57.95	North, Northwest and East.	10	3.05
December	2 P.M. 8th. 75	7 P.M. 10th 46		Northeast, Northwest and North.	7	2.27

Mortality Statistics of New Orleans, compiled from the Weekly Reports of Dr. Baldwin, Secretary of the Board of Health.

Population of New Orleans, 150,000.

Time.	Total Deaths.	Children under 20 years.	United States.	Foreign.
October, (five weeks)	632	271	310	322
November, (four weeks)	480	209	280	200
December, (two weeks)	193	95	125	78

Principal Diseases.	October.	November.	December 13.
Still-Born	34	39	7
Tris. Nascent	24	12	5
Teething	16	6	1
Cholera Infant	4	6	1
Infant. Convulsions	29	23	20
Croup	8	9	4
Scarlet Fever	..	4	3
Measles	..	..	..
Variola	3	4	7
Diarrhœa and Dysentery	54	30	11
Gas. Enteritis	9	7	1
Inflammation of Lungs	19	10	10
Consumption	52	61	22
Apoplexy	13	6	4
Congestion of Brain	7	8	2
Fever Typhoid	20	8	1
Fever Miasmatic	34	7	2
Fever Yellow	125	55	4

Although one case of yellow fever was reported in June, one in July, one in August, and eight in the latter part of September, it was not until the occurrence of these last, that the existence of unquestionable cases of yellow fever was universally admitted. The reports of the first cases, contained in the last number of this journal, give no reason for supposing that they owed their origin to an imported cause, even if the *doubtful* cases be excluded. However, there are members of the Board of Health who deem it probable, or possible, that the fever of this year was imported from Havana, viâ Mobile. By this route, some Spanish fruit-crers kept up constant communication between New Orleans and Havana. Some of them died of yellow fever in Mobile, and one at least here, among the first generally acknowledged cases.

Whether the fever originated in New Orleans from this cause, or not, it in either case demonstrates (if this disease be importable,) the possibility of its being imported by this route in future, and the incapacity of the present quarantine to protect New Orleans. So long as we have a quaratine, the Legislature should, without loss of time, give the Board of Health ample power to protect New

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Orleans from every quarter—Mobile as well as Havana—by which the disease may be admitted. Farther, if, in spite of every precaution, the disease be again located in our midst, the Board should be empowered to *isolate* it in any manner deemed most likely to prove effectual. So long as the State is trying *an experiment*, let it be a satisfactory one, which will demonstrate beyond cavil the efficiency or inefficiency of quarantine; and set at rest if possible, the expensive dispute between the contagionists and anti-contagionists.

CHAILLÉ.

JOURNALISTIC MUSINGS.

THE sideral year of 1857 lands this periodical on the verge of the new year, and the new volume, the FIFTEENTH. Standing on the narrow isthmus of the present, without daring to divine the impenetrable, yet hopeful future, this Journal can point to, and say of its irrevocable past—its FOURTEEN annual volumes—what Cornelia, the Roman matron, said of her children, "These are my jewels!"

Estimating the average number of subscribers to this Journal at several hundred below its present list, and allowing that each had during each year of the Journal's duration, five hundred patients, the number of these latter treated during fourteen years, will have been seven millions! It is believed that not one of these subscribers has failed to glean from this Journal some valuable practical knowledge, or hint, which has aided him in the treatment of the diseases of Southern latitudes.

The extreme sunny side of this Republic is, unfortunately, without publishing houses. This Journal is now published at more than twice the expense that it would cost in New York. Nevertheless, it is believed that the *re-publication* of three or four volumes, selected from the whole series, would be more valuable to physicians practising in the South, than the importation of any similar number of books from foreign lands. This is no vain boast. The writer expressly excepts from this favorable estimate his own humble contributions. If utility, not money, be a criterion whereby to judge of an undertaking, the founding and the upholding of this Journal, (thanks to FENNER and the lamented HESTER,

the pioneers in the same,) few events in the history of Louisiana, have proved more important to the well-being of society, than the mission and the benefits of the New Orleans Medical and Surgical Journal.

Kind contributors! "the harvest is great, the laborers few," continue to coöperate in the cause of science and humanity. The grateful acknowledgements of the editors, and still more, the gratitude of the numerous readers of this Journal, and more than all, the approval of your own consciences and the pleasure of curing or mitigating many of "the ills flesh is heir to," all these are yours; and, when you descend to the tomb—alas! how many eyes that have perused this Journal, are now closed forever—how many hands that contributed to its pages, have "lost their cunning"—I repeat it, when you shall have descended to the tomb, your successors will "read, study and inwardly digest" your medical histories, researches, and discoveries, and bless your memory. Your essays in the present number of this Journal will, in a few days, be read by (counting subscribers, including firms, students and others) as large number probably as the aggregate classes of all the medical professors in this Republic. As you have received instruction from the dead and living, give the same freely, faithfully—ceasing to work only when ceasing to live: "*Vita, sine literis, mors est*"

Most bountiful, salubrious, and august Eighteen Hundred and Fifty-Seven, adieu, forever! Hail! green, flowery, and genial Winter of Eighteen Hundred Fifty-Eight! The sublime apostrophe, which Goëthe sang to the Setting Sun in a distant land, is more than realised in the sea, lakes, rivers, streams, and verdant plains of our beautiful Louisiana! "See how the green-girt cottages shimmer in the setting sun! He bends and sinks—the day is over-lived. Yonder he hurries off, and quickens other life. Oh! that I have no wings to lift me from the ground, to struggle after, forever after him! I should see in everlasting evening beams, the stilly world at my feet, every height on fire, every vale in repose, the silver brook flowing into golden streams. I hurry on to drink his everlasting light—the day before me and the night behind, the heaven above, and under me the waves."

The patrons of this Journal will perceive that the present number surpasses all its predecessors in the amount of its original contributions. The quality, not less than the quantity of these papers, excepting the aberrant one by "the managing editor" (as his honorable co-editors have voluntarily designated him) will, it is believed, proved satisfactory.

BENNET DOWLER.

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL

FOR MARCH, 1858.

ORIGINAL COMMUNICATIONS.

ART. I.—*Charity Hospital Reports:* By W. P. RIDDELL, M. D.; PROF. STONE'S *Clinic:* Wednesday, January 13, 1858.

CASE I.—*Gun-shot Wound in the Thumb.*—In this case, you see that most of the fleshy part of the thumb is torn away, and the injury extends even to the palm of the hand. This is one of those cases in which, most generally, immediate amputation would have been resorted to; and, indeed, it might perhaps have been as well. But the sensibility was very great, and the part much inflamed, attended by a copious discharge of fluids. There was great pain, with excitement, which was more nervous than arterial. In such cases the small veins are very apt to bleed. My method, in such cases, is to clip off what is dangling and apply the dressings, and then await the operations of nature. It is astonishing how wounds will sometimes heal under this treatment. This is especially the case with lacerations of the extremities, and most especially the hand.

I gave tartar emetic, combined with morphia, to aid in subduing the excitement and to calm the pulse; when combined with tartar emetic less anodyne is necessary. I have often noticed, that in this way one-third or one-fourth of the usual quantity of morphia will answer the purpose perfectly. You see that this has begun to heal. You see the granulations. This thumb will get well, but be of no great use as the injury was very extensive.

It is not, however, unfrequently the case that limbs terribly lacerated may be saved, and made to answer many practical uses, although at first sight you might be inclined to pronounce them absolutely hopeless.

CASE II.—*Tumor around the Hip Joint.*—I here show the manner in which I apply caustic along the spine, to produce issues. Here are two, one on each side, just above the hips. The cauterization may be effected by caustic potassa, or, what is called Vienna paste, (*pâte de Vienne*). I

employed what is called the *French caustic pastille;* which, I suppose, is potassa and lime mixed. It does not dissolve as quickly as potassa alone would. A hole is cut through a piece of sticking-plaster, which is then put wherever you wish to apply the caustic. The plaster prevents the spreading of the caustic. In this application of caustic, to control or modify adjacent inflammations, or tendencies to suppuration, we avail ourselves of a principle, which we call *counter irritation.* That term does not fully express the effect which we here produce—the action seems to be peculiar; it is direct and health giving, being at the same time a stimulant and an alterative.

When you have once established an issue, you may allow the sore to granulate and heal; if the granulations should be unhealthy, apply sulphate of copper. There is no use in keeping up the drain from one sore; if necessary, you can establish new ones in other places, which, in turn, are to be treated in the same manner.

This patient has been here before, having been sick from the same cause for three or four years. He had an inflammation around the hip-joint, apparently rheumatic, and complained of much pain. Blisters and morphine ointment were applied; he improved, and left the hospital. Again, this fall, the pain returned, and he came back.

I now find a swelling inside of the thigh. He has considerable pain, which extends up along the back. The tumor itself is not particularly painful, which is a suspicious circumstance. The joint is not sore. I do not think the suppuration extends to it. You observe, that I can force the tumor up, and partly into the abdomen, which does not give much or any pain; the pain is along the nerves. The matter, I therefore suppose, comes from some higher source, and is simply conducted down to the hip by the peculiar anatomical structure of the muscles of these regions. The *psoas magnus* and *iliacus internus* muscles, together with their *fascia* forming a kind of funnel, which receives and conducts down all matter that may be discharged by the spine.

Believing, then, that this matter came from above, I examined, and found that the patient had a good deal of pain along the back. These issues, have relieved him. Whenever the pain develops itself in any particular spot, I shall there renew the issues. The tumor is smaller than it was three weeks ago, and the patient seems to be improving. He at first desired to have the tumor punctured. If such a treatment had been resorted to, (if my diagnosis be correct,) a permanent drain would have been established, for which his system is inadequate. Hectic would have followed. The air having got access, also, would have had a very dele-

terious influence; and the patient would, undoubtedly, have wasted and sunk.

I have been giving him very little medicine. In this case, as in many others which I have seen of injuries to the spine, there is very little or no apparent deformity. I have seen extensive ulcerations, where there was no deformity. If the ulceration be on one side, there may be no particular tenderness along the spine. You must recollect, however, that these patients are all very sensitive, and may acknowledge a pain in any place to which you call their particular attention. Some will have a pain wherever the doctor wants them to have it. There will be points peculiarly sensitive; and, if you place your finger upon them, the patient admits that that is *the spot*, and you diagnosticate at once "*Spinal Disease.*" Many such patients outlive their doctors. You cannot proceed with too great caution in making your examinations and drawing your conclusions.

Case III.—*Hard Tumor on the Nates.*—This is a peculiar disease, of no great importance or consequence, but is sometimes troublesome and vexatious. I have seen these little tumors form on those who are accustomed to ride much on horseback: sometimes they form about the fundament, *and, generally,* in any place exposed to an undue amount of jostling and bruising. Frequently they are small, like a bullet; at other times, more diffused. Their course is, to break down and slough away; but they proceed very slowly. They are like the carbuncle, but less acute. The ulcers which they leave behind are also sluggish, having deep cavities with ragged edges. If we could see these tumors early, or in the beginning, we might put a stop to them, by the stimulation of iodine or caustic. "Magendie's vesicating ointment" will answer the same purpose.

In this case, we can only slit it freely to facilitate the discharge, and apply stimulants to induce a healthier action. (*Operation of slitting and introduction of lint.*) Sometimes we are compelled to treat these little tumors quite roughly: although they are so simple, apparently, they will never get well without aid.

Case IV.—*Ulcer on the Leg, a little above the Ankle.*—This was an eating, painful ulcer when the patient first came into the hospital. Poultices gave no relief. I applied Magendie's ointment, with good effect. (The ointment I usually have prepared specially, as occasion may require. That which is purchased at the shops being, not unfrequently, much deteriorated and quite inferior.)

I employ calomel, ʒ ss; iodine, grs. x; and cerate, ʒ i.

This ointment is more painful than the fly blister, but does not destroy tissues; it is stimulant, but not caustic. The application of this ointment was followed, I believe, by a little chloroform ointment, and then by one of white precipitate, oxide of zinc, and glycerine—the precipitate being ʒi to the ʒi. This patient wants to go out of the Hospital, but if his sores be left uncovered, they will again break out. There is a stagnation of the venous circulation. If the leg be kept bandaged, the sores will get well and remain so. The new formed tissues will become more perfect with time.

CASE V.—*Fracture of the Clavicle.*—This man has one of those peculiarly crooked clavicles, which is very difficult to keep in place. There is not sufficient inflammation, and the bones are not disposed to unite. The patient has been wearing a kind of sling, which, as you see, did not answer the purpose of keeping the bones in contact. I will apply a bandage. It is usual to put a large pad under the arm to keep the shoulder out. I believe it is more generally hurtful than otherwise. (Bandage adjusted.) You see that this bandage elevates the arm, and brings the shoulder back as it should. In fact, it is just what is wanted; but the difficulty is in keeping it in place. It must be pinned and sewed if the patient be very uneasy and restless. The bandage forms, you see, a double triangle, one on the back and one on the breast.

CASE VI.—*Ptosis; Ophthalmoplegia.*—This is a case of some interest. You may recollect that this patient was here last year, and had paralysis of the eyelids; also a paralysis of some of the muscles which control the movements of the ball—more especially the internal recti. His face was swollen; the malar bones enlarged. He passed painful and sleepless nights. It looked as if the brain was affected. I however, considered it a case of syphilitic ostitis. He was accordingly put upon the mercurial treatment for tertiary syphilis, and then upon the iodide of potassium. He improved — has since returned to the Hospital several times; says he has not been taking any medicines lately. I have no doubt, with all due defference to the gentleman's judgment and opinion, that his diet has been too high, and that his present relapse was in a great measure produced thereby.

We all eat too much; far more than is necessary or conducive to our health. It has been estimated by an ingenious English writer that a good English dinner would be sufficient to answer the Hindoo for ten days; we are no better than the English in this respect. It is when we are *dieting* that the efforts of Nature are most efficient in eliminating and casting off the poisons which may be accidentally absorbed into the

system. Excess in eating excites, inflames and develops the disease. The same is true in tuberculosis.

This patient may be considered as relieved. You see he has an imperfect, but much improved, control of the eyes; his color has returned and his general health is pretty good.

Art. II.— *Ethnology of the Negro or Prognathous Race. A Lecture delivered Nov.* 30, 1857, *before the New Orleans Academy of Sciences, by* Sam'l A. Cartwright, M. D.

The natural history of mankind is divided into two parts—Ethnography and Ethnology. The former will be passed over, as it treats of the physical differences among men, their geographical distribution, history, origin, etc.; and the latter only will be considered, or that science which investigates the moral and mental differences among the different groups of mankind, and searches for the laws on which they depend. There are three principal groups, each of which has maintained the physical traits and mental characteristics peculiar to it, unaltered by time, circumstances or adventitious causes, for a period as far back as history extends. Hence they are called primordial, or primitive. Natural historians designate them, as the white, yellow and black: otherwise, the Indo-European, Mongolian and Prognathous.

The ethnology of the Prognathous or negro race, is the subject under consideration.

When we look around us, we behold four millions of that group of mankind engaged mostly in agriculture, and under subjection to that other group, called Indo-European, Indo-Germanic, Arien, Caucasian, or white. Ethnology is interrogated, to know if this be their *normal condition?* It answers in the affirmative, and its reasons are called for. The question has been answered variously, by religionists and politicians, which has given rise to much angry disputations, recriminations, costly experiments, useless restrictions, senseless alarms and disquietude—all of which might have been prevented, if the subject had been examined as an ethnological one.

Every one of the three primordial groups of mankind, and the subdivisions of those groups into tribes and nations, have their variables

instincts, appetites, likes and dislikes, habits, customs, intellectual capacities, moral standards, religious sentiments, etc., distinguishing one from the other—all of which have to be carefully observed through a long series of years before those elementary principles can be reached on which the science of ethnology rests as a basis.

George III. had not studied the ethnology of his colonial subjects in in America, or, it would have told him that it was impossible to bend their necks. The flames of Moscow taught Napoleon the ethnical truth, that Russians differ in instincts and habit of thought from Frenchmen—as he knew that nothing would tempt Frenchmen to burn Paris.

The English, Irish and Scotch, have very perceptible shades of difference in character. The whole trio differ essentially from their neighbors across the channel. Ten millions of them, the *bourgeoise*, at the head of civilized nations, differ essentially from the twenty-six millions of unimprovable peasantry, who are even behind the Hindoos and Chinese, as few of them can read or write ; whereas, there is scarcely an inhabitant of Hindostan or the Chinese empire who cannot read a book. There is something inherent in the French, which enables them to resist the diseases of this latitude better than most other Europeans, or our own people of the North and West. The inefficient tisane practice in acute diseases, so generally adopted by them, is a fearful trifling with life when imitated in the treatment of the acute diseases of the Englishman, German or American. Neither the same systems of government nor the same systems of medical practice are alike applicable to all nations and races of people.

Ethnology, therefore, disowns a common standard formula in government, civilization, morals and medicine, as alike applicable to all men. Where the ethnical elements are different, a common formula is impirical. Diversity of locality as well as diversity of races, should also be regarded. The yellow fever is more obedient to remedial agents in New Orleans than in localities further north. A negro never dies with it in any locality, when treated with regard to his ethnical peculiarities. So strong are they in his favor that, even under mal-practice, death is the exception—and recovery, the rule. A number of the prognathous race died in the epidemic of New Orleans, 1853, more from panic, it is believed, than from the yellow fever of that year. Experience and observation prove that panic is very apt to kill a negro, but it is questionable whether the yellow fever *per se* has that power. So seldom does it attack individuals of that race, that Dr. Rush was disposed to regard the negroes as black angels, sent by a kind Providence to nurse the sick during the terrible yellow fever which ravaged our Northern cities for a number of years in

succession, about the close of the last century and the beginning of the present. Much abolition capital was made out of the idea that this exemption was due to a special Providence, conferring on them a special opportunity to display their gratitude to those who had advocated the doctrine of their equality. Not only the great Rush, but many other good and distinguished men have fallen into grave mistakes in regard to the prognathous race of mankind, in consequence of not setting in the light of ethnological science. That science proves that the moral and intellectual diversities between the prognathous and Indo-European races, are actually greater than their physical. These diversities have continued as far back as history extends, unalterably the same in every climate and under every variety of circumstances, short of amalgamation. In that case, the offspring is a *tertium quid*, unlike either father or mother, and incapable of perpetuating its existence beyond a few generations. Dr. Day, of the Winnebago agency, avers, that the offspring of the Indian and negro has so little vitality that it ceases to exist after the fourth generation. The Mexicans, a mixture of the Indian, negro and white races, are dying out. The hybrid, from a mixture of the negro with the dark skinned European nations, has more vitality than the Irish, English or American hybrid.

Numbers definite in value reign supreme, unaltered and unalterable, throughout the vast domains of inorganic nature. All the simple elements have separate combining numbers peculiar to them. The combining number of oxygen never varies and never can. Its potential equivalent is always the same. It is the only element capable of forming combinations with all the other elements. Hydrogen is an element, which has not the same capacity for combination as oxygen. It is just as impossible to elevate hydrogen in its combining capacities, as to degrade oxygen. They differ in their potential equivalents as one to eight. It is beyond the power of man, or the chemist's art, to make them equal.

If politicians or religionists should take it in their heads to do so, on some *a priori* reasoning, that being elements at the base of the inorganic kingdom they ought to be equal in potency ; and that it was only for the want of well-directed experiments that the latent capacities of hydrogen remained concealed ; the chemist could tell them that they might waste millions on such experiments, or destroy empires, to elevate one or depress the other, and they would be no nearer bringing them to a level than when they first began. So, also, in the organic kingdom, it is just as impossible to add to or subtract from the inherent potency that nature has measured out to the different types of mankind. The prognathous type or hydrogen man, after all the costly experiments that have been

made to elevate him, remains the same that he was and ever will be in inherent potency, when compared to the Indo-European. Like an inferior planet, he can be drawn out of his natural orbit by a race of superior potency to his own, but sinks into his original status again as soon as he ceases to be acted on and sustained by the exterior power that elevated him out of it. All history proves, that when left to itself the prognathous race has never originated a civilization of its own, or sustained one that had been given to it.

The facts, gathered from all quarters of the extensive domain of ethnological science, have been compared and generalized. They are harmonious in declaring that each of the three great groups of mankind has its own special mode of development, its own special potency and series of expansions into a result, beyond which it cannot pass; that result being the termination of its progress, expresses the realization of the end and object of its creation. The ancient Greeks, in oratory, poetry, painting and sculpture, progressed by successive unfoldings until they arrived at an ultimatum, beyond which the Indo-European race cannot go—its inherent special potency having stretched itself to its utmost tension. The Chinese reached the ultimatum of progress that the Mongolian race is capable of attaining. That progress stopped far short of the point the Greeks reached. The ultimate limit of progress the prognathous race has ever made, stops within the confines of barbarism. There its inherent vital potency gives out and can carry it no further. While the yellow types progressed beyond, but came to a stand-still in the fields lying between barbarism and civilization. None but the white type has ever forced its way and maintained its position in that high order of civilization where moral virtue, clad in intellectual light, rules society. The yellow types, or the Arabs, for instance, in the Medieval ages, have often by adventitious circumstances, been brought within the realms of intellectual and moral light, but have invariably fallen back again into the twilight of semi-civilization, their instincts rebelling against the restraints which the moral influence of that higher order of civilization, whose woman is regarded as the better half of man, imposed on them. Their instincts dragged them down to that lower order of civilization, (*if it can be called civilization,*) where the female is regarded as an inferior being and treated as a slave. The Indo-European, or white man, whether in the civilized or savage state, has always instinctively regarded and treated woman as a companion more worthy and entitled to more respect than himself. Cæsar was so well apprised of this ethnical fact that in treaties made with white savages (the Germans) he demanded their women for hostages, well knowing that they put a higher value

upon them than upon their most renowned warriors. It is all a mistake about civilization giving liberty to woman, or elevating her in man's estimation. He owes his civilization to her; because he begins to lose it when out of her presence or cut off from her society. So far from any of the yellow or black races, tribes or nations, regarding her as a divinity or as a companion, they all look upon her as a being of an inferior caste, and enslave her. The Chinese, the highest among them, deny to woman the right to choose husbands; the husband's power is unlimited over the wife. She is not only his slave, but is bound to obey every member of his family — his brothers, sisters, uncles and aunts. The prognathous Australians obtain their wives by knocking them down with a club and wounding them in the foot to prevent them from running away. The Nigritians, or Africans proper, not only enslave their wives, but pave their court-yards with the skulls of the refractory or disobedient. Wealth and power acquired by the husband, so far from elevating the wives, add to their degredation by being used to increase their number. The king of Dahomey, according to the most authentic accounts, has 3333 living wives, besides a vast number whom he has capriciously murdered. Nor does, what is called negro freedom, elevate the colored women, but sinks them lower. The husbands hold them in abject slavery; they dare not kill them, as in Africa, but they beat and maltreat them in the cellars of New York, and other places in the Northern States, which they dare not do in the South. The freedom of the husband, is a loss to the women and children. They are in slavery still, and have lost their white protectors. While they and their husbands were in subordination to the white man, equal rights and equality among all the colored population, male and female, young and old, are sedulously maintained, as indispensable on every Southern plantation, all of which is lost to three-fourths of the black population, the women and children, by what is called negro freedom. Nor are the negro men themselves free in anything but name. A late celebrated ethnologist, De Gobineau, adduces Hayti as a glaring instance of the futility of the attempt to give a people institutions not suggested by their own genius or instincts. He says, "that there, as in Africa, the negro drinks ardent spirits, butchers his enemies, propitiates his sorcerers, and the rest of the time sleeps."

Under all the appliances of British power to prevent it, the prognathous race of the British possessions on this continent are going the same way as in Hayti. The colonies at Liberia and Sierre Leone would long since have been lost among savages, but for the almost superhuman exertions of the missionaries, and the fortunate aid derived from the

apprenticeship system, subjecting the wild Africans to a species of slavery to the indolent colonists.

The Nilotic monuments furnish numerous portraits of the negro race, represented as slaves, sixteen hundred years before the Christian era. Although repeatedly drawn from their native barbarism and carried among civilized nations, they soon forget what they learn and relapse into barbarism. If the inherent potency of the prognathous type of mankind had been greater than it actually is, sufficiently great to give it the independence of character that the American Indians possess, the world would have been in a great measure deprived of cotton and sugar. The red man is unavailable as a laberor in the cane or cotton field, or any where else, owing to the unalterable ethnical laws of his character. The white man cannot endure toil under the burning sun of the cane and cotton field, and live to enjoy the fruits of his labor. The African will starve rather than engage in a regular system of agricultural labor, unless impelled by the stronger will of the white man. When thus impelled, experience proves that he is much happier, during the hours of labor in the sunny fields, than when dozing in his native woods and jungles. He is also eminently qualified for a number of employments, which the instincts of the white man regard as degrading. If the white man be forced by necessity into employments abhorrent to his instincts, it tends to weaken or destroy that sentiment or principle of honor or duty, which is the mainspring of heroic actions from the beginning of historical times to the present, and is the basis of everything great and noble in all grades of white society. The importance of having those particular employments, regarded as servile and degrading by the white man, attended to by the black race, whose instincts are not repugnant to them, will be at once apparent to all those who deem the sentiment of honor or duty as worth cultivating in the human breast. It is utterly unknown to the prognathous race of mankind, and has no place in their language. When the language is given to them they cannot comprehend its meaning, or form a conception of what is meant by it. Every white man, who has not been degraded, had rather be engaged in the most laborious employments, than to seem as a lacquey or body-servant to another white man, or being like himself. Whereas, there is no office, which the negro or mulatto covets more than that of being a body servant to a real gentleman. There is no office which gives him such a high opinion of himself, and it is utterly impossible for him to attach the idea of degredation to it. Those identical offices, which the white man instinctively abhors, are the most greedily sought for by negroes and mulattoes, whether slave or free, in preferrence to all other employments.

North or South, free or slave, they are ever at the elbow, behind the table, in hotels and steamboats; ever ready, with brush in hand, to brush the coat or black the shoes, or to perform any menial service which may be required, and to hold out the open palm for the dime. The innate love to act as body-servant or lacquey, is too strongly developed in the negro race to be concealed. It admirably qualifies them for waiters and house-servants, as their strong muscles, hardy frames, and the positive pleasure that labor in a hot sun confers on them, abundantly qualify them for agricultural employments in a hot climate.

Hence, the primordial cell-germ of the Nigritian has no more potency than what is sufficient to form a being with physical power, when its dynanism becomes exhausted, dropping the creature in the wilderness with the mental organization too imperfect to enable him to extricate himself from barbarism. If Nature had intended the prognathous race for barbarians as the end and object of their creation, they would have been like lions and tigers, fierce and untamable. So far from being like ferocious beasts, they are endowed with a will so weak, passions so easily subdued, and dispositions so gentle and affectionate, as readily to fall under subjection to the wild Arab, or any other race of men. Hence they are led about in gangs of an hundred or more by a single individual, even by an old man, or a cripple, if he be of the white race and possessed of a strong will. The Nigritian has such little command over his own muscles, from the weakness of his will, as almost to starve, when a little caution and forethought would procure him an abundance. Although he has exaggerated appetites and exaggerated senses, calling loudly for their gratification, his will is too weak to command his muscles to engage in such kinds of labor as would readily produce the fruits to gratify them. Like an animal in a state of hibernation, waiting for the external aid of spring to warm it into life and power, so does the negro continue to doze out a vegeto-animal existence in the wilderness, unable to extricate himself therefrom — his own will being too feeble to call forth the requisite muscular exertion. His muscles not being exercised, the respiration is imperfect, and the blood is imperfectly vitalized. Torpidity of body and hebetude of mind are the effects thereof, which disappear under bodily labor, because that expands the lungs, vitilizes the blood and wakes him up to a sense of pleasure and happiness unknown to him in the vegeto-animal or hibernating state. Nothing but will is wanting to transform the torpid, unhappy tenant of the wilderness into a rational and happy being — the happiest being on earth, as far as sensual pleasures are concerned. The white man has an exaggerated will — more than he has use for; because it frequently drives his own

muscles beyond their physical capacity of endurance. The will is not a faculty confined within the periphery of the body. It cannot, like the imagination, travel to immeasurable distance from the body, and in an instant of time go and return from Aldabaran, or beyond the boundaries of the solar system. Its flight is confined to the world and to limits more or less restricted—less restricted in some than in others. The will has two powers — direct and indirect. It is the direct motor power of the muscular system. It indirectly exerts a dynamic force upon surrounding objects when associated with knowledge. It gives to knowledge its power. Everything that is made was made by the Infinite Will associated with Infinite knowledge. The will of man is but a spark of the Infinite Will, and its power is only circumscribed by his knowledge. A man possessing a knowledge of the negro character can govern an hundred, a thousand or ten thousand of the prognathous race by his will alone, easier than one ignorant of that character can govern a single individual of that race by the whip or a club. However disinclined to labor the negroes may be, they cannot help themselves; they are obliged to move and to exercise their muscles when the white man, acquainted with their character, *wills* that they should do so. They cannot resist that will, so far as labor of body is concerned. If they resist, it is from some other cause than that connected with their daily labor. They have an instinctive feeling of obedience to the stronger will of the white man, requiring nothing more than moderate labor. So far, their instincts compel obedience to his will as one of his rights. Beyond that, they will resist his will and be refractory, if he encroaches on what they regard as their rights, viz., the right to hold property in him as he does in them, and to disburse that property to them in the shape of meat, bread and vegetables, clothing, fuel and house room, and attention to their comforts when sick, old, infirm and unable to labor; to hold property in him as a conservator of the peace among themselves, and a protector against trespassers from abroad, whether black or white; to hold property in him as an impartial judge and an honest jury to try them for offences, and a merciful executioner to punish them for violations of the laws or usages of the plantation or locality. With those rights conceded to them, no other compulsion is necessary to make them perform their daily tasks than *his will alone*. It is not the whip, as many suppose, which calls forth those muscular exertions, the result of which is sugar, cotton, breadstuffs, rice and tobacco. These are products of the white man's will acting through the muscles of the prognathous race in our Southern States. If that will were withdrawn, and the plantations handed over as a gracious gift to the laborers, agricultural labor would

cease for the want of that spiritual power, called the will, to move the machines — the muscles. They would cease to move here, as they have in Hayti. If the prognathous race were expelled the land, and their place supplied with double their number of white men, agricultural labor in the South would also cease, as far as sugar and cotton are concerned, for the want of muscles that could endure exercise in the smothering heat of a cane or cotton field. Half the white laborers of Illinois are prostrated with fevers from a few days' work in stripping blades in a Northern cornfield, owing to the confinement of the air by the close proximity of the plants. Cane and cotton plants form a denser foliage than corn; a thick jungle, where the white man pants for breath, and is overpowered by the heat of the sun at one time of day, and chilled by the dews and moisture of the plants at another. Negroes glory in a close, hot atmosphere; they instinctively cover their heads and faces with a blanket at night, and prefer lying with their heads to the fire, instead of their feet. This ethnical peculiarity is in harmony with their efficiency as laborers in a hot, damp, close, suffocating atmosphere—where, instead of suffering and dying, as the white man would, they are healthier, happier and more prolific than in their native Africa — producing, under the white man's will, a great variety of agricultural products, besides upwards of three millions of bales of cotton, and three hundred thousand hogsheads of sugar. Thus proving that subjection to his will is normal to them, because, under the influence of his will, they enjoy life more than in any other condition, rapidly increase in numbers, and steadily rise in the scale of humanity.

The power of a stronger will over a weaker, or the power of one living creature to act on and influence another, is an ordinance of Nature which has its parallel in the inorganic kingdom, where ponderous bodies, widely separated in space, influence one another so much as to keep up a constant interplay of action and re-action throughout Nature's vast realms. The same ordinance, which keeps the spheres in their orbits and holds the satellites in subordination to the planets, is the ordinance that subjects the negro race to the empire of the white man's will. From that ordinance the snake derives its power to charm the bird, and the magician his power to amuse the curious, to astonish the vulgar, and to confound the wisdom of the wise. Under that ordinance, our four millions of negroes are as unalterably bound to obey the white man's will, as the four satallites of Jupiter, the superior magnetism of that planet. Individual masters, by releasing individual negroes from the power of their will, cannot make them free or release them from subordination to the instinctive public sentiment or will of the aggregate white

pulation, which as rigidly excludes them, in the so-called free States, from the drawing room and parlor as it does pots and kettles and other kinds of kitchen furniture. The subjugation of equals to equals by artifice or force is tyranny or slavery; but there is no such thing in the United States, because equals are on a perfect equality here. The subordination of the Nigritian to the Caucasian would never have been imagined to be a condition similar to European slavery, if any regard had been paid to ethnology. Subordination of the inferior race to the superior is a normal, and not a forced condition. Chains and standing armies are the implements used to force the obedience of equals to equal — of one white man to another. Whereas, the obedience of the Nigritian to the Caucasian is *spontaneous*, because it is normal for the weaker will to yield obedience to the stronger. The ordinance which subjects the negro to the empire of the white man's will, was plainly written on the heavens during our Revolutionary war. It was then that the power of the united will of the American people rose to its highest degree of intensity. Every colony was a slave holding colony excepting one; yet the people, particularly that portion of them residing in districts where the black population was greatest, hastened to meet in the battle-field the powerful British armies in front of them, and the interminable hosts of Indian warriors in the wilderness behind them, leaving their wives and children, their old men and cripples, for seven long years, *to their negroes to take care of*. Did the slaves, many of whom were savages recently imported from Africa, butcher them, as white or Indian slaves surely would have done, and fly to the enemy's standard for the liberty, land, money, rum, savage luxuries, and ample protection so abundantly promised and secured to all who would desert their master's families? History answers that not one in a thousand joined their master's enemies; but on the contrary, they continued quietly their daily labors, even in those districts where they outnumbered the white population ten to one. They not only produced sufficient breadstuffs to supply the families of their masters, but a surplus of flour, pork and beef was sent up from the slave holding districts of Virginia to Washington's starving army in Pennsylvania. (See Botta's History.) These agricultural products were created by the labor of savages, naturally so indolent in their native Africa, as to prefer to live on ant eggs and caterpillars rather than labor for a subsistence; but for years in succession they continued to labor in the midst of their masters' enemies—dropping their hoes when they saw the red-coats, running to tell their mistress, and to conduct her and the children through by-paths to avoid the British troopers, and when the enemy were out of sight, returning to their

work again. The sole cause of their industry and fidelity is due to spiritual influence of the white race over the black.

The empire of the white man's will over the prognathous race is not absolute, however. It cannot force exercise beyond a certain speed; neither the will nor physical force can drive negroes, for a number of days in succession, beyond a very moderate daily labor — about one-third less than what the white man voluntarily imposes on himself. If force be used to make them do more, they invariably do less and less, until they fall into a state of impassivity, in which they are more plague than profit—worthless as laborers, insensible and indifferent to punishment, or even to life; or, in other words, they fall into the disease which I have named Dysesthæsia Ethiopica, characterized by hebetude of mind and insensibility of body, caused by overworking and bad treatment. Some knowledge of the ethnology of the prognathous race is absolutely necessary for the prevention and cure of this malady in all its various forms and stages. Dirt eating, or Cachexia Africana, is another disease, like Dysesthæsia Ethiopica, growing out of ethnical elements peculiar to the prognathous race. The ethnical elements assimilating the negro to the mule, when compared with the horse, although giving rise to the last named disease, are of vast importance to the prognathous race, because they guarantee to that race an ample protection against the abuses of arbitrary power. A white man, like a blooded horse, can be worked to death. Not so the negro, whose ethnical elements, like the mule, restrict the limits of arbitrary power over him. Among the four millions of the prognathous race in the United States, it will be difficult, if not impossible, to find a single individual negro, whom the white man, armed with arbitrary power, has ever been able to make him hurt himself at work. It is beyond the power of the white man to drive the negro into those long continued and excessive muscular exertions, such as the white laborers of Europe often impose upon themselves to satisfy a greedy boss, under fear of losing their places and thereby starving themselves and families. Throughout England, nothing is more common than decrepitude, premature old age, and a frightful list of diseases, caused by long continued and excessive muscular exertion. Whereas, all America can scarcely furnish an example of the kind among the prognathous race. The white men of America have performed many prodigies, but they have never yet been able to make a negro overwork himself.

There are other elements peculiar to the Nigritian, on which the disease, called negro consumption, or Cachexia Africana, depends. But these belong to that class which subject the negro to the white man's

spiritual empire over him. When that spiritual empire is not maintained in all its entireity, or in other words, when the negro is badly governed, he is apt to fall under the spiritual influence of the artful and designing of his own color, and Cachexia Africana, or consumption, is the consequence. Better throw medicine to the dogs, than give it to a negro patient impressed with the belief that he has walked over poison specially laid for him, or been in some other way tricked or conjured. He will surely die, unless treated in accordance with his ethnological pecularities, and the hallucination expelled.

There never has been an insurrection of the prognathous race against their masters; and from the nature of the ethnical elements of that race, there never can be. Hayti is no exception, it will be seen, when the true history of the so-called insurrection of that island is written. There have been neighborhood disturbances and blood shed, caused by fanaticism, and by mischevious white men getting among them and infusing their will into them, or mesmerizing them. But fortunately, there is an ethnological law of their nature which estops the evil influence of such characters by limiting their influence strictly to personal acquaintances. The prognathous tribes in every place and country are jealous and suspicious of all strangers, black or white, and have ever been so.

Prior to the Emancipation Act in the British West Indies, the famous Exeter Hall Junto sent out a number of emissaries of the East India Company to Jamaica, in the garb of missionaries. After remaining a year or two in the assumed character of Christian ministers, they began to preach insurrectionary doctrines, and caused a number of so-called insurrections to break out simultaneously in different parts of the island. The insurgents in every neighborhood were confined to the personal acquaintances of the Exeter Hall miscreants, who succeeded in infusing their will only into those who had listened to their incendiary harangues. This was proved upon them by the genuine missionaries, who had long been on the island, and had gathered into their various churches a vast number of converts. For, in no instance, did a single convert, or any other negro, join in the numerous insurrectionary movement, who had not been personally addressed by the wolves in sheep's clothing. The Christian missionaries, particularly the Methodist, Baptist, Moravians and Catholics, were very exact in collecting the evidences of this most important ethnological truth, in consequence of some of the planters, at the first outbreak, having confounded them with the Exeter Hall incendiaries. The planters finally left the Christian missionaries and their flocks undisturbed, but proceeded to expel the false missionaries, to hang their converts, and to burn down their chapels. The event proved that they

were wrong in not hanging the white incendiaries; because they went home to England, preached a crusade — traveling all over the United Kingdom — proclaiming, as they went, that they had left God's houses in flames throughout Jamaica, and God's people hanging like dogs from the trees in that sinful island. This so inflamed public sentiment in Great Britain against the planters, as to unite all parties in loud calls for the immediate passage of the Emancipation act. There is good reason to believe that the English Ministry, in view of the probable effect of that measure on the United States, and the encouragement it would afford to the culture of sugar and other tropical products in the East Indies and Mauritius, had previously determined to make negro freedom a leading measure in British policy, well knowing that its effect would be to Africanize the sugar and cotton growing regions of America. The ethnology of the prognathous race does not stop at proving that subordination to the white race is its normal condition. It goes further, and proves that social and political equality is abnormal to it, whether educated or not. Neither negroes nor mulattoes know how to use power when given to them. They always use it capriciously and tyranically. Tschudi, a Swiss naturalist, (see Tschudi's Travels in Peru, London, 1848,) says, "that in Lima and Peru generally, the free negroes are a plague to society. Dishonesty seems to be a part of their very nature. Free born negroes, admitted into the houses of wealthy families, and have received, in early life, a good education, and treated with kindness and liberality, do not differ from their uneducated brethren."

Tschudi is mistaken in supposing that dishonesty is too deeply rooted in the negro character to be removed. They are dishonest when in the abnormal condition without a master. They are also dishonest when in a state of subordination, called slavery, badly provided for and not properly disciplined and governed. But when properly disciplined, instructed and governed, and their animal wants provided for, it would be difficult to find a more honest, faithful and trustworthy people than they are. When made contented and happy, as they always should be, they reflect their master in their thoughts, morals and religion, or at least they are desirous of being like him. They imitate him in every thing, as far as their imitative faculties, which are very strong, will carry them. They take a pride in his wealth, or in anything which distinguishes him, as if they formed a part of himself, as they really do, being under the influence of his will, and in some measure assimilated, in their spiritual nature, to him — loving him with all the warm and devoted affection which children manifest to their parents. He is sure of their love and friendship, although all the world may forsake him. But to create and

maintain this happy relation, he must govern them with strict reference to their ethnological peculiarities. He must treat them as inferiors, not as equals, as they are not satisfied with equality, and will despise a master who attempts to raise any one or more of them to an equality with himself; because they become jealous and suspicious that their master's favorites will exercise a sinister influence over him against them. Impartiality of treatment in every particular, down to a hat or pair of shoes, is what they all regard as one of their dearest rights. Hence, any special favors or gifts to one, is an offence to all the rest. They also regard as a right, when punished, not to be punished in anger, but with cool deliberation. They will run from an angry or enraged master or overseer, armed with a gun or a pistol. They regard all overseers who come into the field armed with deadly weapons as cowards, and all cowards have great difficulty in governing them. It is not physical force which keeps them in subjection, but the spiritual force of the white man's will. One unarmed brave man can manage a thousand by the moral force of his will alone, much better than an hundred cowards with guns in their hands. They also require as a right when punished, to be punished with a switch or a whip, and not with stick or the fist. In this particular the ethnical law of their nature is different from all other races of men. It is exactly the reverse of that of the American Indian. The Indian will murder any man who strikes him with a switch, a cowhide or a whip twenty years afterwards, if he gets an opportunity; but readily forgives and forgets blows, however severe, inflicted on him with the fist, a cudgel or a tomahawk. A remarkable ethnological peculiarity of the prognathous race is, that any deserved punishment, inflicted on them with a switch, cowhide or whip, puts them into a good humor with themselves and the executioner of the punishment, provided he manifest satisfaction by regarding the offence as atoned for.

The negro requires government in every thing, the most minute. The Indian, on the contrary, submits to government in nothing whatever. Mr. Jefferson was the first to notice this ethnical law of the red man. [See his letter to Gilmer, June 7, 1816, vol. iv. page 279; *Jefferson's Correspondence.*] "Every man with them," (the Indians,) says Mr. Jefferson, "is perfectly free to follow his own inclinations; but if, in doing this, he violates the rights of another, he is punished by the disesteem of society or tomahawked. Their leaders conduct them by the influence of their character only; and they follow or not, as they please, him, of whose character for wisdom or war, they have the highest opinion; but of all things, they least think of subjecting themselves to the will of one man." Whereas, the black man requires government even in his meat

and drink, his clothing and hours of repose. Unless under the government of one man to prescribe rules of conduct to guide him, he will eat too much meat and not enough of bread and vegetables; he will not dress to suit the seasons, or kind of labor he is engaged in, nor retire to rest in due time to get sufficient sleep, but sit up and doze by the fire nearly all night. Nor will the women undress the children and put them regularly to bed. Nature is no law unto them. They let their children suffer and die, or unmercifully abuse them, unless the white man or woman prescribe rules in the nursery for them to go by. Whenever the white women superintends the nursery, whether the climate be cold or hot, they increase faster than any other people on the globe; but on large plantations, remote from her influence, the negro population invariably diminishes, unles the overseer take upon himself those duties in the lying-in and nursery department, which on small estates are attended to by the mistress. She often sits up at night with sick children and administers to their wants, when their own mothers are nodding by them, and would be sound asleep if it were not for her presence. The care that white women bestow on the nursery, is one of the principal causes why three hundred thousand Africans, originally imported into the territory of the United States, have increased to four millions; while in the British West Indies the number imported exceed, by several millions, the actual population. It is also the cause, why the small proprietors of negro property in Maryland, Virginia, Kentucky and Missouri are able to supply the loss on the large Southern plantations, which are cut off from the happy influence of the presiding genius over civilization, morality and population — the white woman.

The prognathous race require government also in their religious exercises, or they degenerate into fanatical saturnalia. A discreet white man or woman should always be present to regulate their religious meetings.

Here the investigation into the ethnology of the prognathous race must close, at least for the present, leaving the most interesting part, Fetechism, the indigenous religion of the African tribes, untouched. It is the key to the negro character — which is difficult to learn from mere experience. Those who are not accustomed to them have great trouble and difficulty in managing negroes; and in consequence thereof, treat them badly. If their ethnology was better and more generally understood, their value would be greatly increased, and their condition, as a laboring class, would be more enviable, compared to the European peasants, than it already is.

November 30, 1857.

ART. III.—*A History of the Diseases of Craven's Creek and its vicinity, from* 1848, *to the present time:* By JESSE PEEBLES, M. D., of Lowndes Co., Mississippi. (Continued from vol. xv, p. 39.)

TYPHOID pneumonia continued to rage during January and February, 1852 — prevailed to a less extent during March and April; I witnessed a very violent case in July. This disease, commenced, as has been sta ted, in the hills and traveled in a South-westerly direction; it did not, however, reach the flat lands to much extent till about March, after the epidemic had abated, and consequently there was not so much of it in that section as there had been in the hills. From November till May it traveled over a scope of country one mile wide by about nine miles in length, embracing the north prong of Craven's Creek, from its source to its junction with the south prong; thence following pretty nearly the course of the main creek to its mouth. Persons living out of this scope were entirely free from pneumonia, and of course there were a few families within it that escaped. The cases that occurred in the fall were confined entirely to the flat lands.

From January throughout the year, there was in many cases a symptom entirely new in pneumonia, or, at least, new to me; viz., after the inflammation of the lungs had run its course, and there was every indication of returning health — a natural pulse, cheerful countenance, etc., the extremities would gradually become icy cold, and the pulse without becoming frequent, but on the contrary, sometimes dropping down to 40 beats to the minute, would become very weak and thread-like, and what was equally remarkable, the patients themselves were never aware that they were not doing well; they were always, when in this condition, quite cheerful, and declared that they felt very well; nor did the countenance nor anything else, except the feeble circulation and cold skin, indicate that anything was wrong. The first case of the kind that I met with gave me great annoyance; every kind of stimulant had been tried in vain, and I had become alarmed. Opium was hit upon and gave prompt relief. After that, I never knew 75 drops of laudanum to fail in giving relief. Sometimes the relief was permanent and the laudanum did not require to be repeated; in other cases, it was sometimes necessary to continue it three or four days, giving it about twice in every twenty-four hours, gradually lessening the dose. In the spring, when the cases were generally mild, it sometimes happened that the extremities on *one side only* would become cold, those on the other side being of the natural temperature, and in these cases, though there was a very perceptible change in the pulse, it was never so weak as in those cases in which the coldness extended to both sides. If this coldness were *entirely*

the result of a feeble circulation, how happened it that it was sometimes confined to the extremities of one side only? When black negroes were in this condition their skins presented a singular dry and ashy appearance.

What would have been the result if these cases had been left to run their course? and what brought about this feeble state of the circulation and cold skin?

In February, I treated an acute case of pneumonia in a negro man, John; left him doing well, but neglected to direct that laudanum should be given if the state of things just described should take place. A week afterwards I was again sent for to see him. "John had been cold and pulseless nearly two days," but before I arrived he had had a hemorrhage from the lungs, and a reäction had taken place. Did not give him any medicine at all, and he soon got well. This was the only case of the kind that did not take laudanum, and that was left to recover in the natural way. All the other cases took opium. It always gave prompt and entire relief, and no other drug ever was of the least benefit; but from this fact is only drawn the vague conclusion that the nervous system was in some way concerned in the production of this strange condition of the system. That it was not caused by any malarial influence acting upon a system debilitated by sickness, is inferred from the fact that quinine never did any good in such cases, but on the contrary, was always injurious, at least so far as it produced buzzing and confusion in the ears, and gastric irritability.

Flux prevailed in the flat lands from April till August. A large majority of the cases were mild and of short duration; a few degenerated into a low continued form of fever, and some cases were rapidly fatal. In one family, numbering about a dozen of souls, all told, there were seven cases, and five out of that number died. I saw only a part of these cases; saw them but once, and shall not attempt a minute description of them, though they were certainly, in several respects, very remarkable, and might have been called bloody cholera; for there was copious hemorrhage from the bowels and nares, and a considerable oozing of blood from the gums; in one or two of these cases there was bloody effusion into the sub-cutaneous cellular tissue on different parts of the body. The two cases that recovered had a protracted convalescence, and one of them suffered severely from a number of abscesses. At the time that these cases occurred, and within half a mile of them, I treated the following case: Miss A. F. was taken on the evening of June the 8th, with ear-ache, which wore off during the night, and on the morning of the 9th she was taken with griping and frequent bloody mucous dejections. I saw her at noon; she was then suffering extremely, and had

high fever; she had also bled freely from the nose, but only once, and it had ceased. I gave her a dose of sulph. magnes., and directed it to be given every four hours in infusion of senna, till her griping was relieved, and her passages changed. On the 10th she was free from pain, and her evacuations were feculent, but her pulse was very quick and weak, and her abdomen swollen and very tender on pressure. Put her on camphor, morphia and carb. ammonia, and directed warm mustard poultices to be constantly kept to her abdomen. On the 11th, her symptoms were not so bad; the swelling had left her abdomen, which was less tender; her flux had not returned, and her pulse was better. But at night her earache returned, and on the 12th, both parotids and tonsils were inflamed and much swollen. She was cupped freely over the mastoid processes, and stimulating poultices kept constantly to her throat and jaws, but without benefit. She died on the 15th, before suppuration had taken place.

There was, in the hills, during the summer months, a continued form of fever for which I have no name; its duration was generally about four weeks. In no case was there any inflammation of the lungs, but in every case there was most or all of the other symptoms that marked the typhoid pneumonia of the preceding winter and spring, viz., inflammation of the frontal sinuses, of the conjunctivæ and of the parotid glands, black tongue, sordes, elongation and shedding of the gums, dysuria, albuminuria, flatulence and colicky pains. But there were wanting in every case, the cerebral disturbance, the obtuse sensibility, the sudamina, the rash, the diarrhœa, the contracted and pointed tongue, the gurgling, the tympanitis and the countenance of typhoid fever. In fact, I saw only one case of typhoid fever *per se*, during the whole year, and there was but very little intermittent or malarial fever of any kind. The pneumonia of the fall was milder than that of the spring; and from the middle of November till January, there was but very little sickness of any kind.

In January, 1853, influenza (?) commenced in the flat lands, mostly among negroes, and continued till about the middle of May. Many of the first cases were protracted typhoid, and exhibited throughout their course a strong tendency to death by asthenia. As spring approached, the cases were more acute, more decidedly pneumoniac and fearfully adynamic; but it was essentially a different disease from the pneumonia of the two previous years, 1851 and '52. There were two cases in which death took place within twenty-four hours from the time of the attack. These cases resembled congestive chills, except that there was crepitation all over the lungs and the perspiration, and quivering sensa-

sion in the bowels were wanting; but death was preceded by that kind of hiccough that attends fatal cases of congestive fever. I recollect one case that was almost pulseless for the first three days, and another in which there could be no pulse found any where; even the skin of the abdomen was cold during the first twelve hours, and for the next twenty-four the pulse at the wrist was barely perceptible; in both there was very distinct crepitation during the chill, and with reäction copious bloody expectoration took place. It was rather of a hemorrhagic character, of short duration, and recovery in both cases was very rapid after reäction had taken place.

In many instances it was necessary to stimulate highly during the first three or four days, and, afterwards to give lobelia, veratrum, etc. In some instances it was necessary to give brandy freely throughout the whole disease, and these cases were always protracted, lasting four or five weeks, and were among the first that occurred. In two cases death took place when, and in a manner, that I did not expect; the patients seemed to have catarrhal fever or influenza, but without any ugly symptom, and everything seemed to be going on well, when they were suddenly taken with violent pain just above the pubes, the pulse almost immediately sunk, and death followed in a day or two.

Did the same disease, modified by the season, continue through the following summer and fall? or, was the summer and fall sickness something essentially different from that of the preceding winter and spring?

The following are fair average cases of the summer and fall sickness of this year. Of course there was, as there always is in this country, some malarial fever — something that quinine will cure, during the summer months; but this is always an unmistakable disease, and no one would or could confound it with anything else, and it is considered here as a very small matter, comparatively.

W. H. K. was taken on the 1st of June with what he thought was bilious fever, and after an active purge, saturated himself with quinine; but not getting any better, I was sent for on the 4th. He then had high fever, though he was in a moderate perspiration, his respiration was natural, there was no cough nor crepitation, nor dulness on percussion over any part of his lungs; but on making a deep inspiration, there was slight pain just below the right scapula, of which he was relieved by cupping. He then took an opiate, and the influence of quinine having by that time entirely worn off, it was repeated to the extent of a drachm, given in ten grain doses, but without benefit, and as there was then no local disease detectible. He was closely watched, and his fever pretty much left to run its course. He was never entirely free from perspiration, but

he gradually lost ground, and about the tenth day of his disease, he was very suddenly taken with pain just above the pubes, and his pulse rapidly sunk; the pain was relieved by an opiate; he took brandy freely and recovered; but his convalescence was very tedious, and during the time he had near fifty carbuncles and large boils on different parts of his body. The amount of liquor that he drank in all was enormous; in health he had waged a war of extermination against it — his mode of destruction being to drink it all — and in sickness he swallowed like Ovid Bolus lied — with a relish and a coming appetite.

J. F. C. was taken at noon, July 13th, with chilliness and head-ache, followed by fever, which soon left him, but his skin had turned yellow, and his tongue had a singular dark greenish appearance, but as soon as his fever was off, he was up and declared that he felt very well—that he had only had a chill, and that quinine would cure him; and, accordingly, quinine he took through the night, in ten grain doses. The next morning he was up and complained of nothing but deafness from the quinine he had taken, and ate a hearty breakfast, but was soon afterwards taken with vomiting and severe pain in the lower part of his abdomen; but he had no fever, and his pulse remained *unaffected*. The pain was relieved by opium, after which he was purged. The next morning he was not up, but had no fever, and complained of nothing except that his abdomen was sore; this was relieved by a strong mustard poultice. By this time his skin was intensely yellow, which was partly attributed to the vomiting on the day before. He was again put on quinine in combination with a small quantity of blue pill. At night his fever rose again and never intermitted. The next day he had slight cough, but there was no crepitation, nor was there ever any, nor was there at any time dulness over any part of his lungs, though once during his illness, he had a smart pleuritic pain in his left side, which was relieved by cupping. His fever continued regularly to increase till about the eighth day, when he became inordinately delirious; his extremities began to get cold, and his pulse to sink. He died on the 22d, nine days from the time that he was taken. Throughout his sickness there was more or less irritability of the stomach, and towards the last, he vomited large quantities of a green watery fluid.

Mrs. W., six months advanced in pregnancy, after several days of lassitude was taken, August 1st, with pain in the head and limbs, and with fever. Labor pains commenced early on the morning of the 2d, and I was immediately sent for. I found her with high fever, intensely yellow skin, and peaked countenance. Her pains were frequent and regular, but feeble, and her stomach very irritable, rejecting every-

thing she swallowed. I learned that she was jaundiced several days before she took the fever or had vomited any. One grain of morphia per anum checked the pains, and quinine was given the same way till she was brought thoroughly under its influence, and kept so twenty-four hours. As it did no good in that time, it was discontinued. Cupping, blistering and anti-emetics having failed to check the vomiting, morphia was given by injection whenever there was any symptom of labor, and the rest left to Nature. She continued to vomit three or four days, first throwing up a yellow, then a green watery fluid, (not the *blue* fluid vomited after the use of morphia or any other sulphate,) which sometimes contained a large quantity of black particles resembling coffee grounds. On the eighth day her countenance cleared up, and on the ninth she sweated off her fever.

Mrs. M. J. P., was taken September 1st, with a chill which lasted about fourteen hours, and was followed by fever which continued, with irregular intermissions, nearly four weeks. Her skin became yellow early in the disease. The second and third weeks she had slight diarrhœa. During the exacerbations of fever she suffered great gastric irritation, and vomited the characteristic green fluid containing the particles resembling coffee grounds.

(*To be Continued.*)

ART. IV.—*Parotitis in the Aged:* By B. S. WARD, M. D.

THE only reason we offer for the following brief remarks, is the fact that, when concerned with mumps in old persons, we have frequently had recourse to practical works with the view of procuring information and assistance; but our researches have generally terminated unsatisfactorily, the authors consulted having treated the disease very lightly, assigning it but little importance. While in children we have generally found mumps what most authors represent it to be, a mild disease, getting well in a few days, and rarely confining to bed; yet in old persons, from fifty to sixty, we have as generally found it severe and not free from danger — requiring on the part of the medical attendant prompt attention, and a skilful administration of medicine.

The modifications presented by parotitis in the aged have not been sufficiently emphasized by teachers, although they are clear and striking.

Why mumps should present more violence in senility than in youth, we shall not attempt to explain. This question involves a nicety of physiological research to which we do not aspire, and an acquaintance with the *occult essence* of the disease which we do not claim. The fact that it does prove violent in the aged, is sufficient for us. We may state, however, that as old age advances, the capacity for resisting the effects of poison of any kind *within* the system diminishes. It may be likewise stated that the capacity for responding to the influence of poison from *without*, likewise diminishes. This arrangement seems to be a wise provision of Nature, since the human system, as it becomes less capable of withstanding disease, becomes less susceptible of infectious influence. An old man is not invaded as readily by contagious disease as a young one, but when invaded is less capable of resisting it. The "*Vis Medicatrix Naturæ*" is less energetic in old age, and as we conceive, this is the same thing as saying that the *physiological processes* are more feeble and less efficient; for we suppose the "healing power of Nature" is expressive only of the natural, or organic condition — the physiological status of the animal machine. In old age, the functions of nutrition and assimilation are comparatively inactive, and likewise the functions of secretion and excretion, through which channel the morbid poisons are in the main eliminated. We may infer then, that as age advances, the animal body becomes less *vital* and more *chemical*. Life is said to consist in the predominance of the *vital forces* over the chemical; between these forces a constant warfare is being waged. But as age increases, the physical forces are more and more advantaged, until finally an equilibrium may be reached in which the organism may be regarded as suspended between life and death. Soon after the chemical poisons obtain the mastery, and dissolution or organic death ensues.

The introduction within the system of any poison is not succeeded by that reactionary movement, or vital resistance, in old age to the same extent as in youth, or middle life; and hence the irregular and protracted recoveries and convalescences of old age. On this account diseases generally, which depend on a peculiar *materies morbi*, are more serious and perilous to them. But our observation teaches us that there may be some peculiarity in the very essence — in the nature of parotitis, that makes it a severe disease in old persons. It may be possible that this is owing to the partiality on the part of this disease for the glandular system. In the various metastases and local manifestations of mumps, we find invariably more or less disturbance in the liver, kidneys, testicles, mammæ, to say nothing of the parotid and other salivary glands which are the common *locale* of the disease, and give to it its name. The force of the disease is spent upon this system.

The invasion of mumps in the aged is, according to our experience, more sudden and violent than in youth. The appetite fails; a febrile movement is developed which in a short time, in many cases, assumes considerable intensity. The pulse, during the first three or four days, is full and quick; the temperature of the surface is much exalted; the skin, in many instances, harsh and dry. The bowels are generally constipated, and the urinary excretion scanty and highly colored. All the secretions undergo diminution in activity, and especially is this true of the hepatic. The stools are scanty in quantity, and either serous, or if consistent, of a whitish appearance. Complaint is often made of fulness and uneasiness in the region of the liver, and we have observed enlargement and tenderness on pressure in the hypochondriac region. Other evidences of hepatic derangement are found in the condition of the conjunctivæ and stomach; the first are yellow and injected, the second irritable with ejections of bilious matters, and patients complain of a bad taste in the mouth. Accompanying these symptoms, no matter what may be the condition of the parotid glands, there will be, in the majority of instances, tenderness and irritableness of the testicles. We do not recollect but one or two cases of mumps in old men in which more or less complaint was not made of tenderness and uneasiness in these glands. But although this sympathy is observed to be acting almost universally, yet we have not known any serious results to occur. The tumefaction and soreness disappear in time by resolution, leaving no permanent lesion behind.

The symptomology of parotitis in the aged is peculiarly characteristic as regards the nervous system. While in the young it is rare to meet with any active participation on the part of this system, in the old it almost always suffers to a greater or lesser extent. Cephalalgia, sometimes extremely severe, is almost an invariable concomitant. The encephalic disturbance is often so great as to threaten convulsions; and we have met with cases in which our fears of apoplexy were seriously excited, and this condition might have resulted in the absence of treatment for counteracting the tendency. When it is recollected that an hypertrophied condition of the heart is very common in advanced life, and that the blood-vessels, especially those of the brain, undergo changes disparaging their resistance to the current of blood circulating within them, effusions and extravasations might very rationally be apprehended. The constant exercise of the propelling power on the part of the heart seems necessarily to beget this hypertrophy and enlargement. Besides, the impediment offered to the return of the blood by the tumefaction and consequent pressure upon surrounding parts by the glands involved, may

augment in a great degree the cerebral disturbance. As to a metastasis to the brain, we do not known that we ever saw a case; indeed we doubt whether any such thing, properly so called, ever occurs. Congestion and inflammation of this organ have occurred, but this, as we think, has been the result of the circulatory derangement and special proclivity, and not specially from the nature of the disease.

The spine is generally involved, often to a considerable extent, in old persons. In most cases a careful examination of the spine will discover some point, pressure upon which, will elicit complaint. Our observation leads us to believe that this point is more frequently opposite the lower dorsal vertebræ near the junction with the sacrum, but occasionally the spinal column throughout its length will be found tender on pressure. Hence, probably, the pains and soreness of the limbs and muscles of which old subjects almost universally complain. The lower limbs are especially painful. Patients between the ages of 50 and 60 generally complain heavily, and apply various embrocations with the view of procuring relief. Often the first question asked of the physician will be — "Can you do something for the pains in my limbs and back?" Relief in these cases is to be sought by addressing remedies to the spine. The tremulousness and general restlessness which accompanies this disease, point to the same pathological condition. We recollect an old man of sixty, of robust constitution, with almost the agility and elasticity of youth, whose previous health was unimpaired—indeed, who had scarcely ever had any sickness, surprised when laboring under parotitis to find himself shaking like one with the shaking palsy. He remarked that he had always been remarkable for the "steadiness of his nerves." His spine was very tender, and so soon as this was relieved by free cupping and revulsions, the tremulousness disappeared.

As to the treatment, little may be said, since the indications are obvious, and are to be met on ordinary therapeutic principles. Regard should always be had to the kind of subjects with which we are dealing. Depletion cannot be carried to any great extent in old persons. But when the constitution is vigorous, the pulse full and quick, the temperature high, with much head-ache and pains in the limbs, blood may be drawn generally, until some impression is made upon the circulation. The result will be a mitigation of the symptoms. The skin which had been harsh and dry will become soft and moist, and the before irritable stomach will become quiet, and the head-ache be allayed. An abstraction of eight or twelve ounces will be sufficient in most cases of the disease in old persons, whenever it is required. We are always cautious in the employment of this remedy in old persons; but the proper amount taken

under proper circumstances, will always be beneficial, not only in mitigating present symptoms, but in favorably modifying the future course of the disease, checking and counteracting pre-dispositions to organic irritations and inflammations. Whenever there are symptoms of spinal irritation, as demonstrated by the evidences above enumerated, free cupping with large glasses over the spine should be practised. We sometimes place from three to six, previouly scarifying well. Attention should be directed to the bowels in the next place. If constipated with hepatic derangement exist, mercurial cathartics should be administered. Calomel, three grains, with rhubarb or jalap five grains, may be given and repeated every three hours until sufficient catharsis has boen effected. To the above, if not contra-indicated by irritability of the stomach or nausea, one grain of ipecac may be added.

Medicines that act upon the skin and secretions generally will now come in with propriety. We are in the habit of using the following: Sweet spts. nitre ½ ʒ; tinc. digitalis 30 drops, camph. tinc. opium 3 ʒ; a teaspoonful to be taken from three to five times per day. This will relax the skin, moderate the circulation, and promote the secretion of the kidneys. But many other diaphoretics and diuretics may be advantageously employed. If the head symptoms do not disappear under the bleeding and other remedies employed, a blister may be applied to the nape of the neck; and revulsives may likewise be applid to the spine in the event the cupping and other means employed do not succeed in dissipating the trouble. Warm teas in connection with the above may be occasionally administered. To quiet the restlessness that almost always accompanies disease in old persons, we should give opium in substance or its preparations. From ½ to 1 grain of opium given at bedtime, will generally procure rest; and this anodyne may be occasionally given in the day, as circumstances may require. Locally over the inflamed glands, warm flannel moistened with the camphorated soap lini ment may be applied.

Art. V.—*Surgical Memoranda for* 1857: By J. J. McElrath, M. D., Camden, Ouachita County, Arkansas.

I. *Fracture of Cranium, with wound of Brain, and Meninges. Subsequent softening and death. Post-mortem appearances.*—Henry, aged 27 years, a slave, the property of William Wyatt, Esq., of this city, on

Sunday evening, 13th January, 1857, was struck upon the head by a stout negro man, with a heavy axe, the edge of the axe passing through the skull, meninges, and about three lines into substance of brain, making a wound three and a half inches in length, extending diagonally across the vertex, through portions of frontal, and right parietal bones; and causing, at same time, a continuous fracture of nine and a half inches in extent. The fracture, including the wound, forming three-fourths of an irregular circle, commencing in the right supraorbital ridge, passing through the whole length of frontal, a portion of right parietal, and terminating in the edge, or border of squamous portion of temporal bone, one line posterior to cornal suture, thus bringing the two extremities of the fracture within less than three inches of each other. The inclination of the axe at the time the blow was inflicted, caused the bones to be widely separated, the fragment being forced outwards, and, apparently, raised above its normal position—a perceptible ridge in the pericranium, marking the line of fracture throughout the greater part of its course. At the place of direct injury, and for a short distance in either direction along the course of fracture, the bones were sufficiently apart to admit the end of the little finger; the fragment, however, was fixed, and immovable in this abnormal position.

Immediately after this severe injury, the boy walked across two squares, marking his course with blood, the hæmorrhage being profuse—sat upon a chair until the hæmorrhage was arrested, the wound cleansed and dressed; and then, with but little assistance, walked an equal distance to his home.

After the removal of all extraneous matter, the wound was closed by means of adhesive strips, lint, and bandage—the posterior angle being left slightly patulous, to afford exit to the discharges.

The patient was enjoined rest in a horizontal position; milk diet, and mild asperients.

Nothing unfavorable occurred during first week. Reäctive excitement reaching its acme on third day—pulse rising to one hundred and twenty beats per minute, gradually falling, to seventy on fifth day; wound discharging pretty freely, an unhealthy sanies, with small portions of cerebral matter.

During the night of eighth day severe convulsions supervened, and only subsided after copious venesection, active and brisk purgation, and the reöpening and cleansing the wound; leaving the patient, however, in a state of extreme prostration, with partial paralysis of right lower extremity. These unfavorable symptoms slowly passed away—healthy suppuration being fairly established, and the lost functions completely restored, on the fifteenth day.

From this time forward every feature of the case portended a favorable termination; the wound healing by suppuration, and granulation; cicatrization being, apparently, complete at the end of fifth week.

Patient was now permitted to resume his usual diet, and to walk about the yard. So much had his health and strength improved at the end of a week, that he was directed to return, on Monday morning, to his usual quarters, four miles from town. In the meantime, the freedom of the city was allowed him during Sunday. I, saw him at 4 o'clock, P. M.; he was walking the street, appeared well and cheerful. Examined his pulse, found nothing unnsual in the condition of the circulation—complained of no uneasiness or pain about the head—pupils not dilated. Questioned him about the particulars of the fight; his memory seemed perfect; said walking did not fatigue him more than usual; nor did he feel any inability in using his limbs—was delighted with the prospect of getting back to the plantation.

At 2 o'clock, A. M., the following morning, I was summoned, in great haste, to see my patient; found him, on my arrival, in deep coma—breathing stertorous—pupils widely dilated—pulse slow, feeble and irregular. Supposing that any collection of fluid within the cranial cavity, not beyond the reach of art to remove in safety, would most likely be found in the neighborhood of the original wound, an opening was made, with the crown of a large trephine, immediately on the border, or edge of the fracture, and at the point of greatest apparent injury. No collection of fluid, however, was found between the cranium and meninges, or beneath these membranes. The blade of a bistoury was now cautiously passed through the cicatrized tissue, in several places, down to the surface of the brain, with no better result. Deglutition being impracticable, further efforts were desisted from, the patient continuing to sink rapidly. Death closed the scene at 6 o'clock, A. M.

Five hours after death, in presence of Dr. D. S. Scott, the Brain was examined—the calvarium being removed by means of a horizontal section on a plane with the orbits. The fracture, throughout its whole extent, was closed by cicatrized tissue; no new osseous deposit was observed. This interposing tissue was strongly adherent to the pericranium, and slightly so to the meninges. Immediately beneath the line of fracture, these last named membranes, presented a thickened and indurated seam, from three to ten lines in breadth. In other respects, however, they were, apparently, healthy. In the right frontal sinus, was a semi-organized mass, of a pale purple color, the size of a filbert—pediculated—the pedicle having its attachment in the fracture.

After removing the brain entire, the membranes were carefully

examined throughout all their ramifications, without detecting any decidedly morbid appearances, unless, an unusual degree of dryness—which was likewise characteristic of the brain—may be regarded as such. Even the anterior, and posterior sub-arachnoidian spaces were, in a manner, destitute of fluid. The ventricles contained, altogether, about four or five drachms of sanguino-serous fluid, which was, doubtless, the product of post-mortem exudation.

The brain, after being divested of its immediate coverings, was examined by paring, or horizontal slicing from above downwards. The right cerebral hemisphere—the part in direct relation to the injury—was absolutely free from morbid lesions; that point, on its periphery, through which the axe had penetrated, differed only in color, so far as the eye or the touch could distinguish, from the surrounding cortical substance—a pale, uniform white seam replacing the gray. The left hemisphere, however, on arriving at a level with the *corpus callosum*, presented a large cavity or abscess, containing pulpy, semi-fluid cerebral matter. This cavity embraced a large portion of the *centrum ovale majus* of this hemisphere; extending from near the anterior cornu of the lateral ventricle, back to the middle of the posterior lobe. No extension of the softening could be traced, either to the periphery of the brain, or into the ventricles, the white, or medullary substance alone being the seat of disease. In no part of the walls of the abscess, could any trace be found of a natural effort, to arrest its progress; nor was there any appearance of inflammatory action, the cerebral vessels, the *puncta vasculosa* being no more prominent or numerous in its vicinity than elsewhere.

Imbedded in this pulpy mass, and near its anterior portion, was a hard, isolated, detached substance, the size and shape of a pigeon's egg, which, on being removed and opened, proved to be a cyst, perfectly organized, and filled with a pale yellow atheromatous fluid of its own secreting.

In no other part of the brain was there observed anything unusul. This organ being the seat of injury, and source of all the morbid symptoms during life, our examination was not carried beyond it.

The history of this case furnishes another fact, evidencing the destruction of a considerable portion of the brain without giving rise to corresponding symptoms, either organic, functional, or mental.

The cyst was evidently the nucleus or starting point from which the surrounding *ramollissement* proceeded and had its origin, at a period much anterior to that of the general destruction.

In a medico-legal point of view, it might well be a question, whether or not the wound was, in any way, the cause of death—the pathological lesion, upon which the event seemed mainly dependent, having its seat in

the opposite hemisphere, and far removed from the locality of direct injury. Yet, the previous history of the patient—his uniform good health, he never having had symptoms of cerebral disease; and besides, there being sufficient recorded evidence to warrant the belief, that a cyst, such as was found in the present instance, may pass through all its stages of development, from incipiency, to its highest degree of organization, within a period of four or five weeks, will justify the opinion that the wound and the death are related to each other, as cause and effect.

II. Fracture of Cranium without depression. Death from effusion and compression. Post-mortem appearances.—W. L., aged 33 years, whilst in a difficulty with E. H., of this city, received a blow upon the side of the head and face, with the flat part of a spade, which prostrated him; he quickly recovered, however, and continued to attend his business as usual until a late hour of the night—eight or ten hours after the receipt of the injury.

Between three and four o'clock the following morning, his unnatural respirations attracted the attention of some persons who were lodging in the same room, when, in attempting to arouse him, he was found to be speechless and insensible. My friend, Dr. A. W. Hobson, who was called to see the patient sometime afterwards, found him dying from coma

I was present, and assisted in the autopsy, before the jury of inquest, four hours after death. The face was of a pale leaden hue; some slight abrasions of cuticle on the cheek, external ear, temporal ridge, and over zygomatic arch; no appearance of blood in or about the mouth, nostrils or ear; nor was the skin cut or lacerated in any part. In the temporal fossa was a purple spot (bruised) the size of a dollar piece; the temporal muscle appeared bruised, and the temporal fascia much infiltrated with blood. A stellated fracture, of no great extent, and without depression, occupied the middle of the squamous portion of temporal bone; and having at its centre, a small triangular opening of less than two lines in its greatest diameter. At this point the bone was no thicker than the finger nail.

A horizontal section, through the external auditory meatus—met by a vertical one, through the course of the sagittal suture, served to reveal the internal injury. On removing this half of the calvarium, a large saucer-shaped coagulum was brought to view, pressing against the external surface of the dura mater. The form, position, and general appearance of the coagulum, impressed me with the belief that the hemorrhage was from external vessels. The small spicula of detached bone being found upon the external surface, immediately below the opening and partially imbedded in the substance of the pericranium, the coagu-

lum having its thickest central part directly behind the opening, and the meninges presenting, neither laceration, abrasion, nor wound of any kind.

From what source, soever, the blood may have deen derived, it is evident that this man was lost for the want of timely surgical attention. An incision through the soft parts down to the seat of fracture, so as to have permitted a free external exit to the effused blood, had it been timely practised, would, in human probability, have saved the life of this man from destruction.

That all injuries of the head should be carefully watched, is a great principle which we find inculcated in all the books. The history of this case affords a good exemplification of the truth of that principle.

III. Resection of Superior Maxilla, for Osteo-Sarcoma. Recovery. Subsequent return of disease and death.—This was an operation undertaken mainly with the view of relieving physical suffering. The nature and history of the disease, together with the age and condition of the patient, rendered the prospect of effecting a radical cure anything but flattering.

The patient, Mr. David Pipkins, aged sixty-five years, since August, 1856, was under the care of Dr. John H. Williams, who furnishes the following history of the case: "Sometime in the month of May previons, patient commenced complaining of 'jaw ache,' which he attributed to a decayed tooth, the second superior molar of right side, it being the only one then remaining. The removal of which, however, did not afford the anticipated relief. Early in August, the cheek being somewhat enlarged and quite painful, the Doctor, supposing there was an accumulation of pus, passed a lancet through the gum into the swelling; a few drops of grumous blood only, followed the withdrawal of the instrument. External medicated applications were without benefit, the tumor continuing to increase slowly, but constantly, attended, too, with an augmentation of suffering. In January a fungous excrescence made its appearance at the molar alveölus, through which a common gum lancet was passed deep into the cavity of the antrum, giving exit, as in the former instance, to a small quantity of grumous blood only. A few days, however, after this last "probing," the tumor commenced bleeding from the alveölar cavity, and continued to discharge blood profusely until the patient was fairly exhausted—exsanguined. From his account of the matter, which was corroborated by the Doctor, he must have lost six or eight pounds of blood in a period of forty eight hours.

"This severe depletion arrested, for a time, the growth of the tumor, and in some degree mitigated the pain. It soon, however, began again

to increase with accelerated rapidity, and with an aggravation of the suffering."

The tumor, on my first examination, February 30th, presented the appearance of a pretty general and uniform enlargement of the right side of the face; it was hard and somewhat elastic; skin of pale purple color, and not adherent to the tumor; cutaneous veins tortuous and enlarged; eye pushed upwards and partially closed; nostril nearly free; gums swollen, seeming to form a part of the general enlargement; no displacement of alveöli, or hard palate; deglutition slightly impeded; neighboring glands not implicated.

From this time until the 11th of March, the day upon which the operation was performed, the tumor had greatly increased, almost doubling its former size, extending far over the ramus of inferior maxillary, closing the eye and forcing the ball strongly against superior orbital plate*, pushing the gums downwards, nearly obliterating alveölar outline, filling up the zygomatic fossa, and overlapping the zygomatic arch, and os malæ. The nostril was still, however, partially free from obstruction, nor was there any marked displacement of hard palate. Submaxillary glands slightly enlarged; deglutition not greatly impeded.

Here was truly a "janus faced" disease, presenting benign as well as malignant features.

The condition of the patient, now, was such that neither temporizing nor delay were admissible. The removal of the tumor was, therefore, at once resolved upon, and accordingly performed, at 10 o'clock, A. M., on the 11th day of March, 1857, just ten months subsequent to the appearance of the disease.

The following were the steps observed in the operation. The patient being placed upon a table with the head slightly elevated and inclined to the sound side, a curvilinear incision was carried through the skin, from the labial commissure to the middle of zygoma, thence horizontally forward to near the external or lesser ocular canthus. The anterior flap being dissected off and reflected back as far as nasal spine and border of the nasal bone, an incision was made through the internal and external soft parts, commencing in the transverse palatine suture, and carried forward to the anterior edge of the bones, across the alveöli, passing one line on the right of nasal spine, to the semilunar notch of anterior nares, thence obliquely across the nasal process, from the inferior termination of os nasi to the middle of the lower border of orbit. A small narrow bladed saw was next applied to the alveölar border, and the bones

* The pressure of the tumor had completely destroyed the inferior orbital plate.

divided along the line of incision—the nasal process being divided with cutting forceps. The os mala was incised, and divided with the saw, through its frontal and zygomatic processes, close to its body. The inferior flap being carefully dissected from the lower and posterior border of the tumor. The separation of the os palati from the palatine process and from its attachment to the internal pterygoid plate of pterygoid process, was now effected by means of an angular bladed knife. At this stage of the operation, cutting instruments were, in a measure, dispensed with, being used only to divide such attachments as did not readily yield to the force of the fingers. No great difficulty was experienced in turning out the whole mass, and with it a portion of the parotid gland. The hæmorrhage, although profuse at every step of the operation, did not demand the ligation of a single vessel. The patient bore the operation, which occupied about fifteen minutes, with great patience and fortitude. In not a single instance did he raise his hand, or say stop, except to allow the blood to flow from his mouth. Syncope occurred once during the operation, and twice soon afterwards.

Everything having the appearance of disease being removed, and the wound well sponged with a strong solution of alum, all hæmorrhage soon ceased; and one hour after the removal of the tumor, the cavity being first filled with lint, the edges of the flaps were brought together and secured by fifteen stitches of twisted suture.

Union by adhesive inflammation was prompt and complete, the pins were removed on the third, fourth and fifth days.

The patient remained under the care of Dr. John H. Williams, and, apparently made a good recovery. The paralysis of that side of the face, which appeared complete soon after the operation, having, in a great measure, disappeared at the end of a month. His general health (May 1st,) was better than at any time since the appearance of the disease.

In the performance of this very formidable operation I was greatly indebted to my young friend Dr. Richard Macon Green, of Princeton, Dallas county, Arkansas, and to Dr. John H. Williams, of Columbia county, for much valuable counsel and assistance; and although the result was not, ultimately, successful, yet we have the consolation to believe that life was prolonged for several months, and that the final sufferings of our patient were rendered comparatively insignificant.

In deviating somewhat from the usual course observed in resections of this bone, I was mainly influenced by the position and supposed character of the tumor. That hæmorrhagic disposition which manifested itself so conspicuously at one period of the disease, was, as I supposed, a sufficient ground for the exercise of more than ordinary caution in the use of

the knife. I was, therefore, desirous of removing the morbid mass entire by carrying the incisions beyond it on all sides, so as to avoid the confusion and delay which might have resulted by incising the tumor itself. The tumor having sufficient space on its nasal and superior borders, and by removing a portion of the parotid gland, on its posterior border also, its insulation was not difficult.

The character of this tumor hardly admits of a rigid classification, embracing, as it did, such a variety of morbid products, amongst which the fibrous element seemed to predominate. The greater portion of the super maxillary of this side including the inferior orbital plate, was either absorbed or embraced within the tumor.

The disease reäppeared during the summer, and destroyed the patient in September last. I have no knowledge of the condition of the patient, nor of the progress of the disease after May, further than hearsay; the old gentleman's home being in Columbia county, twenty-five miles from this place.

I am aware that the propriety of this operation, in view of all the circumstances, admits of many doubts, and may form a subject of just criticism with the more ultra advocates of conservative surgery; yet there may, and do arise cases in which the surgeon is hardly permitted to avail himself of the old maxim of a "choice between two evils," but must interpose his hand—not to preserve or prolong life—only to change the mode of death.

In the present case there were many and powerful reasons to deter from an operation. The age and condition of the patient—the great danger of his sinking under the knife, and dying upon the table, or of failure in his system to reäct after the shock that must necessarily be produced; together with the very strong probability of a reäppearance of the disease at a subsequent period, even should the immediate result of the operation prove successful. These considerations were all fairly, plainly and forcibly impressed upon his mind. Under no ciscumstances, however, would he consent to forego the operation, except from an absolute refusal on our part to perform it. To use the old gentleman's own words, "death, that was neither suicide nor homicide, had no terrors for him."

On the other hand, and in justification of the operation, apart from the resolute and unflinching determination of the patient to submit to the removal of the tumor, there was no reason to suppose, or suspect a constitutional taint or a primary malignant character of the disease. The neighboring glands were but slightly effected, and that only at a late period in the progress of the morbid growth. The original

constitution of the patient was robust, and vigorous and he enjoyed, through life almost, uninterrupted good health. Even now he was only worn down by harassing pains and sleepless nights. In not a single vital function was there any evidence of direct failure from organic change.

In addition to these slightly redeeming traits in the history of the case, there was, at the same time, staring us full in the face, the plain, palpable and undeniable fact, that the disease was progressing surely and rapidly towards a fatal termination. In no event, it seems to me, could the patient, without the removal of the tumor, have survived beyond thirty or forty days.

IV.—*Incised penetrating wound of the Abdomen, with wound and protrusion of the Intestines. Recovery.*—Harriet, aged fourteen years, a slave, the property of Richard L. Hopkins, of this county, in passing down a flight of steps with a large, angular pointed butchers' knife in her hand, fell forward, the knife entering the abdomen one inch below, and about the same distance to the left of the umbilicus, making a vertical wound through the abdominal parietes, one inch and a half in length, through which several feet of the intestines protruded. I saw the patient some hours after the occurrence of the accident. There was but little visible hæmorrhage. The protruded intestines were lying on the lower part of the abdomen and between the thighs, were dry, red, and, as I presumed, somewhat swollen. There was considerable depression of the vital powers; pulse small and frequent; skin moist; extremities cold. The protruded bowel exhibited three wounds, all of which entered their cavity. Two of these, however, were mere punctures, and were closed by means of circular ligature, without regard to the protruded and everted mucous membrane. In the largest wound, which was transverse and about one inch in length, a very large *ascaries lumbricoides* was observed, attempting to make its escape into the "outer world;" after being removed, some half a dozen individuals of the same family made their appearance in rapid succession, and were in a like manner removed. This last wound being closed by five stitches of suture (after Lembert), an attempt was made to replace the bowel. The necessary manipulation, however, caused so much pain that it was deemed advisable to etherize the patient; but in her efforts to resist the inhalation of the anæsthetic, quite an addition was made to the already protruded intestine. Anæsthesia being at length accomplished, the protruded bowel was cautiously and carefully returned into the abdominal cavity, by a kneading process, occupying nearly thirty minutes in its accomplishment. The external wound was now closed by four stitches of interrupted

suture, through the skin only, adhesive strips, compress and bandage, and the patient prescribed rest in a horizontal position, farinaceous, fluid diet, in small quantities; sulph. morphid 1-4 gr. every sixth hour.

Recovery was prompt and complete. The bowels acted spontaneously on the fifth day. Eight days from the accident the patient was performing the usual duties about the house, as if nothing had occurred, and has continued well to the present time, ten months.

The protrusion of the wounded intestine was certainly the redeeming feature in this case. Death from incised wound and protrusion of the bowel is an exception to the rule. The reverse, however, is the case where the wound is concealed from view.

Is it really, Mr. Editor, of so much importance to place the serous surfaces of a wounded intestine in contact, in order to secure union, and, consequently an obliteration of the opening: or is not the inculcation of such a principle the result rather of theoretical speculation than of practical observation? In a number of experiments on the lower animals, I am certain that I could discover no difference in the ultimate result, whether the mucous or serous membranes were placed in contact.

(*To be continued.*)

ART. VI.—*Remarkable Case of Vesical Calculus formed on Wax:* By J. C. NOTT, M. D., Professor of Anatomy in the University of Louisiana.

THE subject of this case was a young man from the country, aged about twenty-four years. He presented himself to me in Mobile, about the 1st of August, 1857. Stated that he had accidentally placed a piece of wax in his bladder, about six or eight weeks previously; and that it was producing a great deal of irritation, pain, frequent inclination to urinate, bloody urine, etc. He was emaciated, haggard, dejected, and altogether in bad condition, mentally and physically.

On being questioned, he stated that he had, with his fingers, moulded a piece of common yellow wax into the form of a bougie, inserted it into the urethra, and broke off about four inches in the bladder; stated, also, that he had no impediment in passing water, and could give no satisfactory reason why he had committed this act of folly. From all the circumstances of the case—his mental and physical condition, etc.—I ha good reason to believe that he was a broken down onanist.

I was, at the time of his visit, about starting off on a summer excursion to the North, and told him I had no time to treat his case; and advised him to apply to some other surgeon, as a stone in the bladder and severe operation would be the consequences, unless he was relieved in a short time.

I examined him with a sound: found the bladder and urethra both very irritable, and so much disposition to spasmodic contraction about the bulb as to render the introduction of instruments difficult, and any attempt to remove the offending body from the urethra, without preparation, improper. I could think of no solvent for wax, which could be used in the bladder, without danger to its tissues; and, advising him to seek the aid of some one else, we parted.

I heard no more of him until he presented himself to me again, in New Orleans, on the 21st of December following, at which time, his symptoms had become much aggravated. He was greatly emaciated and dejected: complained very much of his bladder, and said he passed more or less blood every day.

I attempted to sound him, but could not get a metallic instrument beyond the membranous part of urethra; a small, delicate pointed, flexible instrument was passed, and I could distinctly feel the grating of a foreign substance in the urethra. I advised the lateral operation as the only rational course, and he was placed in the "*Maison de Santé*" for treatment. I operated on the 22d.

Not being able to introduce a metallic proved staff into the bladder, I substituted Simpson's stricture staff, which was passed down to the point of obstruction at the bulb, and cut down upon at its extremity. After some delay and trouble in getting into the small grove of this instrument, I removed it, and succeeded in passing a common groved bistoury staff into the bladder, and with a probe pointed bistoury, made the usual lateral cut into the neck of the bladder.

On introducing my finger into the bladder, I discovered some seven or eight calculi, two of which were sticking to the upper part of the fundus. The wax being lighter than urine, I suppose they were floated when small into that position. They varied in size from a small grape up to an olive, and on attempting to extract them with forceps they crushed and crumbled like a piece of wet crust of corn bread, and some time was lost in clearing out all the fragments. A good deal of bleeding followed, which, I supposed, came either from bulb, or transverse artery. He seemed to collapse from the shock of the operation, and his symptoms were doubtless much aggravated by the hæmorrhage. which was too great for one in his dilapidated condition.

The hæmorrhage was arrested by lint and styptics, and I am satisfied that he had not stamina enough of constitution left to survive the operation, if no hæmorrhage at all had occurred. He gradually sank, and died on the 26th, four days after the operation.

With the assistance of Dr. Morrison, resident physician of the house, I made a post mortem examination. After a careful dissection, we could find no improper cut from the bistoury; in fact, the opening in the bladder was rather too small, the prostate gland not being half divided, the knife did not approximate the pudic artery. The hæmorrhage came, probably, from the transverse artery, possibly from the bulb, the parts being disfigured by the styptics, and sloughy condition of the surface of the wound, it was impossible to say whether the bulb was, or was not touched by the point of the knife. Dr. Morrison, who lived in the house and watched the case very closely, thought that the blood did not flow from any particular vessel, but from the whole cut surface in consequence of a hæmorrhagic tendency of constitution generated by general bad health.

The bladder was thickened in some parts to the extent of *half an inch* — was altogether in a very diseased condition, and I cannot but regard this subject as the victim of onanism.

Now this case was altogether an embarrassing one, and I should be much at a loss how to treat a similar one should it present itself. I publish it with a view simply of bringing the minds of others to bear upon it.

I know of no solvent of wax which could be applied in the bladder; and I do not believe that any body with tact short of that of Civiale could extract a slender piece of wax, four inches long, from the cavity of the bladder with instruments, without the probability of doing serious injury to the coats of the viscus. Wax is lighter than urine, floats on the surface, and being much softened by the temperature of the body, it would necessarily be pinched off in small fragments by any process which could be applied. It would also be difficult to feel, or locate it while floating.

The fragments of calculi saved, filled a half ounce wide mouthed vial, and a large proportion was lost. What surprised me was, the fact that I could not detect a fragment of the wax; it seemed to have been acted on by the urine so as to loose its usual physical qualities, and to be infiltrated, or completely mingled with the calculous deposit. When dry, every fragment became as brittle as chalk.

Since writing the above, I submitted the fragments of the calculi to

the more skilful hands of Professor Riddell, who has obligingly examined them, and writes me the following note:

UNIVERSITY OF LOUISIANA, MED. DEP.,
New Orleans, January 20th, 2858.

PROF. J. C. NOTT—*Dear Sir:* I find the calculous fragments which you sent me, to yield an abundance of bees-wax, when soaked several hours in ether. The wax may then be obtained in a recognizable condition, by allowing the ether to evaporate, collecting the residue and fusing it. The wax seems to have suffered disintegration, and to have become thoroughly incorporated with phosphate of lime, and the triple phosphate of magnesia and ammonia, constituting a porous mass of fusible calculus.

Yours truly, J. L. RIDDELL.

ART. VII.—*Case of Fracture of the Cranium, with the loss of a portion of the Brain:* By JAMES COWLING, M. D., Houston, Texas.

MAY 8, 1857, about 2 o'clock, P. M., was called to see a son of Dr. B., a child about four years old, who a few minutes previously had received, as was supposed, a kick from a mule, and I have no doubt from the nature of the wound, that this supposition was correct. I found the patient lying on his back, his clothes, hands, face and head covered with blood and dust, and frequently uttering a low plaintive wail, or moan, accompanied with somewhat of a convulsive action of the limbs, indicating great suffering. On examination, I found an extensive wound of the scalp, with a fracture of the os frontis and left parietal bone, the membrane (dura mater) ruptured, and a portion of the brain the size of a large hazel-nut protruding. I could not discover any detached portion of bone. With Dr. McCraven's assistance, an attempt was made to return the protruding portion of brain, but without success. The edges of the wound were brought partially together: simple dressing and cold compress being applied, and the patient put to bed.

During this time he retained his sensibility to a great extent, although much inclined to sleep; pulse about 80—irregular. At 10 o'clock, P. M., the patient was found sleeping the most of the time; respiration natural; quite lucid when roused up; pulse somewhat accelerated; heat of surface increasing. The treatment consisted in digitalis, aconite, calomel, ipecac, etc., with ice water to the head.

9th—Patient passed a pretty good night; pulse 100; medicines had not acted. Give enemata, and continue medicines as before. 10 o'clock, P. M.—Bowels have acted freely; pulse 100, weak and wiry; surface of the body cold; feet and hands partially so; very restless; mind wandering, but when quite awake, rational. Apply warm applications to the feet and hands; discontinue cold applications and medicines.

10th — Met Drs. McCraven and L. Bryan in consultation. The patient had passed a restless night, with, at times, much depression; cold extremities, alternated with flushes of pungent heat; this morning reäction more considerable; pulse 100—irregular. Give sp. æther. nit., liq. ammon. acet. ā ā ʒi; aq. dist. ʒi, in a small dose occasionally; cold or warm applications as symptoms require.

10, P. M.—Much restlessness during the day, with high fever; mind occasionally wandering; at present the fever has subsided to a great extent; still restless, but rational when spoke to; by the aid of enemata the bowels have acted freely; the scalp wound does not look well, and is very offensive.

11th—Passed a pretty good night; at times, however, restless; fever much less; surface very warm, but the heat is equally diffused; thirsty; took rice water several times during the night; this morning heat of surface much less than formerly; regular pulse, but 118; mind quite clear; wants to get up and to eat. Continued mist. digital., and the mist. liq. ammon. acet., as symptoms demand.

12th—Patient rested well during the night; mind quite lucid; some fever; pulse 120, wiry; the bowels have acted twice freely; has taken several doses of the mist. æther. nit., and as also the mist. digital. The wound is looking much healthier; discharging freely; continue med.; cold applications to head; sago or rice water to be given an food.

13th—Slept well the latter part of the night; skin moist; some little fever; tongue clean; appetite good; but little thirst; pulse 110; quite cheerful. Give rice, arrow-root or sago, and occasionally a teaspoonful of mist. æther. nit., etc., also an enema, should the bowels not act during the day.

14th—Patient progressing favorably; slept well; tongue clean; pulse 100, soft; skin moist; bowels acted on by enema; calls for food; wound discharging freely. Give light diet; apply cold to the head.

15th—Same state, a little fever yesterday afternoon; passed a good night; pulse 100; doing well.

16th—Slept well; pulse 98; tongue clean; bowels opened by enema; wound discharging freely, the discharge being much healthier.

17th—Patient progressing favorably; most of the protruded brain

sloughed off yesterday; discharge abundant, slightly offensive. It is unnecessary to uote the daily treatment, it being such as would readily suggest itself to any surgeon — such as regular and light diet, paying attention to the secretions, and keeping the wound clean, with the edges approximated as closely as possible by adhesive plaster; much attention was necessary iu this respect, from the great teudency the wound had to gape, its separated edges in the centre, when left unsupported, being nearly five inches apart, and in consequence of the occipto-frontalis being divided, the eye ou the wounded side was nearly closed, when the wound was unadjusted and without support. No sutures were used in the case, for two reasons; first, I have several times seen very bad results from the use of sutures about the scalp; secondly, in this case they would have been of little avail, and would have certainly been torn or sloughed out in a few days, for it could not be expected that the occipito-frontalis would heal, during a free discharge from the wound, and an extensive separation of the pericranium.

20th—The remainder of the protruding brain has sloughed off, the wound looking healthy and discharging freely; at the bottom of the wouud fractured portions of the os frontis and parietal bone are seen, and from its appearance there can be no doubt but the force of the blow with the the apex of the hoof, came iu contact with the head over the suture of the os frontis and parietal, the suture being nearly the centre of the fracture. Its appearance presents somewhat (if the term may be allowed) that of a star, the fractured bones diverging in all directions from centre; the other portions of the os frontis exposed to view appeared very white, but healthy. From the centre of the wound, the upper or posterior portion of the pericranium appeared to be separated about two inches. This would make it appear that the blow was not direct, but rather glancing upwards, and was probably the means of saving the child's life.

June 1st—The patient continues to do well; the wound discharging freely; is up and moving about. About two months from the date of accident, exfoliations of the fractured bones began to be thrown off; this process continued from time to time up to the beginning of November, and there were some eight or ten pieces altogether discharged. A firm cicatrization has now taken place, and the boy is enjoying the best of health; no change in disposition or otherwise to lead any one to suppose that he had been subject to such a severe accident, unless it be the existing scar, which is seen forming an irregular half circle, and measuring, from point to point, near four inches; this will doubtless be much less in the course of time, so that there will be but a slight mark left.

ART. VIII.—*Stomatitis Mercurialis:*

MARKSVILLE, LA., 17th November, 1857.

DR. BENNET DOWLER:—I send you the statement of a case of violent *stomatitis mercurialis* and profuse ptyalism as ever occurred to me, during a long course of practice, suddenly induced by a comparatively small quantity of mercury, which was no less suddenly and completely arrested by the internal use of the iodide of potassium. Should you deem it worthy of a place in the NEW ORLEANS MEDICAL AND SURGICAL JOURNAL, of which you are the very able and accomplished editor, you are at liberty to insert it.

I am, very respectfully, sir,
Your obliged servant,
G. E. ELMER, M. D.

C. B., a young man of spare habit and lymphatic temperament, 17 years of age, came to me about six weeks since with the view of consulting me upon his case, which, upon inspection, presented the following appearances: General enlargement of the abdomen; pain and a sense of tension and fulness in the right hypochondrium, with extreme tenderness and pain when the least pressure was made on the epigastric region; upper and lower extremities much emaciated; urine high colored and depositing the lateritious sediment; the whole surface and tunica adnatæ completely jaundiced; slight cough; percussion over the left subclavicular region gave a dull, flat sound; the ear applied to other parts of the chest recognized the usual respiratory murmur; slight evening fever. My diagnosis, as you will readily imagine, was not difficult. The patient had been brought into this condition by working the latter part of the summer near Red River, or between the river and swamp; had an attack of the marsh remittent fever, of a mild grade, which was at that season of the year very prevalent in all that part of the country, which was in close proximity to the river or swamp. He had at the time, no medical aid, but contented himself by taking the usual patent nostrums, which, to the disgrace of our fine country, are to be found in great profusion in the proud and stately mansions of the rich, as well as in the humble hamlet of the poor and indigent.

Believing that his case presented no difficulty in its management, I merely prescribed, after the usual preparations, a mild or gentle course of blue pill night and morning; grs. iii blue mass; grs. iv pulv. ip. comp., for a dose, with pustulation over the seats of pain and uneasiness by tart. emetic ointment. He had hardly taken the third dose of medicine, before there were suddenly induced a most violent stomatitis, and profuse ptyalism as ever fell to my lot to witness during a long course of practice; the saliva flowed copiously and uninterruptedly; the mouth and

face were violently inflamed—the pharynx likewise. There were aphonia, and pain and great difficulty in swallowing; tongue, which protruded through the narrow cleft of the mouth, was very much swollen, and severely injured by the pressure of the teeth, which left deep indentations on its upper and lower surface.

Expecting no difficulty in curing this case, I immediately commenced a course of practice which its exigencies seemed to require; a large blister was applied to the neck from ear to ear; the cuticle was removed, and the discharge kept up by dressings of simple cerate. The bowels being bound, the patient not being able to swallow medicines of any size, and with the view of directing the current of secretory excitement from the mouth to the intestinal secrements, I administered a drop of ol. tig. every hour. After the third drop, the patient had several copious watery stools, which had no effect in lessening the stomatitis or ptyalism.

I next had recourse to the usual astringent, stimulating and corrective gargles, with opiates at night. These likewise failed. Baffled in my exertions to relieve my poor patient; seeing all my remedies in regular succession productive of no benefit whatever; my patient getting worse, sinking and fainting upon the least exertion of body, affected with tremors whenever he attempted to walk across his narrow room; in fact, the case exhibited all the phenomena of *erethismus mercurialis*, so accurately described by Pearson, as a *dernier resort*. I was resolved to give the iodide of potassium a fair trial, and prescribed the following formula: R. iodide potass. xii grs.; syrup sarsap. ʒ xii — mix. The patient was directed to take a fourth part of this mixture every four hours, and a gr. of sulph. morph. at bed time.

The patient, after taking the fourth dose, to my great satisfaction, commenced improving — every urgent, painful, and unpleasant symptom gradually and quickly disappearing; and from presenting a horrid and squalid appearance, is now metamorphosed into a new man, and doubtless will be speedily restored to perfect health.

Did the iodide of potassium act as an antidote, neutralizing the particles of mercury present in the system? Did it set up in the constitution an action antagonistic to that of mercury? Or, last, though not least, did not the iodide rather modify the scrofulous diathesis which was doubtlessly the cause of all this mischief, rendering our patient morbidly susceptible of, and obnoxious to, mercurial medicines?

ART. IX.—*Method of Preserving Bodies for Dissection:*

MESSRS. EDITORS:—In an article on Medical Schools, in the November number of the New Orleans Medical and Surgical Journal, I alluded to a method of preserving bodies for dissection, which I regarded as a discovery of great importance to students of anatomy, and particularly to those of our Southern States, where the climate is so unfavorable to anatomical pursuits. I have had a good many letters from medical gentlemen requesting me to furnish the formula, and I take much pleasure in laying it before your readers, hoping that this important discovery may awaken a new interest in this fundamental branch of our profession.

The following is the formula which I have used in New Orleans with perfect success, and there can be no question that if a subject is properly injected, it will keep as long as desirable. Take two parts, by measure, of muriatic acid, and one part of water, and as much metallic zinc as they will dissolve—use it undiluted. Cut down on the arch of the aorta and throw in as much of the fluid (according to the size of the subject) as can be injected without excessive force—say from two to four quarts. If it is well done, the muscles all become of a slate color, and the tissues firm. I have now on the table a subject on which I have been demonstrating to my class for 15 days, and it is entirely free from odor, or other sign of putrefaction.

J. C. NOTT, M. D.,
Professor of Anatomy, University of Louisiana.

P. S.—The injection should be thrown in gradually, and it is well to wait some minutes between each syringe full of the fluid. A body may be well injected, if done with skill, by the carotid, or femoral artery, but when the apparatus is imperfect it is better to saw the sternum longitudinally, force the chest open, and place the pipe in the arch of the aorta. Bodies are "embalmed" in most of the cities of the United States by a similar process, and I this morning saw more than a gallon of fluid thrown in through the radial artery, at the wrist. The subject was a lady, whose body is to be transported to the North.

[I beg leave to append to the above communication from the distinguished professor of anatomy in the University of Louisiana, the following note concerning the preservation of the human body, during a course of dissections in any climate. If not the newest, it is certainly the cheapest method known. Many years ago, in Virginia, in order to afford my private pupils anatomical facilities with the utmost privacy and security, I was compelled to use the garret of my dwelling, where the heat was great, and the putrefactive process was, consequently, rapid during

midsummer. I was able to carry on dissections without hurry and without the putrefaction of the subject in the month of August, as well as in the winter. The means adopted were very simple — namely, when the subject was not under examination, it was kept constantly and completely immersed in a saturated solution of common salt. This method neither alters the natural color of the tissues, nor subsequently interferes with the making of handsome dried preparations when the dissection shall have been completed. Before proceeding to dry an anatomical piece, a little soaking in fresh water may be proper, to prevent any saline incrustations, but this is not necessary generally. No accident to health and but little damage to knives can result from this method of anatomizing. In fact, no climate producing salt and water, can be so hot as to render this method inapplicable. The human body can be preserved just as well as pickled pork, beef, fish, and the like. Whether in high or low latitudes, all dissecting rooms which are kept sufficiently warm to be healthful and comfortable to the living, must be unfavorable to the prolonged preservation of the dead, and produce emanations of a deleterious character. BENNET DOWLER.]

ART. X.—*Frambæsia or Yaws:** By GREENSVILLE DOWELL, M. D., Columbia, Texas.

Case—Treatment—Cure.—On the 16th of July last my attention was called to a case of this disease by my friend Judge John H. Jones, of this county. He said he had asked the opinion of several physicians, of a disease affecting the anus in a negro child about two years old. When I saw the boy it had the following appearance: There was an ulcerated fungoid growth around the right side of the anus, which threatened to close it. It had grown so large and had so much encroached upon the anus as to impede his defection. The Judge was then applying nitrate of silver to it, and it had rather a pale color, but it had not lessened the tumor, and had done no good. The boy had been taking, also, a syrup of sarsaparilla, with no good effect. Upon further inquiry the Judge informed me that his mother was a native African, and had had the

* DR. DOWLER,
Dear Sir,—I send you the above case as it is of unusual occurrence, and more particularly to show the good effects of creosote in this disease. I have long been treating Lupus Porrigo, and Nurses ulcerations with it, and in all cases have found it efficient.
G. DOWELL.

same disease ; that about Christmas last he had some fever and an eruption over the most of the body, but it had all disappeared except at the anus, and it continued to grow, and nothing that the various physicians prescribed had done it any good. I immediately suspected the nature of the disease, and applied a solution of creosote to the ulceration; and upon consulting my books on the disease I was satisfied that it could be nothing but a case of yaws. Upon my return the next day the tumor looked black and much reduced, so the creosote was continued of the strength of 4 parts of water to one of creosote.

This mixture in a short time entirely destroyed the ulceration, so much so that on the 24th the Judge told me the child was really well. The syrup of sarsaparilla was continued for some time, and up to this date there has been no return of the disease.

I will here state that the creosote was prescribed from my former success in treating tetter and fungoid ulcers with it; and from but a faint knowledge of the means used to cure this disease. I continued it from seeing the good effects of the first application.

It is a disease that seldom occurs in the United States, and appears to be indigenous to Guinea and the imported Africans in the West Indies. We have a number of native Africans in this country, brought here under Mexican rule, by the celebrated Monroe Edwards, and others of the Lafitte party. This child was a descendant of these, and was very black. His mother had the disease when grown, but how she was cured, or by whom, I could not learn. The eruption is of a tuberculous character, and when left to its natural course assumes a raspberry appearance which gives it the African name, *yaws*. It comes on with fever, as other eruptive fevers, and only once in a lifetime. The treatment pursued by the West Indian physicians is antiphlogistic during the eruptive stage, and after this subsides by revulsives, as in the treatment of lupus. Iodine, arsenic and mercury are relied on. The native of Guinea uses various indigenous remedies, unknown (most of them) to our Dispensatory.

W. Kerr has given us a complete history of the disease in the Cyclopædia of Practical Medicine, and remarks that the disease is similar to syphilis in its infection, but unlike it in its only occurring once in the same individual. It is common to separate the cases from healthy persons, as in cases of small pox, to keep it from spreading. It, like syphilis, is apt to break out again after all appearance has ceased ; but it is not considered as a dangerous disease. Washing with cold water and various vegetable substances are practised, with but little apparent good.

ART. XI.—*Bilobular Hydrocele, each cyst of which contained a liquid of different color. Operation and injection with the Tincture of Iodine; Cure. Observations:* By Dr. MERCIER, Surgeon of the Circus Street Hospital.

ON the 15th September, 1857, Mr. Jean Laboura, a native of New Orleans, about 63 years of age, presented himself at the consultation of the Circus Street Hospital, for a rather voluminous tumor of the left side of the scrotum.

Of robust constitution, and bilio-sanguineous temperament, Mr. Laboura has always enjoyed excellent health; he does not remember having ever been sick. One year ago, after making a false step upon something slippery, he felt in the renal region, an acute pain, which soon became more severe upon the left side, and extended itself down as far as the corresponding side of the scrotum. From this time it began to increase in volume. The testicle of this side became, and continued, sensitive for two months, at the end of which time, the pain disappeared to return no more. However, the tumor of the scrotum has continued to increase gradually and regularly, up to the present time. The pain in the left renal region has always persisted, and the patient, for relief from it, has supported the scrotum for the last eight months with a sac, which he himself fabricated.

Actual Condition.—The left side of the scrotum is occupied by a tumor more than six inches long, indolent, without change of the color of the skin, pyriform, having the large extremity turned down, and presenting at the union of its inferior third with its superior two-thirds, a transverse groove, which slightly depressed it. The testicle and epididymis situated above and behind, offered neither hardness nor abnormal uneasiness. The tumor, weighed in the hand, appeared of the same weight as a like quantity of water, and pushed above, it supported itself upon the external inguinal ring. Having sized the testicle by its root with the left hand under it, pressure was briskly made with the right finger, which sank some little, and suddenly stopped against a resisting plane.

This, then, was without doublt a chronic bilobular hydrocele, without communication with the peritoneum, and without complication with disease of the seminal gland, or epididymis.

The patient was in excellent condition, and longed to be rid of an infirmity which was becoming most inconvenient. The operation being proposed and accepted, was at once performed. The transparency of the large extremity of the tumor being well established, the trocar was plunged into its anterior and inferior part. Two ounces and a half only of a pale yellow liquid, similar to the ordinary liquid of hydrocele,

escaped by the canula. Iodine was immediately injected. A second puncture made in front and a little higher, gave issue to nearly six ounces of a troubled, lactescent liquid, containing innumerable whitish fribrils, very apparent upon the surface of the liquid, notwithstanding its want of transparency. A second injection of iodine was made into this second cyst.

The patient returned home the same evening to Freeport upon the other side of the river, one mile above the ferry. Eight weeks after he returned to the Circus Street Hospital. All traces both of the disease and the operation had disappeared. It would have been impossible to say upon which side of the scrotum the tumor had existed.

Whether the abdominal and diaphragmatic muscles in contracting, after the false step made by Mr. Laboura, reäcted upon the abdominal viscera, and from these upon the blood vessels of the testicle, or whether the inguinal canal and its two openings were compressed by the spasmodic contraction of the muscles of the abdomen, it is certain that the disease was the result of compression upon the spermatic cord.

But the peculiar character of this observation is beyond doubt the presence of *two liquids of different color* in the same hydrocele, and in two cysts distinct and separate. The choclate colored liquid is no longer considered as appertaining to the hydrocele properly so called; it indicates an old hematocele, the contents of which have undergone a certain degree of decomposition. I have seen the liquid of hydrocele lactescent, at other times containing albuminous flakes in suspension. M. Velpeau says that there has often been observed in the liquid of the tunica vaginalis, sometimes free, sometimes adherent, concretions of a friable matter, greasy, micaceous, or in clots, of fibrinous and even cartilaginous appearance. The liquid of the tunica vaginalis was entirely green, of a deep vegetable green in the patient of M. Salacroux. (*Arch. Gén.*, 1 *Série*, t. xxiv, p. 137.) But I am not aware that a case of *bilobular hydrocele, each cyst of which contained a different liquid*, has ever been met with before, or such observation published.

Apart from some slight modifications in the mode of operation, the method practiced in the preceding observation is the one which I have invariably employed for more than sixteen years. With a sharp blow, I bury the trocar in the anterior part of the tunica vaginalis, completely evacuate the liquid from it, inject two drachms of pure tincture of iodine, a part of which is left in the cavity of the cyst, without being disquieted by some bubbles of air which may have been admitted with the injected liquid. I have already operated upon more than one hundred and sixty cases of hydrocele, and have yet to deplore an unsuccessful operation.

In the case of simple chronic hydrocele of the tunica vaginalis, presenting a certain size, I have constantly proscribed incision, excision, the seton, moxa, scarifications of the interior of the sac, cauterization, with caustics or the actual cautery, the employment of tents, mèches, of the canula, or the end of a gum elastic sound, as has been used by M. Larrey. I have also constantly refused to employ as injections, the more or less acrid solutions of the ancients; of lime water charged with corrosive sublimate, as Lembert made use of; of alcohol, pure or diluted; of the solution of caustic potash, or of sulphate of zinc; in fine, of Port or red wine, whether simple or mixed, with a part of alcohol, etc., as was the universal custom until M. Velpeau extolled the tincture of iodine, and generalized its employment in the treatment of hydrocele.

When from one cause or another the enveloping membranes of the cyst have acquired a degree of thickness which renders doubtful the penetration of the trocar by a sharp blow into the interior of the tunica vaginalis, I previously make with an ordinary lancet, a puncture by which the trocar is afterwards very easily introduced as far as the centre of the tumor.

In the case of a slave whom my learned friend, Dr. Lambert, sent to the Circus Street Hospital for hydrocele of the cord, as large as a small hen's egg, I evacuated the cyst by means of a simple cut with the lancet. I introduced by the puncture, a large needle, armed with a thread folded in four, passed it out upon the opposite side, and established in this manner a seton. Morning and evening these slips of thread were moistened with tincture of iodine pure, and then drawn from left to right and right to left. The sac was not slow in inflaming; the seton was then withdrawn, and nine days sufficed to complete the cure. This is the mode of operating to which I always have recourse, when the small volume of the hydrocele, whether of the cord or of the tunica vaginalis, with children for example, renders it very difficult, if not impossible to retain the end of the canula in the interior of the cyst, when the liquid has been evacuated from it.

I have not yet observed in my private practice those cases, where the walls of the tunica vaginalis have undergone a lardaceous degeneration, or have been transformed into a thick, hard fibro-cartilaginous shell. In such cases, I should first try the injections of pure iodine, repeating them one after another, as I do for chronic maladies of the articulation of the knee, and I should have recourse to the incision, only having well established the inefficacy of such attempt. Then I would make incisions right and left as long as the size of the tumor would permit, and treat it as for a case of hematocele.

I have above stated that I am little disquieted by the presence of some bubbles of air in the tunica vaginalis after the operation has been performed. I have often intentionally left air in the cyst; inflammation has succeeded, and both the air and the small quantity of iodine left in the sack have been absorbed, and the cure has been neither less rapid nor less sure.

Several times, whether intentionally or without wishing it, I have buried the point of the trocar in the body even of the testicle. Never has the least accident succeeded; and all the patients whose seminal glands have been injured, have been perfectly cured. I have even observed that, with those whose testicles were knobby, hard, hypertrophied, the enlargement has disappeared more rapidly, and the cure has not been so long delayed. Is not this a result which ought to be expected, when, everyday, to hasten the cure of an old orchitis, many and deep punctures are made into the body even of the testicle, and all kinds of preparations of iodine are applied over this organ, upon the surface of the scrotum?

It would be easy to enumerate the many reasons which render the injection of pure tincture of iodine preferable to all the other means employed for the cure of hydrocele. We wish to call attention only to the impunity with which this therapeutic agent may be introduced between the meshes of the cellular tissue of the scrotum. When M. Velpeau, in 1836, made his first experiments with the tincture of iodine, at the Hôspital de Charité, in Paris, we also desired to make the attempt, and our inexperienced hand injected about two drachms of tincture of iodine into the sub-cutaneous cellular tissue of the scrotum. The scrotum did not sphacelate, and our patient was perfectly cured. Our quasi mishap had the advantage of proving that the tincture of iodine could be injected into some of the tissues with perfect impunity.

ART. XII.—*Critical Researches in Medical Terminology founded on the last edition of Professor Dunglison's Medical Lexicon*:*—By BENNET DOWLER, M. D.

FOR more than the third of the current century, a most successful and voluminous author, generally esteemed by the medical profession for his

* *Medical Lexicon; a Dictionary of Medical Science, containing a Concise Explanation of the Various Subjects and Terms of Anatomy, Physiology, Pathology, Hygiene, Therapeutics, Phar-*

useful and honorable labors, admired by thousands of pupils, whom, as a teacher, he has instructed, Professor Dunglison's reputation is colossal. Almost every year he launches forth into the mighty stream of medical literature, a new octavo, or a new edition, which the critics hail with one acclaim as *Io triumphe!*

Like Duncan "he hath borne his faculties so meek" that a critic who dissents from him encounters perils in proportion to his critical dissents. While "praises of the unworthy are robberies of the deserving," the praises of the worthy bestowed without judgment and discrimination are degrading to the critic's vocation. "I had rather be right," said Clay, "than be President."

But critics do not expect to be criticised, nor reviewers reviewed, being always right, according to Colcridge, who says:

> "No private grudge they need, no personal spite,
> The *viva sectio* is its own delight!
> All enmity, all envy, they disclaim,
> Disinterested thieves of our good name—
> Cool, sober murderers of their neighbor's fame!"

"Every censure, every sarcasm respecting a publication which the critic, with the work before him, can make good, is the critics' right. The writer is authorized to reply, but not to complain. Neither can any one prescribe to the critic, how soft or how hard, how friendly, or how bitter shall be the phrases which he is to select for the expression of such reprehension or ridicule."

Instead of formally reviewing the Medical Lexicon of Dr. Dunglison, in all its details, which the space of this Journal will not permit, it is intended to ramble over the work, noting impartially its general characteristics, at the same time making critical or explanatory remarks, which, independently of any bearing they may have on this particular book, may have more or less value for the earnest and unbiased young physicians who love science more than great names—physicians who receive the labors of their predecessors and teachers with gratitude and humility, yet are resolved themselves to contribute to the further advancement of science.

It is without the remotest wish to censure the learned, useful and estimable author of the Medical Lexicon, that this paper is written. Should its postulates be as free from mistakes as they are from illiberal motives,

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macology, Pharmacy, Obstetrics, Medical Jurisprudence, Dentistry, etc.; Notices of Climate and of Celebrated Mineral Waters; Formulæ for Officinal, Empirical, and Dietetic Preparations, etc.; with the French and other Synonymes: By ROBLEY DUNGLISON, M. D., L.L.D., Professor of Institutes of Medicine in Jefferson Medical College of Philadelphia. Revised and very greatly enlarged. Pp. 992 8vo.; fifteenth edition. Philadelphia: Blanchard & Lea; 1857.

or a wish to detract from the author's reputation, then the following remarks may not be totally devoid of interest.

Lexicon, (a Greek word,) designates a species of dictionary, a glossarial nomenclature, a vocabulary or word-book, bearing the same relation to the more general term dictionary that the latter sustains to the encyclopædia. A Medical Lexicon is a technical word-book, alphabetically arranged and explained, from which extended histories and descriptions not essential to the exegesis of the special terminology, and still more colloquial, obsolete, and non-professional words should be excluded. "*Medical Lexicon,*" placed at the head of the title page of the work here alluded to, indicates in the most rigid terms its legitimate purpose, defines its limits, but gives no warning of an impending and imperfect polyglot.

It is evident (for genius is not a secret to itself) Dr. Dunglison thinks, as do his reviewers, that his Medical Lexicon is the crowning act of his literary life, since in his preface, consisting of but one page, he quotes from "one of the most learned of European medical periodicals, the British and Foreign Medico-Chirurgical Review," the following exquisite but not quite grammatical criticism : "the work has been something prodigious; the work, however, has been now done, and we are happy in the thought that no human being will have again to undertake the same gigantic task. Revised and corrected from time to time, Dr. Dunglison's 'Medical Lexicon' will last for centuries." With a wish to coïncide with this favorable verdict, the following investigation will be conducted. In the meantime—

> "What is't to you that half the town admire
> False sense, false strength, false softness, or false fire?
> What is't to you, that numbers place your name
> First, fifth, or twentieth in the lists of fame?
> Old Time will settle all your claims at once,
> Record the genius and forget the dunce."
>
> [WHITEHEAD.

Few authors will be less offended with animadversion upon their books than upon their morals. Addison relates the case of an atheistical author, who consented on his death bed to receive the visit of a clergyman. The penitent declared that his chief regret was, that of having written an atheistical book which would prove injurious to the cause of religion after his death. The priest consoled him by saying that he need not give himself any concern on this ground, as the book was so superficial that it could not possibly mislead the weakest mind. Whereupon the sick man became very indignant, and dismissing the clergyman at

once, bitterly reproached his friends for having sent him a fool, totally incompetent to give a dying man advice.

The writing of a book *ipso facto* is evidence that the author expects or desires approval or praise. Even dictionaries, wherein originality is generally excluded, form no exception to this rule. In the building of a dictionary the merit is chiefly negative. It is praise enough to be able to say nothing is created, nothing is omitted. Although no isolated individual, whatever may be his erudition, can hope to reach even this negative merit, since no one possesses universal knowledge; nevertheless it is a merit to approximate this unattainable perfection.

The labors and learning supposed to have been necessary to the authorship of the medical dictionaries now extant have been greatly exaggerated. The scissors, paste, pens, ink, paper, and the copying, have been paramount, seeing the vast omissions chargeable to these works—omissions in the terminology of anatomy, physiology, psychology, ethnology, chemistry, electricity, crystallography, morphology, palæontology, zoölogy, and other medical or associate sciences.

The largest liberty is allowed, or at least taken, by the builders of dictionaries; that is to say, they may, and do take without acknowledgment the etymologies, definitions and descriptions of each other, and of all their predecessors, without inverted commas and without remorse, although some authors of dictionaries (Dr. Dunglison not being of this class,) do indicate in their prefaces or introductions, the authorities from whom their materials are taken.

Many years ago, in his letter published in this Journal (v. 125) Dr. Dunglison wrote, in relation to his treatise on Practical Medicine, and generally in regard to natural history, as follows: "Where the *facts or histories of disease* were concerned, I regarded them as common property, unless where the descriptions were entirely original." If this be legitimate in writing treatises, it is *a fortiori* in dictionaries. "Definitions," says Dr. Harris, in his medical Dictionary, "have been considered common property, and transferred from one work to another without acknowledgment—a common privilege claimed by all lexicographers." Small is the glory of such authorship, unless redeemed by a full exposition of terminology, brought up to the existing state of knowledge. This easy habit of copying from predecessors, instead of copying from the actual progress of the sciences, is little adapted to advance lexicography or benefit the student.

He who begins, continues and ends his studies by consulting and referring to reliable lexicons and dictionaries of science, is likely to arrive at precision, strength and clearness of language, without which clearness

and precision of ideas will usually be wantiug, or if not wanting their communicatiou to others, by discourse or by writing, will be vague and unsatisfactory. Half of the coutroversies aud contradictions which rage in religion, politics and science, are due to misunderstandings as to the meaning of words. The best remedy for these evils is found in perspicuous definitious, and where these caunot be fixed, examples and things illustrative of the applications of these defiuitions will tend to produce couviction aud unanimity of opinion.

If each word clearly represented a precise idea, or a thing, the siguification would be, of course, the same to all. The exuberance of synonymes which often clog the wheels of scieuce, and the different meanings attached to the same words are embarrassiug, as are the neologies incidental to the progress of scieuce and new currents of thought. Yet they must find a place in dictionaries, aud being appealed to, as already stated, will tend to produce uuity in the true church of Æsculapius.

No where beyond French territory has the medical world shown a preference for French terminology over the classical languages, the original sources of medical science and nomenclature, as seen in Hippocrates, Galen, Celsus and many other works of antiquity. Even in polyglot New Orleans, the only polyglot city of the Republic, the American vernacular is paramount. The advocates, however, of a non-technical language in science have not duly estimated the evils of rejecting a classical, fixed nomenclature as an universal one, as the Latiu. The French physicians who write in their uative lauguage may send their prescriptions to an apothecary who knows only the English aud technical Latin, and hours may be consumed iu search of a French apothecary, before gettiug a *potion calmante*, *eau hémostatique*, or *potion contre choléra*. In pharmacology an uniform name, so far from augmenting, would dimiuish the number of fatal mistakes. In anatomy, pathology, botany, chemistry, etc., great benefits would be derived from uniformity of nomeuclature.

In a medical lexicon, every unnecessary untechnical word should be excluded. Dr. Dunglison's dictionary, with its biases towards popularization, contains much that is useless in this behalf. Dr. Dunglison's dictionary exceeds all others in barbarisms, chiefly Gallic; that is, the words are to a great extent neither the English nor the accepted classical terms of technological works, and consequently, an useless mass of verbiage encumbers the entire work. To make the matter worse, he gives, without any imaginable reason, definitions and expositions sometime solely under a French term, or under both French and English, or French, English and Latin, so that his nomenclature and exegeses are double, triple, quadruple, etc., as will be seen hereafter.

The principle of inserting isolated French, English, or other words, utterly devoid of philological connection, or any explanation whatever, may be admissible in primmers and spelling books, but in scientific dictionaries never. In a polyglot dictionary each language is self-explaining; but this dictionary, without aiming at that utopian finality in medicine, virtually assumes that character in its vocabulary.

The adoption of a French or other foreign, familiar or non-scientific nomenclature in an English dictionary, is an useless innovation. Upon what philological or technical principle are many of the words mustered into the front ranks of this dictionary, put in French? They are not cognates, nor connected in etymology. They are not synonymes, nor even exigetical. As translations, they are bald. Their various significations are not given. They are without gender, number, or scientific connection. Whether they are adjectives or substantives does not appear. Why not insert Russian, Danish, German, Italian, Spanish, as well as French?

In the Republic of Letters and Science, no modern language is likely to be honored as worthy of adoption as the language for an universal nomenclature. The English, which among the moderns is really the master-tongue, does not dare to claim this high distinction, but prefers the classical, chiefly the Latin. If this technological course should be adopted by all civilized nations, a scientific nomenclature and classification would be soon inaugurated of the utmost advantage to all the parties, and highly conducive to the progress of an exact knowledge in anatomy, chemistry, pharmacy, botany, natural history, mineralogy, etc.

French lexicographers in their dictionaries, shun every English or other foreign word as they do the plague. Their great national dictionary rejects etymological analysis altogether, even the Latin with which the French is correlated by paternity and filiation. This is a good general rule for a popular dictionary, in so far as it does not impede the progress of scientific nomenclature, perspicuity and precision, nor substitute vague, unsettled terms for exact technical ones.

An arbitrary jumble of equivalent untechnical words, the sweepings from various languages, even though they may swell a book to 60,000 so-called terms, cannot be reckoned legitimate in a medical dictionary, nor even in a *family* or domestic one.

If any modern language be chosen for an universal terminology, let it be the English. It is already becoming the master language of the civilized world. Its march is onward; its extension is progressive; its accumulative force, irresistible. In America alone, it will, in a few decennial periods, be spoken as the mother-tongue by a more numerous population

than France and its colonies will contain, as is evident by the authenticated censuses of the two countries during the last and the present centuries.

This dictionary, from A to Z, is fundamentally erroneous in its theory and practice of synonymy. Although this is far from being the smallest fault of the work, it is intended to dwell upon this subject with some minuteness in order that the student of medicine may not be misled in this behalf. It is believed that the following remarks, independently of their applicability to this dictionary, are worthy of consideration by those who desire to cultivate the literature of the medical profession.

The only formal explanation of the plan of this dictionary is the following, which will be copied entire, namely :

"EXPLANATION.—If the synonymy of a term be needed, a mere reference to the term may be sufficient; but if further information be desired, it may be obtained under the term referred to. For example, the French word *Tronc* is said to be synonymous with *Trunk*. This may be sufficient for the inquirer; should it not, the requisite information may be found by turning to *Trunk*."

The title page sets forth the same theory, promising "a concise explanation," etc., "with French and other synonymes." This explanation of the science of synonymy is fundamentally erroneous in its theory as in its application, as the literature of all ages and nations shows.

This dictionary, which thus promises in its title page to treat of technical synonomy, has not in a single instance, conformably to the universally accepted meaning of this term among synonymists, given an exegesis, explanation, comparison, or illustration of either English or French, Latin or Greek synonymes, according to their cognate affinities, or specific differentiations.

"The Universal and Critical Dictionary of the English Language," by Dr. J. E. Worcester, (1853,) a work as yet unsurpassed, defines synonymes thus: "Words of the *same language*, which have the same or a similar signification, as *happiness* and *felicity*, *heavenly* and *celestial*."

"*Synonymes.* Words of the *same language* which have a similar signification. Strictly speaking, words having exactly the same signification are not to be found in any language, unless one of them has been borrowed from another language." (Brande. Dict.) This exegesis is in exact accordance with that which the authors of special dictionaries of synonymes give, among the most prominent of these are, Crabb's English Synonymes; Boinvilliers' French *Dict. des Synonymes*, etc., in which the Dunglisonian principle of simple translation is ignored altogether.

M. Boinvilliers, in his elaborate (Universal Dictionary of French

Synonymes,) after giving the etymology (Greek) defines the word synonyme, as a word which has a similar signification with another. Locke says "that a definition is nothing else but the showing the meaning of one word by several others *not synonymous* terms." Synonyme is, according to the French Academy, a word having the same or very nearly the same signification with another. Wailly's French Dictionary, adopted in the University of Paris, says that this term designates words having very nearly the same signification; *(se dit des mots qui ont une signification à peu près semblable; crainte et peur.)*

Crabb, in his learned work on English synonymes, rejects from legitimate synonymy such words as *abandon* and *quit*, "which are sufficiently distinguished from each other by striking features in their signification."

Now, Dr. Dunglison's typical or representative example illustrative of his synonymy is in no sense a synonyme, but a simple translation, *tronc* being the French for *trunk* — nothing more, nothing less. Synonymy, then, restricts itself to words, the meaning of which are not readily or usually distinguished from each other by striking differentiations, but which, on analysis and a nice comparison, will be found neither identical nor wholly different in signification. For example, invalid and patient are so allied as to be considered synonymous in popular language, yet the first has a very general signification; the other is special; the first is rather indicitive of the absence of good health; the latter of positive suffering from disease. Sickness and invaliding in the army are clearly distinguished in official reports. An invalid soldier is no longer able to perform his duty; a sick one is only temporarily disabled. Here the verbal distinctions are so marked as to render their synonymy questionable.

Synonymy in a popular and still more in a technical dictionary, has but a very limited application, special dictionaries having been assigned to this department of lexicography and philolgical criticism. The precision of technical definitions, unlike the ambiguities of popular language, is generally sufficient for the most fastidious student. Indeed, the actual amount of synonomy in any language, even in the English, one of the most composite, is small. Richardson, the most voluminous, and one of the ablest among English lexicographers, adopts in his colossal dictionary Horne Tooke's theory, which assumes that a word has but one signification, whence all its transferred meanings are derived, and that in the etymology of each word must be found this single intrinsic signification. Nevertheless, judicious etymologists, among whom Dr. Dunglison may be ranked, will often find this rule fallacious or fanciful, the original meaning of words having undergone essential changes.

The omissions in this lexicon must be great, judging by the disappointments that have happened in a hurried looking for such words as were accidentally thought of during a slight examination. Thus the great divisions of the animal kingdom, the vertebrata, mollusca, radiata and articulata were not found, nor the following terms, of great significance to the student: amphibium, palæǫntlogy, palæözoïe, coroner, inquest, ethnology, fauna, flora, fossil, mineralogy, thermotics, thermology, ozonoscope, ozonometer, archytype, atomology, eletrolysis, equivalents, electro-dynamics, binary, crystallography, teleology, probabilities, (doctrine of,) mica, taxidermy, sensationalism, sensuous, sensationalist, glycosuria, chitine, quadrumana, ruminantia, hibernation, selenium, modus agendi, centenarian, octogenarian, physical diagnosis, ens epidemicum, redhibitory maladies, transcendentalism, transcendentalist, hydro-dynamics, rhabdomancy, multiple, dimorphism, isothermal lines, radiation (laws of), conduction (calorific), refrigeration (law of), heat specific, heat latent, heat capacity for, dew, fog, aurora, corona, cold (poles of), glaciers, ossuaries, atomic theory, definite proportions, eocene, miocene, pleiocene, geology, anhydrous (crystals, alcohol, etc.,) haloids, equivalents, protoxide, deutoxide, binoxide, peroxide, perianth, corolla, nascent, pneumatology, etc. Pages might be filled with similar omissions, showing that the Lexicon stands far below the present level of the science of medicine, including its collateral sciences, though it may contain, as it claims, 60,000 words, counting its interminable repetitions, which are not a whit better than nomenclatural tautologies.

Even in nosology, the number of words might be augmented to an incalculable extent by the system of translations, repetitions in all the known languages. In the Lexicon, for example, there are, febris, fièvre, fever; artère, arteria, artery, etc., occupying different, yet primary places in the vocabulary; then, fevers, arteries, etc., are enumerated in detail, and re-distributed alphabetically even where the orthography and the meaning are the same. Again, the exegesis, or description, is sometimes partly under the French, the residue under the Latin, or English, and so on from A to Z.

If the colloquialisms and foreign technical repetitions were thrown out of the Lexicon, and an English or classical terminology, or a terminology composed of both, the words would probably be reduced to less than half of the existing number, without excluding such words as might be requisite for etymological explanation. If further space should be required for a complete terminology, many long though excellent articles might be reduced from pages to lines without interfering with the clearness and precision of definitions strictly technical. Diet fills thirteen

columns—feigned diseases nine; nerve ten; colic five; climate nearly six; besides detached articles on the climate of places, falsification of drugs, about six. There are many similar articles, as phthisis, parturition, fevers, poisons, cataplasms, lithotomy, etc., etc., all of which are accessible in the ordinary text books, being seldom looked for or expected in a Lexicon in detail.

As a dictionary of the Natural Sciences, allied to and constituting a part of medical education, the Lexicon is therefore incomplete in its terminology. It probably does not contain one-third of the accepted terms in zoölogy, comparative anatomy, botany, etc., notwithstanding its positive superiority over the works of a similar class now in the hands of the student, *anno Christo*, 1858, as the sequel will probably indicate more fully.

As already stated, a medical lexicon alphabetically arranged, is special in its aim, being restricted to the teminology of a particular profession, the technical terms of which it explains, and fixes, thereby separating them as much as possible from the vague common or popular language. The Medical Lexicon of Dr. Dunglison, on the contrary, from A to Z, abounds with common, vulgar, or non-professional words, duly capitalized as if filling the rôle of the principal terms, being often isolated and destitute of references to other equivalent terms, and without any exegesis whatever, and are scarcely any more admissible than the words, Thomas, Mary, night-cap, stocking, etc., as "*chambre*, chamber; churn-milk, butter-milk; *colère*, rage; *femelle*, female; *fille*, girl; *jour*, dies; *abréviation*, abbreviation; *amor*, love; *agglutiner*, to agglutinate; *albâtre*, alabaster; *abeille*, bee," etc. Such words taken at random might be accumulated so as to fill many pages.

In some instances wherein equivalent technical terms might be possibly thought of or expected, references are wanting. Thus, *abeille*, bee, p. 20, and *apis*, bee, p. 78, (translations well enough in a school room,) have no notices appended showing that at page 123 under the vernacular, bee, several lines are devoted to this obsolete medicinal insect, indicating how it was formerly used; many hundred examples of such might be given.

It has been well remarked by Bishop Berkeley, that, "definitions which throw light upon some things, cause darkness in others." Churn-milk, butter-milk, girl, love, etc., are so well understood that no equivalent words can be found clearer than themselves, nor do they belong to scientific nomenclature, or medical terminology. They are not introduced into this lexicon to show their etymologies, which indeed, would be ridiculous.

Some of these words, so far from being technical, are of the most common and trivial character. "*Café; lait*, milk; baby, *infans; bairn;* base-born; *école*, school; *coton*, cotton; *cou; acte*, act; bad, sick," etc. "*Jugement*, judgment,"—difference, one d.

Neither as articles of *virtu* nor as scientific terms, are many words like the following, worthy of places and grave definitions in a medical lexicon: "*Cigar*, a small tubular roll of tobacco used for smoking; *cider*, made from the juice of apples, and, when *good*, is a wholesome drink."

"*Émulsionner*, emulsify." The noun or technical term is *émulsion*, as in the vernacular, not the verb. The translation into the imperative instead of the infinitive, *to emulsify*, is not altogether accurate.

The English, or at least the fully anglicised word *bistoury*, is not found in the vocabulary of the Lexicon, but the French term *bistouri*, is followed by nearly a column descriptive of a knife known to every surgeon, closing with the remark, that "the word is used at times where we would employ knife." Now an English student would scarcely think of looking for any other than the English word, which is found in both popular and technical dictionaries. The Surgical dictionary of Cooper, and the *Lexicon Medicum* of Hooper, describe this knife under the vernacular *bistoury*, in precisely the same words, viz: "any small knife for surgical purposes."

Mercure heads an article several times longer than Mercury. Corn, *cerealia*, is explained under the English word, but it had already been described under the French word *blé!*

On page 144, among thirty-five terms and explanations, twenty-nine, are French, all other languages furnishing only five, the latter occupying about one-ninth of the space. Page 320 has fifty terms, of which seventeen are French.

The Latin term *Inflammatio* gives origin to nearly one column. Inflammation follows, with scarcely any English terms. Next follows *inflammation* (French, at least, in pronunciation,) with a copious enumeration of French terms, thus forming three separate articles of nearly three dense columns. *Lac*, *lait*, and *milk* are partly explained under separate words. A threefold series of articles on the membranes, under Latin, English and French terms follow in immediate succession, filling three columns.

"*Herniaire*, hernial; *hernié*, hernial; *hernieux*, hernial." *Herniaire*, according to the French Academy, is an adjective, which applies either to a hernial part or to a rupture doctor, (*chirurgien herniaire.*) *Hernié* is not found in the dictionary of the Academy, but appears in the Supplement, and

applies to parts constituting a hernia. *Hernieux* is not found in the dictionary of the Academy; in the Supplement it is mentioned as obsolete. To these terms as they are translated in the Lexicon appertain tautology and obsoleteness, not to mention inaccuracy in not giving a substantive as well as an adjective meaning.

"*Typhique*, typhoid, typhus." Here is an adjective translated into two substantives.

"*Carié*, carious; *carieux*, carious." The first is a participle, the latter an adjective. "*Céréolé*, cerate." This seems to be either obsolete or unusual. Is not *cérat*, both technically and popularly the only word in use?

"*Affaires*, menses." This colloquial circumlocution, wholly meaningless, unconnected with the usual phrases (as *cette femme a ses affaires)* being indicative of menstruation no more than of dueling, or twenty other things. *Les règles*, the monthly courses, together with other more indirect paraphrases and synonymes, abound in feminine nomenclature, *affaires* being one remote from the technical.

"Critique, critical." Critique, in French Medical terminology, is a synonymous with *crise*, and isolated from *jour* is wholly extraneous to a medical lexicon.

"Malaise, indisposition." *Malaise*, now anglicised, retains its French signification, namely: badly at ease, positive uneasiness, or restlessness; rather precursory to than characteristic of a well defined actual indisposition; *Sans avoir un mal déterminé;* from malus and the Greek aisa." (Boinvilliers *Dict. des Syn.)*

"*Accès*, Paroxysm." This is an erroneous translation, both in a popular and technical sense. MM. Bayle and Gibbert, in their *Dictionaire de Médecine*, say that "we must not confound this word with the words *attack, paroxysm, exacerbation.*" (T. i. 33.) M. Roche says (Dict. de Méd. T. i. 112,) it is altogether wrong to use these words indiscriminately, and proceeding at considerable length he shows wherein they differ; the attack is a sudden invasion; the acces is the progressive development, the paroxysm, being the exacerbation of pre-existing symptoms, etc.; a patient may have several attacks of gout, each of which may have accesses—each of which may present paroxysms. These distinctions, he says, serve a practical purpose; you give quinine as an antiperiodic to prevent the return of the acces which is generally ineffectual in the attack, and generally exasperating in the paroxysm. Dr. Good says: "among recent writers, and perhaps generally at the present day, while the term paroxysm is applied not merely to fever-fits, but to fits of every violent and intermitting disorder whatever, the term accession is

limited to the *commencement* or *onset* of a fit, its *insultus*, as denominated by the Latin writers;" and hence Cullen speaks of the "accession of paroxysms." (*Nosol.* lxii.) Dr. Dunglison's definition of *Paroxysm* is given thus: "the augmentation which supervenes at equal or unequal intervals in the symptoms of acute diseases. Also, a periodical exacerbation, or fit of disease." This definition applies neither to the French nor English word acces.

"*Frapper*, percuss." Here the French infinitive is translated into English imperative mood, instead of the infinitive *to strike*. In neither French nor English is this technical. *Percuter* (to percuss); *percuter la poitrine*, etc., is a regular verb, according to the Supplemental Dictionary of the Academy.

Is "*Acide acétique faible*," which heads more than a dozen of lines, or "*asservation*, conservation," technical?

As on the one hand substantives alphabetically arranged are often isolatedly distributed through the Lexicon without adjectives or definitions, so adjectives, verbs, participles and adverbs, are similarly arranged without the entity, thing, or essence, which they qualify or in which they inhere, the *accidentia* being without the *substantia*, the attribute without the subject.

A vocabulary of detached popular verbs, adjectives, participles and adverbs is a curiosity in a medical lexicon, as "badly, sick; bony, osseous; blood-like, sanguine; *gigantesque*, gigantic; *écumeux*, frothy; *actif*, active; *abstème*, abstemious; *divisif*, dividing; *linéare*, linear; *aigu*, acute; *adipeux*, adipose; *automnal*, autumnal; *accessorie*, accessory; *cancereux*, cancerous; *capiteux*, heady; *carboné*, carbonated; *carpien*, carpial; *caseux*, cheesy; *chronique*, chronic; *chylaire*, chylous; *comateux*, comatose; *compressif*, compressive; *congénial*, congenital; *conglobé*, conglobate; *convulsif*, convulsive; *corné*, corneous; *critique*, critical," etc.

These words correspond neither with any recognized French or English medical terminology. Had they been given in French, the orthography of the singular and plural numbers, or at least that of the genders, would be necessary, as in *comateux*, comateuse, etc., otherwise the student might mistake such adjectives for different words, standing as they do unconnected with nouns, numbers and articles. Thus *convulsive*, applied to a cough and to the pulse, is spelled differently, as *pouls convulsif*, *toux convulsive*, not to mention the difference rising from number.

"*Agissant*, active."—see Dict. de Méd., 1, p. 409. This is applicable to the masculine but not to the feminine gender; an active woman, or an active medicine require a different orthography. Here the orthography is wrong as applying to medicine. M. Roche, in the *Dict. de*

Méd., has an article headed *agissante;* médecine *agissante*, which is explained at length as the antithesis of *expectant* medicine. "*Actif*, active." This word is in the same category; an active poison and an active life differ in orthography, because they differ in gender. "*Dur*, hard." A hard pain and a hard digestion differ in like manner. "*Sérieux*, serious." A serious man and a serious malady differ seriously, and so on from A. to Z.

Classical words in no wise technical, (as abominatio) are inserted, and many which are technical are omitted; while others, again, related to medicine are explained only in a popular sense. For example, "*Denarius;* a Roman coin, equal in value to about ten cents, or eight pence, English. It was marked with the letter X, to signify ten asses." This bit of antiquity is perhaps related to medicine in the matter of fees.* The doctor who has dimes for which he can get "eight pence English," may almost double his money at once by exchanges.

The medical meaning of this term is not even alluded to, although Celsus gives, not its pecuniary, but its posological signification in detail showing that seven denarii make one ounce; the denarius he subdivides in six parts, comparing them with the Greek weights, etc.: "*Sed et ante sciri volo, in uncia pondus denariorum septem esse; unius deinde denarii, pondus dividi a me in sex partes, id est sextantes; ut idem in sextante denarii habeam, quod Græci habent in eo, quem obolon appellant. Id ad nostra pondera relatum paulo plus dimidio Scripulo facit.*" (Lib. v. c. xvii.)

"*Abondance*, plethora." This is not in any of the several medical dictionaries in French that have been examined. *Abondance* is not even with the French the technical name for plethora, but is a non-technical

* Although asses are not now current, I received one, not as a fee, but as a gift, from a young lady who had been my patient. Supposing it to be a coin of Constantine's reign, yet doubtful of my skill in the science of Numismatics, I submitted it to the scrutiny of a learned lady, who, upon returning it a few days ago, kindly sent the following explanatory letter :

CONSTANTIUS, P. P. AUG.:

CONSTANTIUS, FATHER OF HIS COUNTRY, AUGUSTUS.

Fel. Temp. (?) *Reparatio; vel Fel. T. Imp.* (?) *Reparatio.* THE EMPIRE HAPPILY RESTORED.

The latter inscription is very obscure, indeed, nearly illegible ; but as Constantius II, having conquered several rivals, finally became sole Emperor. This is at least a plausible, if not probable, reading. The symbol on the standard being the initial Greek letters of the name of Christ, shows that the coin was struck in the reign of a Christian Emperor. Now the first Constantius, the father of Constantine the Great, was a Pagan. Hence I conclude that the coin belongs to the reign of Constantius II, and the symbol is probably the same as that which was borne by his father Constantine, on the celebrated *Labarum*.

If indeed this is an *as* of that time, (360, A. D.,) it is probably the most remarkable *as* in New Orleans.

* * *

To Dr. B. DOWLER.

one which is turned into English by the word *abundance.* Boinvilliers, in his learned work (*Dict. Universal des Synonymes,* p. 7,) places *abondance, opulence, et richesse* as synonymes, but in no case does it either deviate from the English abundance, or indicate the technical idea of plethora, either physiologically or pathologically.

"ABONDANCE. *Grande quantité.*" (*Dict. l'Acad.*) Nor does Dr. Dunglison's exegesis of plethora agree with abundance simply, for plethora is, as he says, a mordid state, attended with "heaviness, torpor, lassitude, vertigo, tinnitus aurium, flushes of heat, hæmorrhages," etc., agreeing with even the popular French dictionaries: "PLÉTHORE; *Plénitude, excessive d'humeurs.*" (DeWailly. Dict.)

"*Apparatus immovable, appareil immobile,* immovable bandage, permanent bandage," etc. This term is usually written in French, *appareil amovo-inamovible* or *a inamovible.*

"*Divisif,* dividing." Here *divisif* is translated into an isolated English participle, whereas, it is an adjective, and is applied to certain kind of bandage which had been already explained in the lexicon.

"*Accablement,* torpor." M. Boinvilliers connects *accablement, abattement,* and *découragement* as synonymes; but the notion of *torpor* is wholly excluded. (*Syn.* 10.) M. Bouillaud says that this word is employed in pathology only figuratively, as when a patient suffers from the feeling of a heavy burthen. (Dict. Méd.) Boinvilliers limits its pathological meaning to an overwhelming sensation, like that of fatigue.

"*Progression,* walking." This translation is unexpected. The French and English have the same orthography and the same meaning for the word progression. Walking is not progression any more than, nay, not so much as running or flying.

"*Méningette,* pia mater." This is not the accepted, or at least not the usual term for pia mater, which latter the French anatomists call *pie-mère.*

"*Large,* broad." This is a translation not to be admired. Large is large, largus; great in every dimension as well as in breadth.

"*Blasè,* cloyed; an epithet given to one," etc. This participle here, without UN HOMME, is little better than a vulgarism. It is not a technical term any more than "old fogie," "used up," "easy virtue," "fast man." Originally, it appears that this word signified the forcing one to drink, "s'user à force de boire;" on the contrary, in modern times, it applies literally to voluntary excesses in eating and drinking, and figuratively to that physiological and moral state in which the sensibilities are blunted or paralized from excesses.

Brownian, Brownism, Brownist, Brunonian, and Brunonianism, ap-

pear in this dictionary, yet without any definition of what these words mean, except that they relate to the doctrines or followers of John Brown; the doctrines themselves are not given or referred to in connection with these words.

The English word, coffee, is omitted, but *café* is not.

"*Abortif*, abortive." Here is an error in translation. A French noun is translated by an English adjective. The French as well as the English use abortive adjectively when a thing is thus qualified, but the French, in their terminology, use *abortif* as a noun signifying either a fœtus born prematurely, or a drug which causes abortion. It is not said, without straining the English, that Mrs. A. had an abortive, but a miscarriage, or an abortion. Crabb, in his English Synonymes, classes these terms as synonymous, (p. 125.) An abortive may be applied to a drug, but the French word and its translation, standing as they do in this dictionary, are meaningless, either in common parlance or terminology.

"*Abouchement*, anastomosis." Here the popular takes precedence over the technical French, *anastomose.*

"*Aboutissement*, suppuration." This word is pronounced obsolete by the Academy, so that it is neither popular nor technical. In the next place, the French and the English words are identical, namely, suppuration.

Coton being French, is selected in the place of the vernacular, cotton, as the suitable word under which to give the various preparations of that article. *Cou*, not neck, is preferred for nomenclatural ends. Both *coton* and *cou* are in no wise technical, but familiar.

"*Métis*, hybrid." Métis in its primary meaning, does not appear to imply what the English word hybrid does, but is used to designate a person born of an European and an Indian, and *vice versa*, although it is also used in a secondary sense to express hybridity among animals.

The word hybrid in English, applies to plants and animals rather than to human beings, as in the French *métis*. The Academy never intended to say that hybridity in the English sense, inhered in the offspring of the red and white races. The supplement to the Academy's Dictionary has the word *hybride*, which might have been easily translated into hybrid.

"*Rapport*, a report." The English word is not mentioned; the Latin word *relatio*, has no definition, but is simply translated; the French word, *rapport*, in another place, has twenty-two lines devoted to its explanation, all of which is identical in meaning with the English word, report.

"*Déclin*, decline." This word is technical neither in French nor English. In the latter it is sometimes synonymous with consumption, but not in the former. "*Délirant*, delirious." In this sense it is figurative, being rarely used. "*Aromate*, aromatic." This, which is a French noun (the adjective being *aromatique*,) should have the article, *an*, to show that it is to be taken substantively. Many similar examples might be adduced from the columns of the lexicon.

"*Capiteux*, heady,"—an adjective applied to wines which affect the head, as champagne—heady, to obstinate, contumacious or headstrong people.

"*Phlogiston*, hydrogen." This is a brief explanation of a word or theory which, until recently, occupied a vast space in chemical speculations, and which, in a historical point of view, cannot soon become obsolete, owing to its important influences and results.

"*Sporadic.* Diseases which supervene indifferently in every season and situation, from accidental causes, and independently of any epidemic or contagious influence." This definition is fundamentally erroneous. The National Cyclopædia (Lond., 1850,) says: "Sporadic is a term applied to any disease which, being usually epidemic or contagious, occurs at any time in a few persons, without spreading extensively through a district." (XI, 323.) Brande, Webster, Worcester, and other lexicographers, both technical and popular, define this word differently from Dr. Dunglison. Sporadic yellow fever, or small pox, does not necessarily, commonly, or perhaps ever, occur independently of all epidemic or contagious influence. Sporadic cases, on the contrary, are viewed by the people and physicians as positive or probable precursors of the presence of these influences, and indicative of approaching epidemics.

Dr. Dunglison's definition of *creole* is obsolete. Creole is not restricted to the offspring of immigrants born in the old world, but includes their children's children, *ad infinitum*, white and black; cattle and chickens; oranges and newspapers, etc., being synonymous with the word *native*. The South side of the Republic has a better right to interpret this word than the foreigner quoted and adopted in the lexicon.

"*Morphology*, anatomy." This is brief, but it is without reference. If the student, however, should happen to turn to the word anatomy, he will find that "anatomy has also been called morphology"—nothing more! It would be difficult to name a scientific term among "the 60,000 words" in this dictionary, the definition and illustration of which would be of more interest to the student. Its modern history is a novelty. The fundamental principle which it represents in the organic and physiological sciences, is called by Dr. Whewell, a real advance; by Sprengel,

wonderful. In 1790, Goëthe, in a poem, alluding to the apparent confusion and difference among the plants in a luxuriant garden, says:

> "All the forms resemble, yet none is the same as another,
> Thus the whole of the throng points to a deep hidden law,
> Points at a sacred riddle. Oh! could I
> Whisper the fortunate word by which the riddle is read."

"All the parts of a plant are, or at least represent the successive metamorphoses of the same elementary member. In 1795 Goëthe published a 'Sketch of an Universal Introduction into comparative Anatomy,' in which he attempts to establish an 'osteological type,' to which the skeletons of all animals may be referred."

From plants the principle of developed and metamorphosed symmetry was carried into anatomy and physiology, and had it been no more than a poetical hypothesis, its beneficial effects as a working principle in physiology and natural history cannot be questioned.

Mr. Mill, in his work on Logic, says: "This great problem, the most difficult in all physics, the ascertainment of the ultimate laws of organized nature, is one which natural science seems now at least to have fairly come up to; and a beginning has been made at the point where the phenomena appear most accessible to experiment, namely: in separating the effects of partial from those of general causes. I allude to the new and infant science of morphology. Plants and animals in the process of growing up from their germs, have a tendency to develop themselves in a much more uniform manner than they in fact do; that the differences, for example, of leaf, flower and fruit, are mere modifications of one general phenomenon—joint results of one common tendency, and of several partial causes combining with it."

"TYPE. The *type*," says the Lexicon, "is the order in which the symptoms of a disease exhibit themselves and succeed each other. It may be continued, intermittent or remittent." The brevity of this explanation as compared to others less important, which sometimes occupy more than twenty columns, is not the smallest objection that may be urged against it. The fundamental idea, that of phenomenal succession in symptomotology, is erroneous.

Type, in the actual state of science, in anatomy, in physiology, in histology, in micrology, in pathology, in morbid anatomy, in pharmacology, in chemistry, and in natural history, is a primary form which serves to classify, represent or characterize a group or genus, including to a certain extent aberrations, the morphological changes of which are conformable to, or repetitions of the same fundamental law.

In the New Orleans Medical and Surgical Journal, for July, 1848, in

my Review of Solly on the Brain, is an exposition of the doctrine of types, (or typology, as I called it,) as appertaining to physics, chemistry, biology, pathology, botany, zoölogy, etc.; and here I may quote from myself, as follows: "In zoölogy this (the doctrine of Types) is at once a method of study and a fundamental principle of classification. It selects an example or individual form, (and this applies to disease, morbid anatomy, etc.,) combining in the utmost intensity the essential organization or *model* of an entire family, through all its ascending or descending gradations, varieties and even monstrosities. The word metamorphosis, or morphology, either as a synonyme or substitute for typology, seems wide of the mark. They differ more, strictly speaking, than genera and species; the former is so to speak, dynamic, the latter static; the one denotes the great general law of transformation, by which change follows change—shape, shape; the other possesses the idea of individualism, fixity, the essential characteristics of many in one, and of one in many, the representative of the whole in a part, and the point of departure for both synthesis and analysis. Morphology seeks to reduce the multiform laws of organization to one, or to the fewest possible number of ultimate principles; typology is more intent upon seizing prominent criteria in in their material, individual manifestations; the one is more profound, the other more practical; the one seeks to solve the problem of organic nature by the more direct route, and by main force as it were; the other is content with analogies, and with observing such points of contact as may be more easy of access and of more immediate use."

Small pox is typical of contagious, and epilepsy of convulsive diseases, intermittent of paroxysmal, and typhus of continued fevers; quinine of anti-periodics, and opium of narcotics; different and sometimes the same diseases are characterized by types as the sthenic or asthenic; mankind, animals, etc., are grouped conformably to their central types and ideal archetypes.

Dr. Dunglison under the word *experience* gives a few explanatory lines, in which he says "*false experience* is extremely common; and that a man had better, in many instances, trust to that which he has learned from others than to his own fallacious observations." Now false experience is a contradiction. Personal or general experience within one's own knowledge must be true, ever true. Individual or general experience may be and often is falsely interpreted; the facts of nature, however, are eternal, and cannot be falsified by the misconception of their experimental import. "Observations which are fallacious," are in the same category, never being false, though falsely construed. The verity of the observation is not invalidated by the mistaking of its teachings, and applica-

tions. False experience and false facts are misnomers, fictions, or rather contradictions.

Dr. Dunglison's definition of *sensation*, (to take its least disputable point) is not quite accurate: he says, "*objective sensations* are produced by impressions on the peripheral nerves; *subjective sensations* are such as originate centrically," etc. Now all sensation is identical in this, namely, that it is always *subjective;* apart from a conscious state or the feeling of it, howsoever it may be excited, there can be no sensation at all; its cause may be external or objective, or internal or centric. There is no identity in the cause and feeling of a burn. The objective cause, fire, and the feeling or subjective state, pain, are not even analogous. The fire has no subjective feelings of pain, nor any other.

Under the term "Symbol," no symbols but those which are used in prescriptions, have been enumerated. Symbolical notation in chemistry now universally adopted by writers is more difficult to understand and retain in the memory than even the posological signs which are as familiar to the student as A B C.

Classification, as defined by the Lexicon, is vague and unsatisfactory, the principles of structure, function, subordination to central, individual dominant types, affinity, differentiation, or resemblance, being wholly ignored.

"*Mosquito*, (S.) diminutive of (S.) *mosca*, 'a fly.' *Mosquito.* A very troublesome insect in warm, moist situations. Curtains, called *mosquito curtains* or *bars*, are used by way of protection. *The entrance of mosquitoes into the bedchamber may also be prevented, to a certain extent, by keeping the doors and windows closed until the lights are extinguished.* The pain and itching from the bites are alleviated by washing them with hartshorn." This description, which is quoted entire, is incomparably inferior in accuracy to that of Herodotus or Pliny. Herodotus describes the habits of the mosquito; Pliny wonders at its physiological endowments, seeing that Nature had bestowed the five senses upon an organism so minute. Whereupon he says "*ubi tot sensus collocavit ut in culice.*"

The Lexicon neither gives the scientific name nor the classificatory position of this member of the small tribe constituting the *culicidæ*, while the popular description given is remarkably inaccurate, inasmuch as the proposed method of preventing mosquitoes from entering houses by keeping the latter closed until the lights are extinguished, is the surest and best method of inviting the entrance of these insects. They dread a strong light. In a cloudy day, or on the approach of darkness, they swarm out from their dark hiding places, whether within or outside of houses. Extinguish vivid lights, and open windows and doors, and

then expect a great mosquito invasion and little sleep, if deprived of a mosquito curtain :

*Mali culices * * *
Avertunt somnos.*
[HORACE.

It has been already mentioned that the Medical Lexicon has omitted one of the most important technical terms concerning Southern medical jurisprudence, namely, *redhibition, redhibitory actions, redhibitory vices and maladies.* A treatise upon this subject (now that physical diagnosis has attained an accuracy never before known,) is imperatively called for in the slave-holding States, wherein litigation is of daily occurrence in this behalf. As the writers of dictionaries and of systems of Forensic Medicine ignore the social system of the South, it may be allowable to give the following fundamental exposition, which concerns the slave population, now nearly four millions in number.

The Civil Code of Louisiana, says:

"Redhibition is called the avoidence of a sale, on account of some vice or defect in the thing sold, which renders it either absolutely useless, or its use so inconvenient or imperfect, that it must be supposed the buyer would not have purchased it, had he known of the vice. (Art. 2493.)

"The vices of body are distinguished into absolute and relative. Absolute vices are those of which the bare existence gives rise to redhibitory action. Relative vices are those which give rise to it only in proportion to the degree in which they disable the object sold. The absolute vices of slaves are leprosy, madness, and epilepsy." (Arts. 2501, 2502.)

Diseases of the heart, consumption, etc., are probably more frequently causes of redhibitory actions, than the three latter maladies.

If the above paper possesses any intrinsic value for the reader, its length cannot be objectionable, because the topics which it discusses are so numerous and varied that no one occupies much space; of this matter, and of the expediency and necessity of speedily making a new lexicon free from verbiage, vulgarism, error, repetition, and barbarism — a lexicon concise, exact, classical, and without perfidy towards the mother-tongue—a lexicon so comprehensive that it shall include a complete terminology of the existing state of medicine and its allied sciences—of all this, I say, let the unprejudiced, young, energetic men who are not contented with mediocrity, and are now pressing forward in the ranks of the medical profession, form their own opinions; and if this paper has been worked out in a cloud of error, let them charitably suppose, what is really the truth, that its purpose is good, even though its execution be bad, thus taking the will for the deed.

An universal conventional, fixed terminology and nomenclature chiefly in Latin, with definitions and descriptions in the various mother-tongues, including the subordinate trivial terms of each, would facilitate the processes of study and education incalculably, without affording any just ground of complaint that might arise from prefering or rejecting the language of any particular nation. All nations would thus fall into one line so far as a general classification is concerned.

Can any one doubt that it would be advantageous to the anatomical student of what nation soever he may be, if the *shoulder blade, omoplate,* (French,) should always be termed *scapula;* the *hips, bassin,* (Fr.,) the *pelvis*? and so on in pharmaceutical preparations, plants, and medical chemistry? It is not necessary to be a classical scholar in order to write and to understand these terms, which, if generally adopted, would soon become familiar as the mother-tongue to scientific men, though not linguists.

An exact knowledge of medical terminology, with every advantage which a clear established exegesis or description, and uniformity of plan can bestow, is a laborious but indispensable part of study.*

* While revising the manuscript of this article, many paragraphs of which have been suppressed from a fear of trenching too much on the reader's patience, some numbers of that excellent Journal, the *Gazette Hebdomadaire de Médecine*, for January, 1858, have been received, in which is commenced a series of articles by M. VERNEUIL, having the following title, which show that French physicians need a new Medical Dictionary, or at least a supplement to existing dictionaries: "*De la necessité d'un Dictionnaire Supplémentaire à l'usage es Médecins; Pour faire suite aux erreurs, lacunes et imperfections de la littérature médicale.*"

ART. XIII.—*Anatomical Anomalies in the Human Subject:* Reported by WM. C. NICHOLS, M. D.

NEW ORLEANS, January 31, 1858.

MESSRS. EDITORS:—The following remarkable example of transposition of the thoracic and abdominal viscera, was discovered a few days ago, in our dissecting room, by Dr. Nichols, my assistant demonstrator; and my attention being called to it, I requested him to make a memorandum of the facts. I herewith transmit you a note to me on the subject, which you may, if the facts are thought worthy of a place in your Journal, lay before the profession.

In the "*Histoire des Anomalies*," of Geoffroy Saint Hilaire, as well in works of less extent, will be found numerous examples of displacements of organs, and presenting an infinite variety of combinations. Their curious relations, too, with normal and morbid anatomy, as well as with the laws of development, have been pointed out. I do not, therefore, present this case as one of entire novelty; but it may have some peculiarities which would attract the attention of the student of *têratologie*, and afford a ray of light by which to elucidate its imperfectly understood laws.

J. C. Nott,
Prof. Anatomy, University of Louisiana.

New Orleans, La., Jan'y 29th, 1858.

Dr. Nott — *Dear Sir:* Being requested to dissect out the anatomy of the neck on a subject in the dissecting room of the University, I found that the parts revealed by the scalpel did not correspond with the idea I was attempting to illustrate. The surgical anatomy of the cervical region was the point in question; and I discovered that the relation of the nerves and blood-vessels presented a diversity not hitherto observed in my dissections.

The first anomaly apparent, was the unusual course pursued by the external jugular vein, which, instead of passing in its accustomed track, came obliquely inward towards the mesial line, and taking the course of the carotid sheath, joined the subclavian near its junction with the internal jugular vein. Other deviations from what are considered normal type, stimulated further research; and, on removing the anterior walls of the chest and abdomen, the problem was solved in the disclosure of the fact, that all the viscera of the body were transposed. In compliance with your request, I herewith subjoin a detailed account of that transposition.

The heart was placed to the right of the mesial line. This position of the heart led to a compensatory change in the size of the lungs — the right being the smaller, and consisting of only two lobes. The aorta sprang from the right ventricle, which was found with firm, thickened walls, whilst the left was thin and flaccid. The remaining vessels proceeding from, and converging to, this viscus, presented like deviations. The pulmonary artery arose from the left ventricle; the pulmonary veins emptied their contents into the right auricle. Here evidently the arterial assumed the place of the venous heart.

On account of the situation of the heart, the arterial trunks given off from the arch of the aorta, though coming in proper succession, distributed the arteria innominata to the left side; then followed the

right carotid, and lastly, the right subclavian artery. The aorta continued its descent without approaching the spine, and perforated the diaphragm to the right of the mesial line. Just above the aortic orifice was found the œsophageal opening, and the ascending vena cava pierced the diaphragm on the left side.

In the position of the abdominal viscera, the same interesting diversity existed. The liver occupied the left hypochondriac region; and the stomach and spleen occupied a corresponding site on the right side. The right kidney was on a higher plane than its fellow; the pyloric orifice of the stomach turned to the left; the duodenum, with its flexures, was under the margin of the right lobe of the liver, extending from the left of the spine into the right hypochondrium; the head of the colon was placed on the left iliac fossa, the ascending took the usual route of the descending colon; and the sigmoid flexures, resting in the right iliac region, then the rectal portion of the intestine over the margin of the right psoas muscles.

Returning to reëxamine the vascular system, the aorta was found passing throughout the whole of its extent to the right of the spine, making the left intercostal and emulgent arteries longer than those of the right side. The right phrenic artery was a branch of the cœliac axis; the left came from the aorta about an inch above the cœliac axis. The superior mesenteric artery presented its convexity and concavity in a direction opposite that usually observed, and the inferior mesenteric sent its hæmorrhidal branch into the pelvis, directly across the right iliac artery. The division of the aorta into the common iliac arteries took place not in the median line, but to the right of the fourth lumbar vertebræ.

The position of the respective viscera necessitated a corresponding change in the disposition of the important venous trunks. The ascending cava took its origin beneath the left iliac artery—continued in its course to the heart along the left of the spine, and received the left spermatic and the right and left renal veins; the right renal vein being longer than the left, and crossing the spine above the aorta. The right spermatic opened into the right renal vein. The right vena innominata, longer than the left, crossed the roots of the large vessels arising from the aorta, met its fellow to form the descending cava; which latter vessel continued along the left of the mediastium to be distributed to the left auricle.

The left pneumogastric nerve passed over the left subclavian artery, returning its recurrent laryngeal branch beneath that vessel; whereas, the corresponding nerves of the right side, came in relation with the arch of the aorta. Moreover, the arch of the aorta, inclining to the right,

curved obliquely above the right bronchus, which was found greater in length and of less diameter than the tube of the right side. Other changes of minor importance were noticed in this caprice of nature.

Such anomalies as enumerated, being interesting to the practical anatomist, as well as to him who delves into the more minute investigation of the component parts of the human frame, I willingly submit this description to your consideration.

With high regard, your obedient servant,

WM. C. NICHOLS.

ART. XIV.—*To the Honorable, the Members of the Senate and House of Representatives of the State of Louisiana, now in Assembly convened, at Baton Rouge.*

GENTLEMEN:—The reports which different Boards are by law compelled to present every year to your honorable body, sometimes submit to your consideration facts incomplete and inaccurate, and often leave aside important and decisive facts, thus presenting to you only one side of the question. The deplorable result of such reports is to lead you into error, and, as a consequence, to make you persist in vexatious measures, detrimental to the community, and to authorize extravagant and useless expenses which your greatest wish is to avoid.

The following reflections were suggested to us by reading the Report which the Board of Health has lately submitted to your consideration. This report abounds in erroneous interpretations of facts, and in poetical and fastidious lucubrations on matters entirely disconnected with the subject; and restricts itself to a disdainful silence with regard to patent and undeniable facts, the revelation of which, would have upset the edifice it was trying to build.

The said Report contains a scientific part which belongs essentially to medicine, and it is in connection with that scientific part that we thought it our duty to submit to you the following reflections. We notice that a member from the city has already taken up the financial question of the Report, and we are satisfied that with the zeal and the desire to do well we know him to possess, if there is anything "rotten in Denmark," he will certainly point it out.

The Board of Health, in the first page of its Report, speaking of an appropriation of fifty thousand dollars made by Congress, tells you: "It is a question of *vital* importance to the *efficient* administration of a system of quarantine laws, that every practicable facility be afforded to the thorough fumigation of vessels and cargoes, by discharging and landing the same, whenever this becomes imperative either through the foulness of the one, or the susceptible character of the other," * * * "and," it adds, "without warehouses and wharves, this has been impossible." That is to say, that what was of a vital importance could not be put in execution. It is, in fact, known by every one that when the ship Protector arrived at the Quarantine having on board cases of varioloid, neither the vessel nor the cargo was subjected to a complete fumigation. Most of the passengers on board of that vessel were allowed to land the day after its arrival at the Quarantine, and came up to town. One of them, a young man from Bordeaux, detained for ten days at the Quarantine, and, having left with the consent of the attending physician, came to me the day after his arrival in town, with all the symptoms of varioloid. Such is the origin of the numerous cases of small-pox which have occurred in town since the arrival of that ship. All this does not prevent the Board from saying, page 17 of its Report, "That the sanitary condition of the city has been very favorable during the year just closed—the total mortality being 6,067, which is an excess of 378 deaths over the year 1856, and 1,638 less than the last eight months of the year 1855, when the mortuary records of the city passed into the charge of the Board, which naturally leads to the hypothesis, that if, without warehouses, without wharves, the mortality of a whole year could be diminished by 1,638 on the mortality of eight months only of the preceding year, what cannot be expected from the Board of Health, when, with the fifty thousand dollars of the federal government, they will have built warehouses and wharves?" But we will see in the course of our remarks, whether this diminution in the mortality has been the result of the sanitary measures taken by the Board of Health, or whether it was not due to the natural course of things.

We follow the Report step by step, and we come to the Quarantine Convention held in Philadelphia in May of last year. We would have liked not to entertain you with that unfortunate Convention, which, notwithstanding the great noise it made, merely brought forth a ridiculous little mouse; but the Board of Health, by allowing it thirteen pages of its Report, and by spreading over four pages the result of its efforts, forces us to speak of it. The Convention was assembled for the purpose of reviewing the laws of the several States, establishing quarantine, with

the view of reducing their provisions to some common standard, in accordance with settled medical and scientific opinions.

This was a problem not only difficult, but perfectly impossible to solve; and, although the Board of Health knew "how difficult it was to understand how a principle of uniformity was to be made operative in places so unequally situated, in relation to the perennial sources of yellow fever as New Orleans and Boston, it nevertheless agreed to send three of its members to Philadelphia. The traveling expenses for those gentlemen were only to be fifteen hundred dollars, and the Board of Health, having proved itself a self-supporting institution, was well able to foot the bill."

Although the Report adds, "That its labors (the Convention's) have been neither altogether barren nor uninstructive, we think will appear on a careful examination of them;" still the Board acknowledges that the Convention did not meet its expectations when it says, "that it has met the expectations of those who convened it, or of the great body of medical men outside, who looked to it with anxious hopes or cold misgivings, or of the yet greater public, who trusted to this experimental assemblage for some useful development of knowledge, experience, hints, or suggestions, by which physical evils were to be successfully combated, and the maladies and sufferings incident to social bodies redressed, cannot be affirmed in an unqualified manner." When we shall come to examine the work of the Convention, we will see how very small a proportion of good and of instruction may be derived from it.

Notwithstanding what the Report says, the opinion of physicians, North and South, did not change with regard to the non-contagion of yellow fever, and there is not a single member of the profession whose convictions have been altered by the epidemics of 1853–4 and 1855.

Outside of a narrow circle, the number of contagionist and non-contagionist physicians with regard to yellow fever, remains what it was before 1853. "The common sense of the people," says the Report, "unskilled in the refinements of scientific hypothesis, solved the problem by a process quite as logical as, and certainly far more practical than, the conjectures of the learned." After the epidemics of 1853 and '54, the people did not show any more common sense than ordinarily; they did not take the trouble to observe facts, to analyze them; they were frightened; the contagionist physicians improved that chance to speak of quarantine, and the people called for the establishment of quarantine as a means of protecting themselves against future epidemics.

If it be true, that in 1853 yellow fever, in going beyond its ordinary limits, invaded localities until then undisturbed, it is not correct to say: "Whenever a case of fever occurred, the rule was that it did not terminate

with it, but that other cases happened in proportion to the nearness and frequency of intercourse with it, and that this took place irrespective of localities, habits, modes of living, etc.; that pine barrens offered no more shelter against its inroads than vicinage to swamps, which was supposed to favor it; that the solitary hamlet was as defenceless against its ravages, after its introduction, as the crowded city or town." To point out only one case out of many: The captain of a schooner is taken sick seven hours after leaving the lake end of the new Canal; he lands at Bayou Lacombe, where he dies three days after, with all the symptoms of yellow fever. He had been carefully attended to by persons of the place; no one is attacked with the disease. The disease had died away with the patient. Every one knows that steamboats leaving New Orleans have landed persons attacked with yellow fever at different places along the coast, amongst others, at Memphis; that out of those patients some got well, others died, without the disease attacking any inhabitant of the place. On the other side, the plague, little minding those too generous men who, thinking that a steamboat coming from New Orleans had cases of yellow fever on board, armed themselves with guns and ordered the captain not to approach the wharf, would jump in the midst of them and destroy them as unmercifully as it had done the people of the city. It is also known that in some of our western parishes, the inhabitants had established *cordons sanitaires* and intercepted all communications, either direct or indirect with the infected places. These precautions did not prevent yellow fever from breaking out among them, and treating them with all its severity. These are some of the many facts, the value of which we will be called upon to appreciate directly.

But let us return to our convention. The report says: "This policy (enactment of laws and ordinances establishing quarantine regulations as a means of defence against its future ravages,) was then inaugurated, not in obedience to the judgment of the medical community, but in spite of it." Let us notice, *en passant*, the opinion of the medical community, which, although in direct opposition to the common sense of the people, is not thereby invalidated.

"With this state of antagonism between public policy and doctrinal opinion in the profession," says the report, "the convention was assembled, and the materials for its deliberations prepared and digested by those who shared in the ascendant medical dogmas obtaining on this subject. Under these delicate circumstances it was altogether fit and becoming that the subjects presented to the convention should embody points as little hypothetical as their nature would admit. It was manifestly impossible in the balanced state of public and professional senti-

ment, to have affirmed or denied the exclusive views of either doctrine without controversies, the result of which would, at least, have been questionable, and for the deciding of which the convention possessed no more positive or abundant proofs than existed previous to its assembling. What alternative was then at its control but to embody in a few, plain, practical and general propositions all that could be safely assumed as true by the common consent of the large body of its delegates? In this completion of its labors, we are constrained to affirm that it has gone as far in the vindication of the policy of quarantine as admitted facts will warrant. To have gone farther would have been to commit itself to a partizanship, to doctrines the extravagant development of which can only be justified on the ground of incontestible certainty."

Let us translate "*public policy*" by "contagionist," and "*doctrinal opinion*" by "non-contagionist," and we will be able to see through the obscure phraseology of the report. We see that the convention contained a great majority of contagionists, that notwithstanding the superiority of their number, those contagionists did not bring in the convention arguments strong and numerous enough to beat their opponents, the non-contagionists, and see their own doctrines triumphant. The drawing up of the programme of matters to be subjected to the discussion of the convention, is trusted to a few members, who embody in a few, plain, practical and general propositions, all the assertions that this great conclave of delegates composing the convention can proclaim as so many axioms. The majority of the convention being composed of contagionists, those resolutions must necessarily be in favor of quarantine.

People had a right to expect something new, original, from a convention numerous and composed of physicians from the most distant points of the States.

Still, from the shock of all the intellects composing the Philadelphia convention, not the least light has been reflected. The propositions which constitute a summary of their work, as published in the report, contain no new information—not a new idea. They are merely the reproduction of old European ideas, now forgotten. Some of them, however, are of a simplicity which will certainly not be envied by their transatlantic orignators. How can it be, will you ask me? One of the two delegates sent by our City Council, at a cost of one thousand dollars, gave an account of the deliberations of the Philadelphia convention, in a letter addressed to the *Bee*, and full of that wit and that charm of style which characterizes everything coming from his pen, as an extract from that letter will prove:

"PHILADELPHIA, May 17th, 1857.

* * * * I arrived in Philadelphia on the 12th, and it was on the 13th that the medical convention was to meet, instead of the 15th, as my commission read. The first meeting took place on the 13th. An old and respectable physician of Philadelphia, whose name escapes my memory, made a short and brilliant allocution, in which he welcomed the delegates from the other States, and explained in a plain and brief manner the motives of our meeting, hoping, or rather giving us the assurance that order and obedience to parliamentary courtesies would not cease to reign. We then proceeded to organize the meeting. The result was the election of a President, and the appointment of a "Committee on Business." After the exchange of a few unfruitful words the meeting adjourned to 4 o'clock in the afternoon. The second meeting was nearly as fruitless as the first. A violent indisposition prevented me from being present at the two meetings of the next day. On the 15th, the last meeting took place. It was marked by a little skirmish between Dr. Jones, of New Orleans, and Dr. Condie, of Philadelphia, originating with the examination of the report of the Committee on Business.

One of the resolutions said that no member would be allowed to speak more than twice, and ten minutes each time, on the same subject. Dr. Jones objected to this clause, on account of the Committee on Business not having been instructed to establish rules, but only to prepare the work, which seemed to be very fair. Dr. Condie rose and defended that clause, saying it had been established to prevent delegates from making long speeches, wasting precious time which might be better employed. Dr. Jones thought he discovered in those remarks a personal attack, and answered. Dr. Condie, in his turn, replied with some bitter words, when the President by intervening put a stop to the discussion. Order was re-established, and the debate took a peaceful and amicable turn.

The long and the short of all this was that the Northern Delegates did not wish to engage in a discussion about yellow fever; with regard to which Dr. Condie said that for him the discussion was quite useless, he being perfectly satisfied of the non-contagiousness of yellow fever. So that after a series of resolutions, amendments and counter resolutions, the whole was presented and adopted or rejected in the greatest hurry. The convention voted an adjournment until next May. The second convention will take place at Baltimore. In the meantime a committee appointed for the purpose, is instructed to prepare the work. May God help them all, and send some order in that tower of Babel!"

We have seen with what deplorable hurry the Philadelphia convention adopted and spread through all the States a series of resolutions pur-

porting to be the nearly unanimous expression of sentiment of its members. Let us review these resolutions and see if we can say of them what a great writer of the last century said of a bad book; "however bad it be, still we may find something instructive in it."

To make it easier, let us follow the Board of Health of Louisiana. The first and second propositions read thus: "There are certain diseases which may be introduced into a community by foul vessels, and cargoes, and diseased crews and passengers. These diseases are small pox, and, under certain circumstances, typhus fever, cholera and yellow fever."

The third proposition reads: "3d. When the latter diseases (typhus or typhoid fever, cholera or yellow fever,) are introduced in this manner, their action is limited to individuals coming within their immediate influence, and cannot become epidemic unless there exist in the community the circumstances which are calculated to produce such disease independent of the importation."

The convention proclaims the doctrine of the transportability of yellow fever, as well as of small pox. The Board of Health rejoices at it; the argument will be brought before the Legislature for the purpose of maintaining the quarantine laws. In its joy it says:

"Is it, then, no indication of the shock medical doctrines have sustained by the incidents of pestilential yellow fever since 1853, that in the first public assembly of a convention of medical men, a point formerly so controverted, is conceded, and yellow fever is classified with small pox and typhus, as disorders susceptible of introduction into communities by the same vehicles and in the same modes? In contemplation of such a result, we must surely regard the convention as having moved forward with a bold stride towards the frank and open admission of the doctrine of the transportability with all its consequences."

From the fact that the Philadelphia convention admitted the transportability of yellow fever, and all its consequences, does it follow that such a doctrine is true and must prevail? We repeat it, the work of that convention is the work of a few physicians having the same opinions, and whose convictions were well settled. The report they presented to the convention was not discussed. We see nowhere the arguments brought for or against, those used to establish or oppose the transportability of yellow fever. The correspondent of the *Bee* tells us that those gentlemen had resolved not to speak of yellow fever, or at least not to discuss it. A few propositions had been drawn up beforehand; they must be adopted by the convention without a frown; "and," says the New Orleans delegate, "after a series of resolutions, amendments and

counter-resolutions, the whole presented, and adopted or rejected by steam, the adjournment to next May was proposed and adopted."

Is it because the convention of Philadelphia has decided that typhus fever, cholera and yellow fever, like small pox, could be transported by foul vessels, diseased crews or passengers, that we must admit without inquiry such doctrines and all their consequences? This would be giving up a sacred right, and conceding to a few individuals a supreme power and omniscience.

As long as cholera was confined to the banks of the Ganges, it was believed contagious, and consequently transportable, but from the time when taking a western course, it jumped right over the *cordons sanitaires* that had been opposed to its passage, no one in Europe believed any more in its contagious nature. In the United States we do not believe in its contagiousness any more, and we do not suppose the resolutions of the Philadelphia convention will change public opinion with regard to it. What we have just said of cholera may in all points be applied to typhus fever. They are two distinct diseases which it was absurd to assimilate to small pox, as it regards their means of transportability.

The contagion of yellow fever, and consequently its transportation from one place to another by foul vessels, diseased crews and passengers, by porous substances, such as cotton, rags and so forth, was maintained by Chisholm, Blanc, Pym, Moreau de Jonnès, Bailly, Pariset, François, Audouard, Arejula, Palloni, and by Rush in the beginning of his life. It is known that after observing the disease during several epidemics the latter changed his opinion.

The contrary opinion, viz: that yellow fever originates in certain geographical limits; that it is not contagious and is not transmissible by foul vessels, diseased passengers, etc., was advocated with as much talent, and certainly with more appearance of reason, by Valentin, Devèze, Leblond, Ferguson, Jackson, Wilson, Gillkrest, Rochoux and Chervin. The last named author, more than any other, spared neither time, labor nor dangers to throw light on that question, and he is one of those who most contributed to bring about a positive result. That result was to incline decidedly medical opinion to the side of non-contagion. Chervin's opinion is that of the great majority of physicians who live in countries where yellow fever generally prevails. The report is mistaken in making a contrary assertion.

In the first and second propositions, viz: "There are certain diseases which may be introduced into a community by foul vessels and cargoes, and diseased crews and passengers. These diseases are small pox, and under certain circumstances typhus fever, cholera and

yellow fever;" the convention admits the contagiousness of yellow fever in all its extent and with all its consequences. In the third proposition, "when the latter diseases (yellow fever, etc.,) are introduced in this manner (foul vessels, cargoes, diseased crews and passengers,) their action is limited to individuals coming within their immediate influence, and cannot become epidemic unless there exist in the community the circumstances which are calculated to produce such diseases, independent of the importation." The convention, although denying for yellow fever the possibility of becoming epidemic, admits, nevertheless, its contagious nature! Strange contradiction which can only be explained by the hurry in which the convention closed its deliberations! And here, indeed, you declare yellow fever contagious like small pox; that it may be transported by diseased persons, by inanimate substances, and that its action is limited to persons coming immediately within its influence, and that, however, it cannot become epidemic, etc.! This is a contradiction. If a passenger having yellow fever may give it to persons coming under its immediate influence, these latter may certainly transmit it to other people coming in immediate contact with them, and so on. Now, let us suppose that one man having yellow fever gives it only to two individuals, and that the period of incubation of the disease be five days; let us see what will happen? At the end of 5 days, we would have 2 patients; of 10 days 4 patients; of 15 days 8 patients; of 20 days 16 patients; of 25 days 32 patients; of 30 days 64 patients; of 35 days 128 patients; of 40 days 256 patients; of 45 days 512; of 50 days 1,024; of 55 days 2,048; of 60 days 4,096; of 65 days 8,192; of 70 days 16,384; of 75 days 32,768; of 80 days 65,536; of 85 days 131,072; and, finally, at the end of 90 days, 262,144 patients. That is to say, in the space of three months, usually the time of our epidemic, we would have 262,144 patients, and you pretend that a disease which may be transmitted from one diseased individual to a healthy one, cannot become epidemic!

But will you say such a supposition cannot be made? I will answer in my turn: try the experiment with the itch or the small pox. Put in immediate contact and in the way mentioned above, 262,144 individuals liable to take the itch or small pox, and you will see if my proposition cannot be realized. The reason of it is, that itch and small pox are eminently contagious, whilst yellow fever is not, whatever you may say to the contrary.

I take special notice of the avowal contained in the third proposition, namely: "that yellow fever, however contagious it may be, cannot become epidemic, unless there exist in the community those circumstances which would give rise to the disease without importation."

Farther in the report we find: "4th. The circumstances alluded to, consist in vitiated states of the atmosphere, from local causes in connection with peculiar meteorological conditions." What are those vitiated states? what are those local causes? The convention here only repeated what has been asserted for more than a century without a shadow of proof. When I see in the subsequent articles, assertions which have been brought forward a hundred times, and as often refuted as impracticable utopias; when I find the following truth which really needs no demonstration: "14. This officer should be a regular physician, of unquestionable talents and experience, and possessed of great decision and rectitude of character, etc., etc.;" when I reflect that it is for such a miserable result that our City Council sent two physicians, and the Board of Health three of its members, at a cost of twenty-five hundred dollars, I involuntarily think of the young girl quoted by Doctor Benjamin Franklin, who had paid too dear for her whistle.

And when I read: "19. With a view to procure a uniformity in quarantine regulations throughout the several ports of the United States, the assembling of another and probably several conventions similar to the present one will be required," I can't help singing out: God save the taxpayers!

We will not follow the Board of Health in its review of the different opinions which have obtained currency, as to the method by which yellow fever is diffused as an epidemic. The way this awful disease originated in the West Indies and in some parts of Mexico, and of the United States, will ever be an unsolved mystery. The theories resting on telluric emanations, certain peculiar conditions of the atmosphere, or on the effluviæ arising from diseased or dead bodies infecting healthy persons, have been overthrown by facts, one after the other. The only positive thing we know about it is, that yellow fever makes its appearance only in summer and within certain geographical limits beyond which it very seldom extends. After all, what is that to us, whether yellow fever was first brought from the coast of Africa to the West Indies, and thence to New Orleans? It is of as little consequence to us to know whether that disease may be imported among us by foul vessels or cargoes, by diseased crews and passengers. Quarantine having been established chiefly to prevent the introduction of yellow fever in our community by vessels or persons coming from foreign ports, the main question for us is to determine if that disease does not originate spontaneously in our community, without the aid of importation, and therefore if quarantine is not a useless measure, vexatious, ruinous for the business of our city, and one which for public interest ought to be got rid of at once?

We will not go farther back than last summer, 1857, to find the requisite materials for the solution of that important question. On that point our opinion is different from that of the Board of Health. They think yellow fever was imported to us from abroad by persons or inanimate objects; we think the disease had a spontaneous origin among us. We will look at the facts on which rest the opinion of the Board, and then we will lay before you, those on which we base our convictions. You will have all the materials at your disposal, and will be able knowingly to decide whether quarantine is as good as the Board of Health proclaims it to be, or whether it be an evil, as we say it is.

The Board of Health recognizes at once the difficulty of the work. They say, page 21: "It will not fail to be noticed that the mortality from yellow fever runs up to a figure of 199, against 74 in 1856, and 2,670 in 1855. How this fever originated, whether from the operation of causes natural to our locality, or by its introduction from abroad, it is difficult positively to state."

"Upon this question there will still exist the same diversity of opinion now as there did previous to the establishment of the policy of protection by your quarantine regulations. It must be borne in mind, in forming a judgment upon this point, that where there are so many interests conspiring to defeat the purposes of the law and to induce its repeal, the truth will be carefully concealed, and active efforts made to baffle its discovery. Besides this, there is the inherent difficulty of ascertaining all the antecedent circumstances of the first cases, as residence, occupation, pursuits and habits; for, not unfrequently these cases come under the care of practitioners who are either indifferent to the important issue of its domestic or extraneous origin, or are biased by preconceived notions, and neglect to sift all the attendant circumstances of the case."

"From such concurrent reasons, it often happens that the only time when these facts could be certainly established, is permitted to pass, and with it all the hope of a reasonable and satisfactory solution of the difficulty. But whether introduced or not; whether of indigenous origin or otherwise, the main issue remains unaffected which regards the fever as competent to propagate itself in our latitude under the influence of propitious circumstances. The facts themselves, however originating, are too clear for dispute or denial. It is well known that nearly the whole mortality occurred within a comparatively narrow strip of the city, and occurred too, in houses contiguous to each other, and occupied by a class of persons whose intercourse is regulated by none of the conventionalities obtaining among those living under different social necessities."

The Board of Health says: "The difficulty of having positive infor-

mation on the first cases of yellow fever, comes from the fact that those cases fall in the hands of practitioners who are either indifferent to the important issue of its domestic or extraneous origin, etc." Shortly after its organization, the Board of Health addressed to all the members of the profession the following circular:

"To DR. * * * *Dear Sir*—As the Board of Health is very desirous of obtaining a full and authentic statement of all the incidents connected with the first cases of yellow fever occurring in our city during the current year, you will greatly oblige it, and through it the public, if you will inform yourself on the points involved in the annexed queries, and transmit the answers, in the first cases of this disease which may fall under your care.

"QUERY FIRST—How long has the patient resided in the city?

"QUERY SECOND—In what District and Ward is he living at the time of sickening?

"QUERY THIRD—What is his occupation, and does it require him to visit the shipping?

"QUERY FOURTH—If his occupation requires him to be much among the shipping, inquire in what vessels he had been before falling sick?

"QUERY FIFTH—Has he ever had yellow fever, and when?

"QUERY SIXTH—Has he been with or in the same house with one sick of yellow fever?

"The Board relies with much confidence on the public zeal and good will of the medical profession to aid it in solving, by a careful scrutiny into the above facts, the interesting and at present much vexed question of the indigenous or imported origin of yellow fever, and hopes that, to the extent of your opportunity and knowledge, you will sanction its efforts to ascertain all the facts involved in these inquiries.

"Yours, with respect,
"A. F. AXSON,
"*President Board of Health.*

"P. S.—The replies may be addressed to me through the Post office."

A little further we will see whether the physicians who had the first cases of yellow fever during the summers of 1856 and '57 lacked zeal and devotedness to public welfare. We will show the manner in which they answered to the call made upon them in the name of the community's good, and we will also show the way the Boarth of Health behaved towards them.

The Report says: "As soon as it was ascertained that cases of yellow fever had occurred, members of the Board immediately repaired to the places in which they happened, and carefully examined them. Having found no reeking offals, no vegetable and animal remains, no heap of putrefying matter, no overflowing privies, no excess of dampness under foot, in the quarter or rooms, or in the atmosphere, and no overcrowding;

in fact, having found said places exempt from all the causes believed by common opinion adequate to produce the fever, the Board came to the conclusion that something else is indispensable to constitute infectiousness in the true sense of the word, and that that something is the actual presence of the sick with yellow fever." Here then, we have the Board of Health contagionist in the extreme; facts must lend themselves to the explanation of its opinions; they forget how hard it will be for them directly to show that, in the absence of all the circumstances usually considered as favorable to the development of yellow fever, an individual actually sick, has been the starting point of the small epidemic of last summer. They are making suppositions, and they forget that at the beginning of the Report they have written those remarkable words: "The feebleness of human reason is betrayed when it essays to fathom the profound secrets of nature by hypothetical speculations."

We had yellow fever last summer without a doubt. The Board of Health stated its character, and reported 199 deaths from that disease.

Having been exempt from yellow fever the previous year; the city having enjoyed remarkable health, and Quarantine having been in operation for nearly two years, it became most important to trace yellow fever back to its first origin. By means of careful researches, they might have come to conclusions extremely important, and of great value for the elucidation of the vexed question of the plague which every year carries away part of our population.

Let us see what the Board of Health tell us with regard to this:

" From the evidence bearing on the origin of the fever, it will be seen how difficult it is to ascertain the truth. Without stopping to inquire into the accuracy of the opinion which announced a case of fever in June, July and August, it will suffice our purpose to commence our investigation of the facts, from the time when it began to prevail in so incontestable a manner as to preclude all dispute of its true character. This was not until the 20th September, the day on which the man Christian Miller died. This man, it must be remembered, lived up to the 12th September, at Mehle's, on Girod street, next door to A. Rose's clothing store. He was the first case which died in the Charity Hospital, throwing up black vomit. The second was Franco Savali, the Sicilian, who boarded and lodged at A. Rose's, who died on the day after, viz., the 21st of September. Now, if Mrs. Rose's testimony is to be relied on, that Franco had recently been in Havana, and only 11 days before his death in the city, is there anything improbable in the opinion that he, a well man at the time of his arrival in the city, may have brought the infection with him ?"

Let us see, now, the information that the Board of Health was able to get touching those two cases of yellow fever — the only two which seem to have deserved to attract their attention :

"APPENDIX E.—TESTIMONY.—*Early case of Yellow Fever.*—Christian Miller, on the testimony of his landlord, Mr. Mehle, has never spent a summer in the city before, although he has been residing in the State about two years; does not know of his ever having had yellow fever; has been employed at a saw-mill on the river, and near the city until about the 12th August, when he came to the city and took lodgings at Mehle's. From this time until the 11th September, he has been more or less at work as a day laborer on the levee, unloading steamboats and ships. Mehle does not know of his having been at work on any of the steamships. On the 11th September he shipped on an up-river steamboat as deck hand; fell sick on the 14th with fever, stopped at Napoleon, Arkansas, and returned to the city on the 17th, on the steamboat City of Memphis; was carried to the Charity Hospital, and died on the 20th September. Had black vomit.

"Franco Savarle, a Sicilian, living in, or adjacent to, the city for the last ten months, according to Mr. A. Rose's testimony, at whose house he was a lodger at the time of his falling sick; occupation was that of an oysterman on the Lake; had been living with Rose about one month before sickening; had just opened an oyster and fruit booth; was one of a company of six Sicilians associated in the selling of fruit and oysters: Does not think he visited the shipping; bought his fruit down town; believes he took the fever in the city; believes yellow fever indigenous to New Orleans. Franco was carried to Charity Hospital, died on the 21st September, throwing up black vomit. Mrs. Rose, wife of the above witness, on being visited by Capt. Ivy and another member of the Board, a week after her husband's testimony had been given, testified that Franco and his friends were Sicilians, that Franco had been in the city about eleven days before he was taken sick, that he had recently come from Havana, and that the company owned a small schooner, with which they were in the habit of oystering and bringing in their supplies; does not know if Franco or his comrades were in the habit of visiting the Havana steamers. Cross-questioned—she did not know the size of the schooner, that Mike (Mitchell Casuballo) commanded it and made Pass Christian his head quarters; that Larose, another comrade, lived in Girod street near the cemetery; did not know how recently Franco had been in Havana; knew him to have been there from his conversation; was certain that Franco had not been in the city more than twelve or fourteen days.

"Larose, to whom Mrs. Rose referred, was visited by the writer, (the President of the Board of Health,) and gave his testimony substantially as follows: Franco is one of six Sicilians, returned from gold hunting in Australia, about eight months since; returned by way of Liverpool, sailed direct from Liverpool here in a steamship. They have not been in Havana for six months; Franco worked at a saw mill up the river, caught the fever there! Cross-questioned—if Franco did not come here direct by steamship from Liverpool, did not know how he came; knows that neither he nor his comrades had been in Havana; did not come by New York, nor from thence via Havana; the schooner is a fishing smack; neither he nor Mike was in the habit of running into Havana

with the smack; all that Mrs. Rose and husband said was untrue, Franco took the fever at a mill up the river.

"Adam Straser died at the Charity Hospital on the 22d of September, was a boarder at Mehle's where Miller boarded. From Mehle it was learned that he had been about six months in the State, living at Franklin, was a harness maker, came to Mehle's on reaching the city, which was on the 12th September, was taken sick on the 17th, the day Miller returned to the city, died with black vomit on the 22d September.

"The respective residences of the landlords, whose houses furnished the earliest cases, are as follows: Mr. A. Rose keeps a clothing store on the corner of Girod and Tchoupitoulas streets. Next door on Girod is Mehle's dwelling. Next to him is Lyons, at whose house a man of the name of Carroll died on the 21st, believed to be of yellow fever, although his burial certificate was signed by a physician as having died of jaundice. His sister, Anne Carroll, died ten days before at the Charity Hospital, and her certificate was signed by one of the resident physicians as typhoid fever, on account of the doubtful character of her symptoms during life and the appearance of the cadaver after death.

"Immediately in the rear of these buildings (these words deserve all your attention) is a large warehouse in which there has been stored the whole summer large quantities of Rio coffee. This is the neighborhood visited by the Mayor and described 'as particulary clean.'"

The Board of Health, in commenting upon the above testimony, says: "Now, if Mrs. Rose's testimony is to be relied on, that Franco had recently been in Havana, and only eleven days before his death in the city, is there anything improbable in the opinion that he, a well man at the time of his arrival in the city, may have brought the infection with him? We ask the question, not that we have any doubt that the thing is impossible, for the medical records of yellow fever contain instances of infection under circumstances every way analogous to that hypothesised, but we ask it as one of the modes of solving this unknown question!"

Franco, then, is the starting point of the disease. We would be willing to admit it, did we not have at our disposal facts previously observed, very plain, very precise, and leaving nothing to be discussed. But, is it on Mrs. Rose's testimony that the conclusion is arrived at, that Franco went lately to Havana; that he died eleven days after his arrival in town; and that he brought with him the origin of the disease which spread to his neighbor, Christian Miller, who was lodging at Mehle's? When you cross-examine her, she does not know how long Franco has been back from Havana; she knows that he was there, because she heard him speak of it. She is certain that Franco had not been in town more than twelve or fourteen days. It was eleven days just now, it is now twelve or fourteen; it might be twenty days or a month were she examined to-day. In fact, Mr. Rose, husband of the preceding witness, says,

namely: that "Franco had been living with him (Rose) about one month before sickening. He thinks Franco caught his disease in town; he thinks, besides, that yellow fever is indigenous to New Orleans." Here comes, now, a third witness, Larose, one of Franco's comrades, and who tells you that they did not go to Havana for six months; that their boat is a fishing smack; that he and Mike are not in the habit of going to Havana; that the testimony of Mr. and Mrs. Rose is entirely false; that Franco was taken sick whilst working at a saw mill up the river.

The report says:

"As regards the weight to be attached to Mrs. R's testimony, we think her quite as credible as either her husband or Larose, and perhaps more so, inasmuch as her husband had, in a strictly technical sense, disqualified his value as a witness, by becoming a partisan to the doctrine that the fever was of domestic origin, in the avowal of his belief that such was the case; and far more credible than Larose, who in denying the truth of both the Rose's evidence, as well as contradicting his own statements, evinced his perfect worthlessness as a witness."

Every one will not be of the same opinion as the Board of Health. Mr. Rose may believe that yellow fever is indigenous to the city, and not lie when he says that Franco had been living at his house for a month when he was taken sick. Larose, who is one of Franco's comrades, who is with him, ought to know a little more about him than either Mr. or Mrs. Rose; he does not contradict himself anywhere in his testimony, and when he says that Mr. and Mrs. Rose's testimony is false, and that Franco was taken with yellow fever whilst he was working in a saw-mill up the river, we would be tempted to believe him, inasmuch as Christian Miller himself was taken sick with the same disease whilst up the river.

If it be true that Franco had arrived from Havana before the eleven, the twelve or fourteen days, or yet before the month that he spent in the city before being sick, he is the only one, or at least the first one who would have been able to introduce yellow fever in the midst of our community! But now we find out that he is not the first man attacked by the disease, nor the first dying from its effects. On the 11th of September, Christian Miller, in all appearance in good health, when his neighbor Franco also seemed to enjoy perfect health, shipped on an up-river steamboat as deck hand, fell sick on the 14th, with fever; stopped at Napoleon, Arkansas, and returned to the city on the 17th, on the steamboat City of Memphis; was carried to the Charity Hospital, and died on the 20th September, having had black vomit.

Miller could not have brought yellow fever from Havana, nor from any other foreign port. He had been lodging at Mehle's since the 12th of August, and had never gone on board any of the steamships. But,

says the Board of Health, Miller took the disease from Franco. Franco, when Miller left the city, had not yet felt the least symptom of the disease that was to kill him; he lived in the next house; did not have any direct communication with Miller, and still he communicates to him the germ of a disease, the existence of which he never suspected to be within himself. We might as well say that a man who is to have a chancre in eight or ten days, or who will have a strong fever, an intense cephalagia, with stiffness, etc., and an eruption of small pox in fifteen or twenty days, may communicate from to day syphilis or small pox to one of his neighbors. The argument is pressing; the Board of Health felt it and thought to destroy it by running to it, when they say, "This may explain well enough, on the assumption of the credibility of Mrs. Rose as a witness, Franco's illness and the infection of those who fell sick in the same house; but how does it bear upon the case of Miller, who died the day before Franco, and was taken ill while on his way up the river? To understand all the possibilities of such a case is beyond the reach of human testimony." Verily human testimony will never be able to explain things which have never existed, still less, things which could not have existed. Miller was taken sick and died before Franco, because it was not indispensable that he should take the disease from Franco who was not sick. We might say with as much reason that Franco was infected by Miller; Miller fell sick, because he was then in favorable condition to take yellow fever, as had been Joseph Linder, Dominique Maillot, and Gehergen, about whom we will speak directly.

But, are the cases of Franco and Miller really the two first cases of yellow fever observed last year? and is the Board of Health right in saying: "it is much to be regretted that no evidence, directly or indirectly, bearing on this subject has been within the knowledge of this Board?"

We read in the New Orleans Medical and Surgical Journal, of November, 1857, page 357, the following:

"Art. XI.—*Cases of Sporadic Yellow Fever.*

New Orleans, September 29th, 1857.

Dear Sir—Having been informed that you intend to write an article on the yellow fever of 1857, I take the liberty to send you the notes of a case that came under my treatment.

Respectfully your obedient servant and grateful former student,
Doctor James Jones. F. B. ALBERS, M. D.

"Joseph Linder, 22 years old, a Swiss; has resided in New Orleans four months; has been employed in an ice house all the time, situated near St. Mary's Market; he almost never left the house, and had not been near the shipping.

"He was attacked with a chill followed by high fever, and pain in the back and head, on Monday night, July 13. He went to the house of a

sister, No. 78 Julia street, where I first saw him on Wednesday, July 15th, having been sick for thirty-six hours. Cups had been freely applied to the back, head and epigastrium by his friends; the bowels had been opened by castor oil.

"His face was flushed; skin very hot; pulse 120; tongue hot to the touch and loaded with a white fur; great thirst, and a little nausea.

"Thursday, July 16th. Sleepless night; tongue red; face and eyes very red; skin intensely hot; very high fever.

"Friday. Bleeding from the gums; the other symptoms about the same; late in the evening threw up a quantity of dark colored fluid, resembling a strong decoction of coffee.

"He died very quietly a little before midnight.

"The treatment was as the case indicated.

"Autopsy eight hours after death, assisted by Drs. B. Dowler, A. C. Young, and J. P. Barbot.

"The body yellow, particularly the extremities; muscles very rigid; stomach contained about six ounces of a dark fluid; liver of a florid yellow color; gall-bladder enormously distended, with a yellowish colored fluid; the bladder nearly full of urine: lungs collapsed and healthy; heart somewhat enlarged, its walls darker than usual, and contained a quantity of dark coagula."

"From Mr. Lyous', shoe-maker, south-east side of Girod street, between Tchoupitoulas and Commeree streets, (next to Mr. Mehle's.)

"1st. James Carroll, Irish, aged 18; four years in the United States, and for the past year in New Orleans; has not left the city in that time. He died on Sunday, September 20th, in the fifth day of his illness. He was attacked the same day as Franco—15th September.

"This young man was not attended on by any physician; but on the day before his death was brought up to Dr. S. Woods', apothecary, on Tchoupitoulas near Julia, to be cured of what his friends called jaundice. Dr. Woods declined doing anything for him, alledging that he was too low.

"Carroll died the next day, and Dr. Woods, without making the proper inquiries, but satisfied with the friends' statements, delivered a certificate of 'death by jaundice.' He afterwards ascertained that the deceased had had no secretion of urine for four days before death; had had hæmorrhage from the gums and nose, and had vomited matter like coffee grounds."

And then, page 531, *New Orleans Medical News and Hospital Gazette*, Nov. 2, 1857, we read the following:

"CHARITY HOSPITAL REPORTS — YELLOW FEVER.— *First case at the Charity Hospital in* 1857.—Ann Carroll, an Irish girl, aged about 20 years, was admitted into ward 34 on Wednesday, September 9th," [six days before Franco was taken sick,] "with striking appearances of yellow fever in advanced stage. She was yellow; the face and arms covered with small red specks, which were either petechiæ or musquito bites; was somewhat delirious, with marked *subsultus tendinum* and hæm-

orrhage from the gums. *Previous History.*—This girl had lived in New Orleans about twelve months; lived in the Third District, or lower part of the city, until one month ago, when she moved up to the First District, and settled on Girod street, No. 27, between Commerce and Tchoupitoulas. There the family lived in a filthy nest of poor Irish until the time of her attack. The physician who attended her in the early stage of her attack, has given the following particulars: He was called to see her first on Monday, the 7th September"[—eight days before Franco was taken sick.] "She had then been sick several days." [This is worse yet for Franco's case.] "Found her with hot fever, pains in the head, back and limbs, and very costive; was told she had not had a stool for four or five days. He ordered a purgative and diaphoretic. The next morning he found her still very sick. Went again in the evening, and finding his directions were not properly attended to, he told her mother he could not return, and advised the removal of the girl to the Charity Hospital. He admits that he did not suspect the case to be yellow fever, yet the symptoms were such as are often met with in that disease. On Wednesday evening, the 9th September, she was conveyed to the Charity Hospital, and presented the appearances before described.

"September 10th—Saw her the first time; she was then delirious, hands very tremulous, blood oozing pretty freely from the gums, skin yellow, very restless. Could not hesitate to pronounce it a case of yellow fever. She was not vomiting, though the mother said she had thrown up some brown stuff the day previous. Called again late in the evening, and found her worse in all respects. She had retention of urine, but it was afterwards ascertained that a large quantity was drawn off by the catheter.

"September 11th—Visited her this morning, and found she had just expired, and without vomiting." [She died, as you may perceive, four days before Franco was taken sick.] "*Autopsy*—Two hours after death. *The Body* — Warm, plump, hardly so yellow as before death. *Liver*—Slightly yellow, not at all enlarged, nor engorged with blood, but rather dry when cut; not the least bronze color of the under surface, or any other part, such as was supposed by Drs. Stewardson, Swett and Frick, to be characteristic of bilious remittent fever. The gall-bladder contained a considerable amount of very black, grumous bile. No bile visible in the small ducts. *Stomach*—Contained about two ounces of reddish blood and mucus. The mucous membrane was highly engorged, and looked very red, even after a stream of water was allowed to run upon it several minutes. It was very much corrugated. *The Duodenum*—Was also engorged; its contents somewhat darker than that of the stomach. No stain of bile visible to the naked eye. *Small Intestines*—Mucous membrane for the most part highly engorged, and covered with thick, dark, bloody mucus; no bile stain. *Large Intestine*—Mucous membrane less injected; contents more fæculent. *Spleen*—Nearly double the normal size. *Kidneys* — Considerably engorged. *Bladder* — Contained about four ounces of urine; no other parts examined. *The Ticket was signed Typhoid Fever, Complicated with Jaundice.*—We have here an illustration of the difference of opinion in regard to the diagnosis of yel-

low fever that is witnessed on the first appearance of this disease *every year*. A few days after the death of this girl, her brother died with black vomit, in the same house, and her mother was attacked with yellow fever, but recovered."

However high may be the opinion we entertain of the young physician who signed the ticket, we cannot see here a case of typhoid fever. Taking in consideration the important fact that this young girl's brother died a few days afterwards, throwing up black vomit, and that their mother, in turn had yellow fever, without, however, succumbing to it, we feel disposed to adopt, and in fact we do adopt the opinion of the editor of the New Orleans Medical News and Hospital Gazette, who says he "could not hesitate to pronounce it a case of yellow fever."

Let us return to the New Orleans Medical and Surgical Journal, in which we read, page 360, line 9: "These cases (of John Lindner, by Dr. F. B. Albers; of Christian Miller and others, by Mr. J. P. Barbot;) are deemed important in relation to the question of the domestic or imported origin of yellow fever, being among the first in the season; Dr. Albers's case, which was witnessed by several physicians during the patient's life, and was confirmed by a post-mortem examination, is supposed to have been the first case thus unequivocally established, except one which occurred in the practice of Dr. Mercier, a memorandum of which the Doctor has kindly furnished, as follows: Dominique Maillot, Frenchman, aged 19 years, in New Orleans for the last two years without leaving the city, living in Seventh street, Fourth District, between Constance and Annunciation; goes to the old French Market, Second District, every morning; goes back home at 12 o'clock, M., every day; never went on board a ship nor near the shipping; very temperate habits; never was sick. Was taken sick on the 17th of June, 1857, presenting all the symptoms of yellow fever; was transferred to the Circus Street Hospital on the 18th, and died on the 26th, at half-past 6 o'clock, A. M. The progress of the case from first to last, with the exception of black vomit, was that of yellow fever. He had suppression of urine for thirty eight hours before death.

"Post-mortem examination the same day at 2 o'clock, P. M., eight hours after death. All the abdominal organs were in a high state of congestion. The black vomit fluid was found in the œsophagus, stomach, duodenum, and the upper part of the small intestine.

["Dr. Mercier, desirous that the President of the Board of Health should examine the case, both before and after death, sent invitations to that effect.—B. D."]

The editor of the Journal, Dr. Bennet Dowler, to the cases above cited, might have added the following:

"New Orleans, September 1st, 1857.

To Dr. A. F. Axson, President of the Board of Health.—*Sir:*—I was called to see a young man named Gehergan, on the evening of the 19th of last month, in Marigny street. He had been two days sick; he was 20 years old or thereabouts. His mother (a widow) had been for some years in New Orleans; himself, but six months, direct from Ireland. He was employed as hostler in the omnibus station in Marigny street. Being overheated, and whilst bathed in profuse sweat, he stripped and bathed in a tub of water in which he had put a large piece of ice. Shortly afterwards he was taken ill with the fever, which ended fatally on the seventh day. This is all I know of the case. His place of employment was near a mile from the levee. It was an indubitable case of yellow fever. There was wanting not a single sign or symptom which characterizes that fever, and suppression of urine and black vomit closed the case and confirmed my opinion.

"This young man had been nowhere where he could have contracted the disease by communication. I made no autopsy.

Respectfully,

Signed, J. H. Lewis."

The Board of Health says, page 25, line 9 of its report: "From the evidence bearing on the origin of yellow fever, it will be seen how difficult it is to ascertain the truth, *without stopping to inquire into the accuracy of the opinion which announced a case of fever in June, July and August;* it will suffice our purpose to commence our investigation of the facts, from the time when it began to prevail in so incontestible a manner as to preclude all dispute of its true character." *The vital question* being to determine if yellow fever *is of domestic origin*, or *is imported*, since the quarantine, in favor of which are the members of the Board of Health, if useful in the last case, would become absurd in the first; it was not at the time *when the disease had begun to prevail in so incontestible a manner as to preclude all dispute of its true character*, that the Board of Health ought to have begun their investigation. It was already too late when the Board set to work; the facts had already been observed and had left them behind. Hence we see to what insignificant result they arrive. They do not know exactly if yellow fever took its origin in the midst of bags of Rio coffee, piled up since the beginning of the year, in a warehouse adjoining the houses where lived Christian Miller and Franco Savali; or whether it was imported from Havana by that same Savali, or whether it did come to us with some passengers, trans-shipped in the

Bay of Mobile, from the steamer Quaker City, on board of one of the mail boats. And when it becomes necessary to decide, when they must, since in the opinion of the Board, Savali brought the germ of the disease from Havana, explain how Christian Miller was first taken sick the first one and died the first one, the Board only give that disheartening answer: "*To understand all the possibilities of such a case is beyond the reach of human testimony!*"

Instead of waiting until the 20th of September to get engaged in difficulties in which they are still entangled, without having even the consolation of having determined the origin of yellow fever, why did not the Board give some consideration to cases of disease that physicians, prompted by intentions most honorable and disinterested, had signalized to their attention? *Why should it not have stopped to inquire into the accuracy of the opinion which announced a case of fever in June, July and August?* Which were those cases? *In June, that of* DOMINIQUE MAILLOT; *that young Frenchman*, living in Seventh street, between Constance and Annunciation, *Fourth District*, who was taken sick on the 17th, and who died on the 26th, at the Circus street Hospital, attended to by myself. *In July*, that of JOSEPH LINDNER, living No. 78 Julia street, *First District*, taken sick on Monday night, July 13th, and died on the 16th of the same month, attended to by Dr. F. B. Albers; thirdly, *in August*, that of GEHERGAN, Irishman, 20 years old, living in Marigny street, *Third District*, taken sick on August the 19th, and died on the 26th, the seventh day after the breaking out of the disease, prescribed for by Dr. J. H. Lewis. The Board of Health might have seen *Dominique Maillot* before his death, and have been present at the post-mortem examination, as I had sent them invitations to that effect. Observations on the cases of *Lindner* and *Gehergen* had been sent to them by Drs. Albers and Lewis, with a precision of details, an honesty of intention and a disinterestedness which ought to have attracted all their attention. Why, then, did the Board of Health, established by your honorable body chiefly to investigate all the questions connected with the plague which periodically destroys part of our city population, and sometimes spreads over some of our parishes—why did they not, I say, give to those three cases, certainly the most important, anything more than a brief notice, uttered in the following disdainful words: "*without stopping to inquire into the accuracy of the opinion which announced a case in June, July and August?*" May it please the Board of Health, when men as learned, as respectable and as disinterested as Drs. F. B. Albers, Bennet Dowler, A. C. Young, J. P. Barbot and J. H. Lewis, will say that they observed a case of yellow fever, the whole community will not doubt their word.

The Board of Health do not believe in the domestic origin of yellow fever; they think the disease breaks out among us, only under the condition that it be imported from some port of Mexico, of the West Indies, or of South America; and they proclaim the possibility of protecting us against that disease by means of quarantine measures. The three cases of *Maillot*, of *Lindner* and of *Gehergen*, breaking out in three different parts of the city, far distant from one another, (Maillot in the Fourth District; Lindner in the First, and Gehergan in the Third,) and with individuals kept away, by the nature of their work, from the shipping, from steamships and other localities reputed capable of communicating the disease, furnish the most incontestible proof that *yellow fever has a domestic origin, and that to break out and spread in our community it needs not be imported from abroad.*

Hence, the conclusion we may strictly draw from these facts, established in a most positive manner, is that: THE QUARANTINE, A MEASURE COSTLY BY ITSELF, VEXATIOUS, AND ESSENTIALLY DETRIMENTAL TO THE COMMERCE OF NEW ORLEANS, IS, AT THE LEAST, USELESS.

It is not this year only that the Board of Health has not stopped to inquire into the accuracy of opinions which do not agree with their own. Already had that plan been adopted during the last year. Here is a proof of it:

"NEW ORLEANS, February 3d, 1858.

To Dr. MERCIER:—*My Dear Confrère:*—I did not keep a copy of the letter you speak of, but I am able to give you the substance of my answers to the queries of Dr. Axson, with regard to the first case of yellow fever I met with in 1856:

"Thomas Patrone, second mate of the ship 'I Tre Fratelli,' (The Three Brothers,) was taken sick on the 8th of August, 1856, during the evening. He had taken dinner at the lake with some friends, and had drank rather freely; thinking himself slightly unwell, he took a dose of salts on the morning of the 9th; and on the 10th, in the evening, feeling worse, had himself removed from on board the vessel, where he had remained, to the house of a friend, at the corner of St. Peter and Old Basin. I saw him for the first time on the morning of the 11th; he was very sick; the skin was already of a dark yellow tinge; the excitement and anxiety were very great; the urine was wanting, but there was hæmorrhage from the gums and the anus, frequent vomiting, brown on that day, quite black the next day. All the symptoms were there; it was next to impossible to be mistaken as to the nature of the disease; but, as I had not heard anything yet about yellow fever that year, and not wishing to be considered one of those alarming physicians who are

fond of discovering, the first ones, the existence of an epidemic, I had the patient examined by several physicians who have been practising here for a number of years, and all agreed at once that it was yellow fever. Thomas Patrone died on the morning of the 13th of August. He was strongly built, and must have been 35 or 36 years old. The ship 'I Tre Fratelli' came directly from Sener, and had not touched at any infected port. Everybody on board had enjoyed perfect health during the trip. The ship had been in port about twenty days when Patrone was taken sick.

"Patrone had visited no hospital; seen, accosted no person sick in New Orleans. The above is all I can recollect of the case. I told you in a private conversation, and I repeat it now, I was surprised last year when I saw the report of the Board of Health to the Legislature, and did not find any mention of my letter, whilst in the same report, were recorded *in extenso* letters from two physicians who had had two cases of fever originated in Tampico or Vera Cruz, or on vessels coming from those places, if my memory does not fail me.

"But, why had not Dr. Axson treated my letter in the same way? It would have been justice, even courtesy, after having asked for it with so much eagerness. Did my letter, my poor letter, frighten the Board of Health?

Yours respectfully,

[Signed.] NATILI."

When honorable physicians who seek the truth with as much eagerness as the Board of Health, and who, as much as it, take to heart all that concerns the good of the community, took the trouble of transmitting to it the first cases of yellow fever subjected to their observations with all the details capable of satisfying the most exacting, is it not strange that the Board of Health exclaims: "It is much to be regretted that *no evidence* directly and indisputably bearing on this subject has been within the knowledge of the Board?" What more was required in order that the observations of Dominique Maillot, Lindner and Gehergan should bear directly and indisputably on this subject? If Maillot had worked on board of the steamships that run to Vera Cruz, or to Havana; if Lindner had lately arrived from St. Thomas or Tampico; if Gehergan had had daily intercourse with persons lately arrived from Rio Janeiro, or Jamaica, would not the observation of *their* cases have been recorded *in extenso* in the Report of the Board of Health as had been, the year previous, those of the two individuals who had contracted their disease in Tampico, or Vera Cruz, and of which Dr. Natili speaks at the end of his letter?

It is, with the observations of the cases of Gehergan, Lindner and Maillot under its eyes, that the Board of Health did not hesitate to write: "It must be borne in mind, in forming a judgment upon this point, (origin, domestic, or imported of yellow fever,) that where there are so many interests conspiring to defeat the purposes of the law and to induce its repeal, the truth will be carefully concealed and active efforts made to baffle its discovery." Now, we ask it of you frankly and without any equivocation, who is it that has been "*trying carefully to conceal the truth? Who has made active efforts to baffle its discovery?*" Is it Dr. J. H. Lewis, whose letter to the President of the Board of Health, is full of that spirit of truthfulness and of loyalty which the whole community has always recognized in him? Is it Dr. Albers, who, in order not to leave any doubt as to the nature of the case, invited his honorable *confrères*, Drs. B. Dowler, A. C. Young and J. P. Barbot, to be present at the post mortem examination of the case? Lastly, is it ourself, who went personally and requested the President of the Board of Health to come and see our case before his death, and to be present at the post mortem examination? Or else, is it the Board of Health, which, instead of commenting and reporting on those three facts so important, to show up all their value, is satisfied with saying: "Without stopping to inquire into the accuracy of the opinion which announced a case of fever in June, July and August, it will suffice our purpose?" etc., etc.

You have the *facts* before you, and it is left to you to decide the question with impartiality. Our conscience does not make us dread your verdict. Whatever be your decision, the irrelevant way of acting on the part of the Board of Health will bear its fruits, and we can give it at once the assurance that, henceforth, not a member of the profession will take the trouble of laying before *them* facts, the knowledge of which, might contribute to the welfare of the commonwealth, entrusted to their administration in this behalf.

We think we have given you, gentlemen, irrefragable proofs that yellow fever is of domestic origin, and that it requires not the aid of importation to develop, propagate itself, and become epidemic amongst us; an opinion which is entertained with us by nearly all of the physicians of New Orleans. As a strict consequence of that principle, we have shown you that the establishment of a Quarantine is a thing at least useless; besides, that it is vexatious and ruinous to the trade of our city—a conviction which is entertained by nine hundred and ninety-nine of our merchants out of every thousand.

In order to arrive at that last result, we have been obliged to prove that the ideas of the Board of Health of Louisiana, relating to the na-

ture of yellow fever, its origin, and its mode of propagation, are erroneous, false, and founded neither in fact, nor on reason. We will turn the question on the other side; we will, with the Board of Health, admit that yellow fever is of foreign origin; that it cannot break out spontaneously in our city; that in order for it to develop itself and become epidemic, it must be imported from abroad from an infected place. We will also admit, for the sake of argument, that yellow fever may be imported by "foul vessels and cargoes, diseased crews and passengers;" that persons, apparantly in good health, but having the germ of the disease in their blood, may communicate it to persons perfectly healthy otherwise; and we will try to determine whether the Board of Health derived from the observance of the laws of Quarantine, any benefit for the community. We hope to prove that the Board of Health did not do anything useful; that it was, that it is, and that eternally it will be, impossible for it to accomplish anything advantageous for our city. On the other side, we hope to prove that the measures they have adopted and put in vigor have only been vexatious, ruinous for some business firms of our city, and that the tendency of these measures is to transfer to neighboring ports, the whole of our trade, for a part of the year, with Mexico, the West Indies and South America.

Leaving aside the cases of death from *cholera* and *yellow fever*, the very disorders which it is the aim and policy of the law on Quarantine to exclude, and which, by the way, it has not been the good luck of the Board of Health of Louisiana to exclude, as may be seen by referring to the mortality of the city of New Orleans for 1856 and 1857, as published by the Board, and you will see that the mortality for 1854–5 has not been larger than for 1856–7, after the mortuary records of the city had passed into the charge of the Board. It is to be observed that, in New Orleans at least, the first year following immediately after a large epidemic offers generally cases of yellow fever numerous and almost all fatal. The following year, that is, the second after an epidemic, offers sometimes, but rarely, cases of yellow fever, but still serious. Then come one, two, three and sometimes four, successive years during which our city enjoys a really remarkable state of health. Such was the fact after the dreadful epidemic of 1853. The years 1854 and '55 offered cases of yellow fever rather numerous, and of extreme gravity; 1856 and '57 were remarkably healthy, as far as yellow fever is concerned; 1858 may be so; but look out for 1859. The Board of Health, which is not the cause of yellow fever not visiting us for two years, will not be able then to prevent it from committing its ravages among us.

The diseases which the Board of Health pretends to keep away from

New Orleans are, YELLOW FEVER, FIRST OF ALL; then cholera, small-pox, typhus, typhoid fever, Chagres fever, bilious fever, remittent bilious fever, and JAUNDICE!! Do not think it a joke; I will give you directly the proofs furnished by the Board of Health. In that long list, we do not see the *pox*. Still, its right to be inserted in the list, is as incontestible as that of simple bilious fever, remittent bilious fever, or of JAUNDICE. Did the Board of Health understand that they would raise a smile?

Cholera, small-pox, typhus, typhoid fever, continuous bilious fever, remittent bilious fever, JAUNDICE, (for the sake of argument,) we suppose that New Orleans, at a given time, is entirely free of all those diseases) being able to reign, and in fact, reigning constantly in New York, Philadelphia, Boston, Baltimore, Cincinnati, Louisville, St. Louis, Memphis, —some of them, continuous bilious fever and remittent bilious fever, for instance, existing in preference in the States surrounding Louisiana, and in some parishes of that State, in order to protect New Orleans against their invasion, it would become necessary to establish Quarantine at the Opelousas Railroad, on the river, at the Jackson Railroad, at the Rigolets, at Bayou St. John, at the Pontchartrain Railroad, at the lake end of the New Canal, on the river above and below—in fact, on the ways that put the city in communication with the parishes and the neighboring States.

When, then, all those diseases may be introduced in the city of New Orleans through *eleven* different ways, to establish quarantine on one or two of these ways only, is a lack of common sense not less than of logic. It is an act of folly.

Yellow fever being the only disease that does not reign *regularly* in the States north of Louisiana, and consequently, is not apt, *under ordinary circumstances*, to be imported in our city by the river, or the Jackson Railroad, or the Opelousas Railroad, did the Board of Health, through the quarantine established on the river, protect us against it? Could it have protected us?

To the first question, the Board of Health answers itself, NO! Its report, presented to your honorable body, in January, 1857, declares 74 deaths from yellow fever during the summer of 1856. The one we have at present before us, carries the number of deaths from the same disease during the summer of 1857 up to 199. In the opinion of the Board of Health, yellow fever, being of foreign origin, and unable to develop itself spontaneously among us, must necessarily have been imported from abroad in 1856 and 1857. Therefore the quarantine was unable to protect us against it, during those two years at any rate.

But in future will its inability be the same? Will it protect us any better than it has done so far? The Board of Health explained to you the impossibility of its applying the quarantine regulations in all their rigor, on account of the want of warehouses and wharves. Now, it asks a favor of you, viz: a new appropriation without which they could not use the $50,000 from the Federal Government for the construction of warehouses and wharves. Having in its possession every practicable facility to the thorough fumigation of vessels and cargoes, by discharging and landing the same, whenever this becomes imperative, either through the foulness of the one, or the susceptible character of the other, the Board of Health gives you the assurance that henceforth our city will have nothing more to fear from the diseases against which the quarantine is intended to protect us. In our turn, we tell you that you have already spent too much money for the benefit of that useless quarantine. We advise you not to vote any more, the least appropriation in favor of an institution which, so far, has been injurious and will never do any good.

Let us show, now, the impossibility of adopting quarantine regulations capable of protecting New Orleans against yellow fever: By pretending that yellow fever was imported here, last summer by Franco Savali, who had arrived from Havana, according to Mrs. Rose's testimony, eleven days previous to his feeling the first attack of his disease, the Board of Health concedes that the period of incubation of yellow fever may be of thirteen days, (a fact long ago acquired to science,) since to the eleven days of residence at Mrs. Rose's we must add at least two days to come from Havana to New Orleans. The strict consequence of this fact is, that an individual ought not to be allowed to come to town before thirteen days after his leaving a place where yellow fever reigned. The measure might be enforced with persons coming here by the river; but how are we to put it in execution with people coming from Mobile? An individual may leave Havana, go to Mobile, stay there nine days, and reach New Orleans before the breaking out on him of yellow fever, the germ of which he had contracted before his departure from the Island of Cuba. Instead of going to Mobile, that same gentleman might have had time to go to Charleston, take the mail and reach here before the end of the thirteenth day.

At his arrival at the Rigolets, the doctor of the station will ask him where he comes from? and when he will answer, from Charleston, Augusta, or from any other part of South Carolina, or of Georgia, in virtue of what law may that individual be put under Quarantine? And how many, who are in a hurry to come from Havana to New Orleans, will

have recourse to that means, to escape your so-called sanitary measures? And then, when there will be yellow fever in Mobile, in Pensacola, Tampa Bay, St. Marc, Key West, etc., how are you going to protect New Orleans against the invasion of the disease without interrupting *entirely* all communications with these ports? How is communication with all those ports to be interrupted? Every mail boat ought then to be detained at least ten days at the station of the Rigolets, in order to submit to a thorough fumigation its cargo, its crew and passengers. All the schooners running between New Orleans and Mobile, Pensacola and other ports along the coast of Florida, must be subjected to measures just as strict. Allowing even that such a strict Quarantine could be established at the Rigolets station, what would prevent individuals coming from Havana, from going to Mobile, thence to Jackson, and coming here through the Great Northern Railroad in less than thirteen days, that is, before the disease they would have contracted in Havana, had time to manifest itself by any exterior signs. It is plain to all unprejudiced minds, that any one coming from Havana, Mobile, Pensacola, Key West, and other ports in the Gulf of Mexico, when yellow fever is prevailing in those different places, may easily escape the Quarantine regulations and in that way render them perfectly useless.

We have said that it would be necessary to detain at Quarantine not only the crews and passengers of the mail boats and schooners passing through the Rigolets in order to be useful, but also to submit their cargo to a thorough fumigation. The Board of Health gave us to understand that the cases of Miller and Savali might very well have been caused by bags of Rio coffee from a warehouse in the neighborhood. It adopts the opinion that the disease may be conveyed from one place to another by the luggage and the clothes of crews and passengers, by rags, by spongy substances, etc. We receive every day from our sister city, passengers' luggage, bales of cotton; these, then, must be subjected to a thorough fumigation if we do not want to run the risk of seeing break out in our midst the germ of the disease they carry with them. Who ever could seriously think of enforcing such strict regulations, when it is so easy to escape them? We need not argue any longer to prove that it is absolutely impossible to enforce the Quarantine regulations with all the necessary rigor to render them useful, admitting that they are useful at all.

With regard to that point, we have been astonished to see the way the Board of Health acted towards Dr. Joubert. How could the Board, which thinks that yellow fever may be conveyed by the clothes and baggage of passengers, how could it, I say, have allowed this hon-

orable physician to come back to the city without having previously submitted his clothes and himself to a thorough fumigation? That physician, after spending a long time attending generously to the officers and crew of the French steamship of war *Le Tonnère*, (a prey to the most destructive epidemic of yellow fever that we ever heard of,) came back to town, with all his clothes saturated with yellow fever. The Board of Health did not force him to bathe in vinegar, and to submit his baggage and his clothes to a thorough fumigation; nor have we heard at any time of Dr. JOUBERT transmitting yellow fever to any one in his practice.

Besides, at the arrival of the steamships from Havana, Vera Cruz and Greytown, at the quarantine station, did not the captains of those steamships come up to town in order to try and get their veesels relieved from the quarantine, which was weighing on their vessels? Every one understands the losses that many of our merchants have necessarily sustained in consequence of having their vessels detained at quarantine. (We only speak of that on the river.) That detention, besides, was made in a vexatious and onerous manner. From the 10th of December, 1856, to the 8th of November, 1857, sixty-two vessels, most of them once, others two and three times, had to submit to quarantine, and the quarantine fees reached as high as $33,620, besides $20 collected in town from the bark Ninevah, and $20 from steamship Granada, making a total of $33,660! We would like to know if at any other place, under any other circumstances, such a heavy tax has ever been levied?

The quarantine was enforced in a vexatious manner, did we say? It is sufficient to open the report of the Board of Health, pages 50 and 51. We see there, that not only from the 26th of June to the 13th of July and 19th of August, during which time were observed the cases of yellow fever of Dominique Maillot, Lindner and Gehergen, but also from 15th of September to the 12th of November, when the Board of Health had acknowledged that yellow fever was in town, and that a good number of cases were within the walls of the Charity Hospital, they submitted to all the severity of quarantine vessels, out of which some had had or still had yellow fever on board, but the greater number of which merely came from ports where this same disease existed at the time of their sailing. Some of these vessels had been out at sea two months, and did not have any sick persons on board; that consideration was not looked upon as sufficient to free them from the vexation of quarantine. Moreover, other vessels were detained under pretexts still lighter: some on account of *bilious fever*, others on account of *remittent bilious fever;* two diseases that are met with in the city of New Orleans and in the surrounding parishes at all seasons of the year, and in as large numbers

as anywhere else in the world. Some were detained on account of Chagres fever, a disease which never broke out spontaneously among us, notwithstanding the numerous cases coming here, every week, in the first times of the annexation of California. Some, finally, were detained on account of JAUNDICE!

Because a passenger, or some one of the crew of a vessel, in consequence of a fright, of a cold, or of any other reason, will have seen his face become yellow, that vessel will have to remain at quarantine during the hottest time of summer, and when yellow fever is already raging in New Orleans!

We think we have given proof that yellow fever, cholera, typhoid fever and some other diseases, against the introduction of which in our community, quarantine was established, originate spontaneously here, without the help of importation, spread here, and sometimes take the character of *epidemics*. We came to the natural conclusion that quarantine cannot protect us against those diseases; and, therefore, becomes a useless measure. We have proved, besides, that quarantine has been exercised in an injudicious and vexatious manner; that it is a nuisance to our business; that it was the cause of severe losses to many of our merchants; and, finally, that its result, in times not far off, will be to take away from our port and direct to other ports, all our trade with Mexico, Cuba, the West Indies and South America.

If my reasoning has not been strong enough to carry conviction to your mind, let the two Houses, gentlemen, appoint a special committee to investigate the matter. I may give you already the assurance that, out of twenty physicians practising in New Orleans, your committee will find nineteen who endorse my medical doctrines about the non-contagiousness of yellow fever, and the uselessness of quarantine. I may also assure you that your committee will bring you back the ardent wishes of all our merchants for the repeal of a law that a former Legislature thought bound to enact, in order to bring back confidence to a frightened population, still under the effects of the disasters of the epidemic of 1853.

MERCIER, D. M. P.

NEW ORLEANS, February 16, 1858.

P. S.—The following letters, which have been sent to us this morning, bear too much upon the subject to be omitted. We give them as they have come to us, without commentary.

February 19th, 1858. M.

NEW ORLEANS, February 19th, 1858.

I occupy part of the house, southeast corner of Tchoupitoulas and

Girod streets, belonging to Mrs. A. Rose, and from which the Sicilian, Franco Savali, was taken to the Charity Hospital.

I knew Franco Savali, saw him in his shop on the 4th September last, when I returned from the North, and every day afterwards until he was taken sick. My wife has seen him almost daily (as we occupied the same house,) for two weeks before my return from the North.

[Signed,] ROBERT COBURN.

I never told any one that Franco Savali had recently come from Havana, and had been in the city but eleven days before he was taken sick; if any one understood me to have said so, he or they must have misunderstood me, as I speak English very imperfectly. I have seen Mr. Braun's statement and my husband's, about Franco, of February 19th, 1858, and agree with them in all respects.

[Signed,] JEANNETTE ROSE.

New Orleans, February 19, 1858.

The undersigned, residing respectively at the southeast and southwest corners of Tchoupitoulas and Girod streets, do hereby declare that the report of the Board of Health of the case of the Sicilian Savali, is inaccurate, and subjoin the following facts of our own positive knowledge, giving his history for more than two months before his illness and death.

Franco Savali rented from A. Rose, a room for the purpose of selling fruit and oysters therein, in the end of July, 1857; he began putting up the fixtures himself, in the beginning of August, and attended to his business, and we saw him daily in the city until he was taken ill and died. [Signed,] A. ROSE,

Southeast corner Tchoupitoulas street.

JOHN BRAUN,

Southwest corner Tchoupitoulas street.

New Orleans, February 19, 1858.

ART. XV.—*Journal de Physiologie.*

THE eminent Physiologist Dr. E. BROWN-SÉQUARD, has projected, and before this number of the New Orleans Medical and Surgical Journal reaches the reader will have published the initial quarterly number of the *Journal de Physiologie de l'Homme et des Animaux*, at Paris, assisted in the editorial department by DRS. CH. ROBIN, CH. ROUGET and THOLOZAN,

together with collaborators, both French and foreign, European and American. Under such happy auspices, and at a period characterized, as is the present, by new, important and progressive experimental researches, results, and generalizations in physiology, the guiding star to practical medicine, this Journal will doubtlessly receive and reflect a steady and increasing light over the vast expansions of two continents—everywhere among the faithful *in Æsculapio*, whose mission is to prevent disease, to heal where prevention fails, and to alleviate whatsoever is incurable. The victors in war, the dispensers of power, the rulers of cabinets, the explorers of the sidereal heavens above, and the discoverers in the earth beneath, stand at a lower level, on the platform of humanity, than the skilful physician who relives the afflicted, and restores health.

Ye vigorous, hopeful and progressive experimentalists, whilst respecting the land-marks of the past, adventure, Columbus-like on the sea of innovation and discovery; and if baffled and driven back, you will have at least dissipated the *ennui* of compilation and the *vis inertia* of authority which brood over the stagnant waters of life. Offer on the high altar of experimental physiology, hecatombs of animals; invoke the true light wherewith you may read the living characters of organic science. "Let there be light."*

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*As many readers of the New Orleans Medical and Surgical Journal understand the French language, the subjoined exposition of the plan of the *Journal de Physiologie* may be acceptable and the more so because there is no similar Journal in France, nor, perhaps in any country, Germany excepted.

Ce journal aura pour objet, en outre de la physiologie pure:

1. La *chimie organique*, *l'hygiène*, la *toxicologie* et la *médecine légale* dans leurs rapports avec la physiologie.

2. *L'anatomie descriptive*, *l'anatomie comparée*, la *tératologie* et *l'histologie normale et pathologique*, en tant qu'elles éclairent la physiologie

3. Les applications de la physiologie à la *pratique de la médecine de la chirurgie et de l'obstétrique*.

Ce journal paraîtra quatre fois par an: le 1er janvier, le 1er avril, le 1er juillet et le 1er octobre.—Chaque numéro sera de 10 à 12 feuilles (160 à 200 pages), avec planches gravées et des figures intercalées dans le texte.

Les matières seront classées de la manière suivante:

1. Travaux originaux, formant plus de la moitié de chaque numéro.
2. Mémoires publiés à l'étranger, en entier ou par extrait.
3. Analyse raisonnée et critique des livres publiés en France et à l'étranger.
4. Appréciation des progrès de la physiologie en France et à l'étranger.

Les deux ou trois premiers numéros contiendront une historie détaillée des progrès de la physiologie, pendant les années 1855 et 1856.

Le Prix de l'Abonnement, *Payable d'Advance*, *est.*—Pour Paris, 18 francs; pour les départments, 29 francs; pour l'étranger, 25 francs.

On s'abonne, a Paris, Chez MM. J. B. Baillière et Fils, rue Hautefeuille, 49. Agent in the United States, F. Benington & Son, Booksellers, Philadelphia, Pa.

Les auteurs français qui voudraient avoir leurs ouvrages annoncés et analysés devront en faire déposer deux exemplaires chez MM. J. B. Baillière et Fils.

Les auteurs étrangers qui voudront avoir leurs ouvrages analysés devront en faire parvenir un exemplaire, *franc du port*, à MM. J. B. Baillière et Fils, ou à M. le docteur Brown-Séquard, rue du Dragon, 16, à Paris

If it be true, as La Place affirmed, that the progress of astronomy has been a constant triumph of philosophy over the illusions of the senses, it is no less true that the progress of physiology has been a constant triumph over the illusions of authority, and the prejudices against innovation. To say that Medicine is progressive, is to utter a truism. In the hands of the ancient Hebrew priests, it consisted in a few hygienic measures, as ablutions, and local quarantine and seclusion against well known contagions. The Egyptians introduced neither quinine nor anæsthetics, but a few cathartics and emetics. The Greeks added much to the preëxisting data, which, at a later æra, began under the great master Hippocrates to assume the form and coherency of a science; yet twenty centuries elapsed before a Haller or a Harvey, a Jenner or a Rush, a Bichât or a Brown-Séquard, and other brilliant orbs constellated the expansions of the Æsculapian heavens.

What Coleridge says of the mental applies to the medical status of the European world: "Germany—Past and Future; England—Past and Present; France—the Present." He forgot America. Let the *Journal de Physiologie* avoid that amusing weakness so prevalent in the other hemisphere.

French medical literature almost entirely ignores that of America, while the latter recognizes the progress of the former in the original language, or by means of translations. Admitting that in equal numbers of medical men the completeness of elementary education of the French exceeds on an average that among Americans, the latter compare favorably with the former as authors and thinkers, while they excel in "rendering to Cæsar the things that are Cæsar's," than which nothing is more praiseworthy in the Republic of Letters. The very Frenchmen, particularly Parisians, whom the Americans laud most, speak the least favorably of American Medicine, than which nothing can be more unamiable, not to say unjust.

B. DOWLER

SCIENTIFIC CORRESPONDENCE.

ART. I.—*Letter from* E. BROWN-SÉQUARD, M. D.:

Professor of Physiology; Laureate of the *Académie des Sciences* of France, Vice President of the *Société de Biologie* of Paris; Member of the American Philosophical Society, etc., etc.

PARIS, December 12th, 1857.

MY DEAR SIR—I take the liberty to say, that you are greatly mistaken if you believe that your name and your discoveries are known in Europe; and that you must accuse yourself of the ignorance of the *learned* (so called) men here, concerning what you have done. This is not because you live at one of the parts of the circumference of the enlightened circle of this world that you are unknown. It is because you have not employed the proper means to be known. You have sent your printed papers to the *Académie des Sciences*, or the *Académie de Médecine* or to *illustrious* men. If you needed a tomb for your papers you could not do anything better than to send them to such *unreading* men, or *locked up* libraries. The only chance you may have had to have your papers read by some active, young and intelligent man is that the illustrious men may have sold them at two cents a pound, in *physical weight* to a grocer, and that, in buying coffee or sugar, an intelligent young man may have found your papers and secured them at four or five cents a pound. But this is a very narrow chance, and there is no wonder that nobody knows you here.

Why did you not send your papers to the men who have time to read —to the fifty young physiologists of France and Germany? Don't you know their names? Don't you know that they write more and lecture more than the illustrious men?

I have been fifteen years engaged in physiological researches. If you are *au courrant* of what is done in the centre of the circle I spoke of a moment ago, you must know that I was engaged more than eleven years ago in experiments on subjects on which you yourself have been at work, and you have never sent me, or at least I have never received from you, one single bit of your printed papers; and here, among the most active young men, I do not know one who is not like me. I may name Rouget, Colin, Verneuil, Vulpian, Broca, Bouley, Gobaux, Faivre, Grubler, Davaine, etc. I might fill the page with names.

There are active young societies in Paris, and, above all, the *Société de Biologie*, where your name has never been heard, except pronounced by me. Have you sent a single paper to this society? Your name is not on the catalogue of their library. Accuse yourself, therefore, if you shine only in the United States, and even there in *our common great country*,* I have vainly tried to procure most of your publications. I have succeeded in procuring only three of your papers, and they are not the most important.

Now, my dear sir, I want you to do what would be the most useful to your reputation, and, therefore, I hope you will not blame me for this letter. Believe me, dear sir,

Respectfully yours,

E. BROWN-SÉQUARD,

16 *Rue du Dragon, Paris.*

DR. BENNET DOWLER, *New Orleans.*

[REMARKS.—This letter, from one of the most illustrious of living physiologists, is not as it might seem to the reader, an answer to one from myself, (for I had not written to its author,) but is the spontaneous suggestion of his own kindness, and, how little flattering soever it may be to myself, it will not be devoid of scientific interest, should any one now or hereafter, think it worth while to study the nature, extent and the import of my researches, particularly "in the United States, wherein," Dr. Brown-Séquard says, yet quite erroneously, that "I *shine*." To be useful, not to shine, has been my fixed purpose, and while I am duly thankful for the commendations and friendly testimonials which I have received, I have, nevertheless, little cause to flatter myself with either friendly or adverse criticism, in regard to the recognition of the fundamental principles or experimental results of my researches, which began in 1841—18 years ago. I published in the *Western Journal of Medicine and Surgery*, several papers on CONTRACTILITY, CAPILLARY CIRCULATION, ANIMAL HEAT, etc., commencing with April, 1843. My fugitive papers on these and kindred subjects would make two or three octavo volumes of considerable size; my unpublished far more. I will not now affirm that these papers are valuable or otherwise; but I may affirm that one who cultivates science in a disinterested manner, with neither aid nor encouragement, does his duty, and need give himself no further concern in the matter, after having published his experiments and deductions. All the blame, if any, lies with those who refuse to recognize, accept, and profit by his labors — who refuse either to verify or disprove his experiments

* M. E. Brown-Séquard is a native citizen of the United States.

and deductions, and who prefer the speculations of the hour to experimental investigations and original researches as guides to faith and practice. An experimenter who voluntarily imposes labors upon himself has no right to complain. Goëthe declares that a complaining devil is the most contemptible of beings.

The naked chief of an African tribe who asked a French prisoner brought before him, "Do the people in France talk much about me?" was a vain negro. I have not fallen into this error. Dr. Brown-Séquard does not, therefore, surprise me in the least by informing me that my humble researches are unknown in Paris; but he does surprise me in supposing that this matter is much better for my labors in the United States, excepting some vague commendations which are often bestowed on sciolists. Original researches which do not coïncide with the reigning theories of the day, have but a small chance of recognition so long as mere compilers make the books put into the student's hands. The compiler is too apt to wait for a general approval before accepting anything new, even though true, which might greatly derange and endanger the building of a systematic work.

It is not for a moment to be suppposed that any individual is of sufficient importance to be an object either of unjust treatment or of universal attention, of envy or of eulogy and approbation on the part of the entire profession. The moral excellence of the enlightened medical mind on the whole, favors the speedy recognition and reward of merit in the individual, yet the history of the past shows not a few exceptions. Many theories have been inaugurated, have reigned, have been deposed and almost forgotten during a single generation. If the leader of the flock, in jumping a fence, should prostrate it, the whole flock of sheep will nevertheless jump just as high as their leader. It is easier to drive a hundred after the leader, than one without his guidance.

The facts rather than the theories of the past, remain as the great land-marks of thought, being immortal, though they may have been erroneously interpreted. Academies, courts and celebrated authorities may inaugurate a fundamental idea, which otherwise might have long remained in obscurity. Although a rational belief is based on evidence alone, just as a rational love is based only on moral excellence; yet there are a thousand contradictory beliefs, and daily experience shows that mutual admirations are common in the absence of moral worth. Physiological belief seems to be occasionally the result of destiny, seeing that some authors who have adventured on the dark sea of speculation and innovation, particularly in neurology, have by unwarranted assumptions based on a fanciful anatomy, obtained without hesitation the sanction of

the profession, having been called great discoverers, although their language is barbarous, their theories incomprehensible, their anatomy undiscovered; on the other hand, Harvey's real discovery, "it is well known, was not received by any physician who was more than forty years old." Hence the credit or discredit attending the reception or rejection of a discovery cannot be regarded as a very serious matter.

In the case of Dr. Brown-Séquard, academies, journalists, and compilers vie with each other in their endorsements and praises of his distinguished and meritorious physiological labors.

BENNET DOWLER.]

ART. II.—*Letter from* WM. B. ATKINSON, M. D., *of Philadelphia.*

PHILADELPHIA, January, 1858.

MESSRS. EDITORS:—For some months medicine and medical men have been at a discount. Our city has been enjoying an unusual state of health. Until within a very few weeks, we have had no disease of a contagious or epidemic nature in our midst. To be sure, we have had thirty and even forty deaths, weekly, of consumption; but our good citizens scarcely think a second time about that; nor do our fair daughters deem it necessary, in view of such a warning, to pay any more attention to their dress, the covering of their feet, or even to be more careful, when heated from dancing, in exposing themselves to the chill night air.

At present we have a certain amount of scarlatina; which, in some families, has made sad havoc, though we have reports of it in a mild form in many portions of the city. In several other parts of our State, it is raging to a fearful extent. With this exception, our mortality reports, for this fall and winter, have presented a gratifying appearance to all who feel interested in the health of the people.

As a medical metropolis, we keep well up to former years. The classes at our various colleges are quite as large as usual; and, I apprehend from various data, that the graduating classes will not fall short of last year. Within the last few years, the corps of teachers has been much strengthened by numerous additions, in the shape of special lecturers. We have now special classes, for the study of pathological anatomy, well prepared, by means of microscopes of the highest powers, to dive deep into the mysteries of morbid structure. Then, we have

classes in operative surgery, where each student is enabled to perform on the subject—each operation under the eye of the lecturer; next comes obstetrics, where the learner can manipulate with manikins, etc.; and a host of lecturers on diseases of the lungs, the heart, etc., etc. To the student, who is at all desirous of studying, no want of opportunity presents, and every available moment can be well employed, in the preparation for his future life of usefulness.

At the last conversational meeting of our Medical Association, the subject of rheumatism, more especially the acute form, was well and learnedly discussed by a number of the members. Dr. R. K. Smith read an interesting paper, consisting of a series of interrogatories, relating to points, some of which were the cause of much difference in opinion among medical writers and others. As to whether the cause is a blood poison, or one of the nervous system? Is it urea? What tissues are invaded? What remedies are most serviceable? Value of the lemon juice? Merits of alkaline; or, the calomel and opium treatment? Action, and value of colchicum? Use of local applications?

Dr. S., in commenting upon his questions, took the ground of it being a blood poison. That urea, was one poison; that affections of the heart, resulting from this disease, eventually terminate in death. The tissues involved, were the fibrous and sero-fibrous. That remedies relieved, by eliminating the poisons from the blood. That bleeding was, unquestionably, an important remedy. That lemon juice had not been productive, in his hands, of the benefits ascribed to it by others. That the alkaline treatment, being just now the popular plan, was not yet fully decided upon. That calomel and opium were found by every one, of great benefit. That colchicum was likely to disappoint in many cases. That local applications, and especially of cold water, were invaluable.

Drs. Remmington, Bell, Nebuiger, Emerson, H. Hartshorne and *Turnbull,* participated in the debate which ensued. Cold water seemed to be regarded by the majority as a dangerous remedy. They seemed to depend on blood-letting, cups and leeches, calomel and opium, colchicum and the alkalies—as iodide of potassium, liq. potassæ carbonatis, etc. Lemon juice did not meet with much favor. The secondary affections—as endocarditis and pericarditis—were not looked upon by the majority as so dangerous as Dr. Smith seemed to think; and were thought capable of being permanently cured.

I much regret being compelled to give a synopsis of this interesting discussion, as much interest attaches to the remarks of each individual.

Letter from Prof. J. C. NOTT, M. D., *announcing his resignation of the Chair of Anatomy in the University of Louisiana.*

NEW ORLEANS, February 2, 1858.

DR. BENNET DOWLER:—*My Dear Sir:*—I have given notice to the Faculty of my intention to resign the Professorship of Anatomy in the University of Louisiana, at the close of the present course of lectures. I will return to Mobile the latter part of March to resume the practice of my profession.

I take this step, not without regret, as the position here is one much to my taste; but my friends and relations in Mobile have made such appeals to me, and given such evidence of attachment and confidence, that I feel it would be unwise and unfeeling, at my time of life, to cast aside such friendships as can never be formed again.

I shall always feel a deep interest in the University, as well as your journal, and shall not cease to use my exertions to secure the prosperity of both.

Very respectfully and truly yours,

J. C. NOTT.

[As a scholar, naturalist, physician, surgeon and lecturer, Prof. Nott stands among the foremost. His retirement after a brief but brilliant career in New Orleans, must cause no small regret to his professional colleagues, to his medical class, to the public, and particularly to his personal acquaintances, who know the goodness of his heart, and his disinterested devotion to science. The first love, the old attachments and associations, (and possibly some sad as well as pleasant *souvenirs*—grave-stones as well as hearth-stones—) seem to have preponderated over the fame and emoluments of a professorship in New Orleans. 'Tis the same feeling which Goldsmith's Traveler has expressed:

> "Where'er I roam, whatever realms to see,
> My heart untravel'd fondly turns to thee;
> And drags at each remove a lengthening chain."

"Sweet home" and the mobility of Mobile have *Not(t)* after all a Medical Journal. The New Orleans Medical and Surgical Journal will, therefore have the advantages of the contributions of the ex-Professor's pen, which will be some compensation.—B. D.]

[The following series of letters, selected from many, which neither my leisure nor my data will permit me to answer, either in print or by mail, is published under the expectation that some of the readers of this Journal will, through its columns or otherwise, contribute such information as may aid the able gentlemen who have written these letters; and thereby, promote the common good of the profession, and the public. B. D.]

Art. IV.—*Letter from* A. K. Gardner, A. M., M. D., of New York, *on Fibrous Tumors of the Uterus, in the Black Race.*

Bennet Dowler, Esq., M. D: *Dear Sir:* At a recent meeting of the New York Pathological Society, I stated that, in my experience, fibrous tumors of the uterus were found most commonly in the black female. Since then, others have not only corroborated this opinion, but have even gone so far as to state that fibrous tumors exist in the uterus of almost every black female; and, that without making any limitation of age. My object in writing, is to obtain a little information from your extensive experience, whether it is equally noticeable with you, or, whether it may be ascribed to the degeneracy of the blacks in this Northern latitude? My request would be more fully answered, if it suggested to you a theme for a paper in your excellent Magazine, which I never fail to carefully read.

And this reminds me that I am indebted to you for an appreciated review of my recent work on *Sterility*, in a late number. One suggestion occurred to me when reading it, viz.: that it was not intended to instruct the *magna clara lumina* of the profession; all the good intended for them was, to put into a condensed form the floating opinions and the settled facts scattered through many, often inaccessible volumes. With the highest respect, I am, most truly yours, Augustus K. Gardner.

Art. V.—*Letter from* Prof. Lawson, M. D., *on the Statistics of Consumption.*

Cincinnati, Jan. 26th, 1858.

My Dear Doctor: As I mentioned in a former note, my attention is directed to the subject of *Consumption;* and I am making an effort to collect facts on that subject.

With the view of ascertaining the comparative prevalence of the disease in the different portions of the United States, I am collecting the statistics, as far as possible. Can you aid me in obtaining the statistics of phthisis in New Orleans, for a series of years? It is a subject of

more than ordinary interest. You will confer a special favor, by furnishing the facts in regard to New Orleans.

Our statistics, generally, are lamentably defective; yet I hope to obtain enough to demonstrate the points where the disease prevails to the greatest extent. Very respectfully,

Dr. B. Dowler. L. M. LAWSON.

ART. VI.—*Queries.*

1st. How long will the placenta continue to perform its functions after vitality ceases in the fœtus?

2d. If from known causes at seven months' vitality should be destroyed in the fœtus, but having been carried to full period, will the placental murmur continue distinct up to hour of delivery?

Please give me Dr. Dowler's views, or that of others, through Medical Journal, or otherwise, and oblige, yours truly,

WM. A. MCCLURE,
Shugarlak, Miss.

ART. VII.—*Letter from* HENRY HUGHES, ESQ., *concerning the Animal Temperature of the White and Black Races.*

PORT GIBSON, MISS., Jan'y 12th, 1858.

BENNET DOWLER, M. D., ETC.—*Dear Sir:* In the peculiar sociology of the South, I find it important to know the comparative thermology of the races.

1. As a rule, is the natural caloric of the bodies of races the same? Is animal heat a unit, or specifically different?

2. If, in the different races, the body's natural caloric is different, what is the comparative difference between the natural caloric of the African's body and that of the Caucasian's body?

In this matter, there can be no higher authority in the world than yourself. I therefore invoke, without hesitation, your scientific attention to these points. So far as I can ascertain, the science of "Ethnical thermology" has never been developed.

I have the honor to be,
Very respectfully, your friend,
HENRY HUGHES.

ART. VIII.—*On Prolonged Gestation.*

DR. DOWLER—*Dear Sir:* Among your extensive notes, you have doubtless some on prolonged gestation. Presuming such to be the case, and being at the same time aware of the difficulty of collecting a number of cases, I herewith forward the notes of a case:

February 20th, 1857—Cessation of menstruation; April 20th—Nausea commenced; June 15th—Motions of child distinctly felt; December 11th—Confined; breech presentation, first position; labor tedious; child weighed 11 pounds; mother and child in good health to date. I may mention the lady in question is the wife of the Rev. * * * of this place. The above notes of dates are not from memory, they having been noted at the time.

Yours truly, * * *

January 6, 1858.

ART. IX.—*Letter concerning Births, Deaths and Marriages.*

To DR. B. DOWLER, New Orleans — *Dear Sir:* The American Association for the Advancement of Science, at its session held in the city of Montreal, in August last, appointed the undersigned a committee to prepare and report a plan for a uniform system of registration of births, deaths and marriages, applicable to the United States.

The necessity for such a measure, to meet the growing demands of science in its application to vital statistics, and the facilities which it would afford in establishing legal evidence in courts of justice, are of too obvious a character to need enforcing by argument. The success with which systems of registration have been employed in Europe, and the gratifying results that have attended their application in some portions of the United States, lead to the hope that the time is not distant when we may have throughout the Union a practical and thorough system, accurate in its details and comparable in its results.

From the diversity that exists among the several States, with respect to the organization of the subordinate divisions of government, and the duties of the several local officers who would be charged with recording the primary facts, we apprehend that the details of arrangement would need to be varied, to meet the circumstances of each, although the results, when obtained, might admit of a uniform classification.

To acquire a knowledge of these varied conditions, and of the means best adapted for meeting them, we respectfully request an expression of

your views upon the following points of inquiry, as applied to the State in which you live, or in which you are best acquainted with the local details of State government:

1. What officers or persons might with the greatest propriety be charged with the duty of registering births, deaths and marriages?

2. What rules would be necessary to secure full and accurate reports to this officer?

3. What compensation should be allowed for registration and report, and how should this be paid?

4. What are the probable difficulties that would attend the application of the system, and how might they be lessened or removed?

5. What plans of registration are now in use, and what is their history, and what are their defects?

6. Have any systems of this kind been attempted without success? and if so, what were the causes of their failure?

Any further facts or suggestions that you may be pleased to offer in reference to this measure will receive our careful attention. It is particularly desirable to procure any reports or publications that have been issued, or laws that may have been passed, upon this subject. We are also desirous of procuring sets of the blanks that may be in use for making registration returns.

Persons to whom this circular is addressed, if living in the New England States, will please direct their answers to E. B. Elliott, Esq., at Boston; those living in New York, New Jersey, Pennsylvania, or the States north of the Ohio river, or north line of Missouri, to Dr. F. B. Hough, at Albany, N. Y.; and those living in the Southern and Southwestern States, to Dr. J. Wynne, 62 Clinton Place, New York City.

It is hoped that these inquiries may elicit a full and free expression of opinion and advice upon this deeply interesting and very important subject, and that, from the combined experience of those who have given it their attention, we may be able to recommend a plan that shall meet the existing requirements of science, and accomplish the varied practical results expected from it.

We are, with great respect,

Your obedient servants,

JAMES WYNNE,
E. B. ELLIOTT,
FRANKLIN B. HOUGH.

ART. X.—*Letter from* S. M. BEMIS, M. D., *on the Influence of Marriages of Consanguinity upon Offspring.*

LOUISVILLE, KY., January 12, 1858.

DR. B. DOWLER, *Dear Sir*—I hope it will prove convenient and agreeable to give me your aid. I have been successful, beyond my anticipations, in collecting accounts of marriages between kindred parents; so much so, that it is scarcely probable that any additional number of instances will materially affect the result. I am now laboring to ascertain the results of marriage, under circumstances where no influence of consanguinity prevail. So that, by a comparison of results, we may ascertain whether commingle ment of the same blood exerts any, and if any, what influence upon the offspring.

If your study of the vital statistics of the South have led you to form any conclusions in reference to the following inquiries, will you please state them to me? What is the average fecundity of marriage in your State or parallel? What the average duration of the child-bearing period? What proportional number of children survive to maturity?

All the inquiries being considered to relate to the rural population; for almost all my consanguinity statistics, are drawn from a population inhabiting rural localities.

Do you know of any authoritative publication, in journals or elsewhere, upon supposed causes of congenital defects; or, the influence of parental idiosyncracy, or, impressibility of parental peculiarities, upon offspring?

Will you do me the great favor to give me the benefit of your long attention to important points of physiology, in answer to my inquiries above?

If I should have time to analyze and offer my report, it will present the profession with a great amount of new and probably valuable information; for it will contain the results of probably not much short of 1000 instances of marriage between kindred parents.

The points, of which I wish more especially to be informed, are:

1—The degree of consanguinity of parties, (whether first, second, third or fourth cousins, or, as in rare instances, uncle and niece)?

2—The date, approximative, of marriage?

3—The number, sex, and *condition* of children, born to each marriage?

4—The number of children dead, cause of death; and age, when known?

5—The constitution, temperament, and occupation of parents? with any habits or circumstances calculated either to favor or retard the normal developments of offspring.

I wish my report to be entirely unprejudiced, my only aim is *truth;* and I desire those instances of such marriages where no defective issue results, or, which are sterile, as carefully sought out and reported as the opposite; and, whenever the defects of offspring may be reasonably referred to other causes than consanguinity, I wish the facts distinctly set forth.

I am well aware that investigation is, in these cases, often foiled by the sensitiveness of parents upon the subject of kinship; but, in such instances, information as circumstantial and reliable may often be obtained from friends of the family.

I trust, my dear sir, I will not be accused of making undue demands upon your time and patience, in these requests; such is my anxiety to furnish a comprehensive report, and one that will embody a great amount of new and valuable information, that I will await your answer with much solicitude, and cherish grateful recollections of all who consent to become my co-laborers.

If your engagements should prevent your own participation in these inquiries, would you be so kind as to entrust them to an intelligent student, or any reliable person whom you may select?

Very respectfully, and truly yours, etc.,

S. M. BEMISS.

P. S. Names of parties not desired, as all personal allusions will be avoided in my report. Should you know of any dissertation, in English or French, calculated to throw light upon any of the inquiries contained in my letter, please do me the favor to order them to be sent to my brother, J. B. BEMISS, Esq., of New Orleans.

ART. XI.—*Vis Medicatrix Naturæ in Surgery.*

DR. BENNET DOWLER—*Dear Sir:* Wishing to inform the profession of an occurrence which I consider rather novel, I know of no better mode than through your most excellent Journal.

Early in the spring of 1857, I was consulted by Major J. Willis, in reference to an adipose tumor, of the size and shape of an almond, situated on the scalp. I advised excision, but my hands at that time, from debility, being too unsteady to operate, I postponed it until fall. But during the summer, he called my attention to a small black spot on the tumor, just where his hat rested when wearing it. This spot was about the size of a small pea, and looked as if the skin was being absorbed, leaving the tumor exposed. Wishing to see the result of the absorbing

process which I thought was going on, I requested him to let me see the tumor again in a month or two. Accordingly, early in the fall, he came to me with the tumor adhering to the skin by a very small portion, and he wishing to get rid of it on account of the offensive odor it emitted, I, in the presence of Mr. John E. Burch, removed it with my thumb nail, without the loss of five drops of blood.

Now, why this spontaneous elimination? The old gentlemen, during the summer, had fallen into very bad health, but the tumor had existed for some thirty years, during which time he had suffered frequently from ill-health.

If you think this phenomenon of enough interest to insert in your Journal, do so. I remain yours, etc.,

THOS. B. HOPKINS, M. D.

Lanier, Claiborne Parish, La., Jan., 1858.

ART. XII.—*Ethnology and Antiquities.—Letter from* PRINCE MICHEL OBRENOVITCH, of Servia.

[WHILE the last sheets of this journal were passing through the press, THOS. MULLETT, U. S. Deputy Surveyor, then attending Dr. Boynton's able lectures on geology, in New Orleans, called at my house, and communicated many interesting facts in regard to antiquities—as mounds, works, etc.—which have fallen under his observation, in making surveys in Louisiana.

He made a circumstantial statement concerning a vast number of human bones of extraordinary size, recently found in an ancient cemetery near a large mound, in connection with the remains of very regular fortifications or earth-works, upon which the oldest class of trees are now growing. The colossal size, attributed to these bones, cannot, of course, be admitted without the utmost scrutiny of competent anatomists; and this subject is now alluded to only for the purpose of calling upon the readers of this journal to investigate the antiquities, ethnology and palæontology of the South.

The Royal Society of Northern Antiquaries, which now numbers many learned men and nearly twenty crowned heads, in Europe, Asia and South America, has probably a richer cabinet of American antiquities, illustrative of American ethnology, etc., than can be found in America itself, excepting the department of craniology, by the late Dr. Morton, of Philadelphia.

The following letter, (an autographic manuscript,) signed PRINCE

MICHEL OBRENOVITCH, of Servia, gives some of the details of the proceedings of this society, at its annual meeting.* B. D.]

To BENNET DOWLER, M. D., Fellow and a Founder of the Royal Society of Northern Antiquaries, etc., New Orleans, Louisiana.

Royal Society of Northern Antiquaries.—The annual meeting of this society was held at the Palace of Christiansborg, Copenhagen, on the 5th of May, under the presidency of his Majesty, FREDERICK VII., King of Denmark.

The secretary, Professor C. C. RAFN, communicated a report of the proceedings of the society for the past year, and exhibited:

1—The Annals of Northern Archæology and History, for the year of 1854, with two plates, containing—*First:* The Saga of King Oswold, the Holy, in the Old Northern or Icelandic original; with an introduction by John Sigurdsson; together with a Danish translation, by Thorleif G. Repp. *Second:* Inquiry into the ancient history of Virdaland, with a glance at some Swedish remains, by Abraham Cronholm of the University of Lund. *Third:* Notice of the Danish language in Angel, by the Rev. Eiler H. Hagerup, of Great Solt, in Sleswig; and of the Dalecarlian and Gothland dialects, by Carl Säve, of the University of Upsala. *Fourth:* Faro grammar, by the Rev. Veneeslaw U. Hammershaim, of Nord Straumey, Faro Islands. *Fifth:* A monumental inscription of the 14th century, with remarkable variations in the form of letters, by Jacob Helms, of the College of Ribe, in Jutland. *Sixth:* A Runic Alphabet, of 1547, communicated by P. Seidelin. *Seventh:* Description of several remains in Scania and South Halland, examined by Nicolas G. Bruzelius, of Lund.

II—The Archæological Review for 1852-1854, with illustrations, containing several treatises, among which: On the old Northern language, by George E. Lund; old English and old Northern, by Gisle Brynjulfson; on the remains of Orkney, by George Petrie, of Kirkwall; on the bronze of the Kelts, Germans, and Slaves, by John E. Woeel, of the University of Prag; a Report on the cabinet of American antiquities with ethnographical remarks, by C. C. Rafn; on the antiquities of the Slavonian nations and Bosphora and Bosphorian monuments, by Edwin M. Thorson; connexion of Denmark and Portugal in the middle age, by Erik C. Werlauff.

III—Vestiges d'Asserbo et de Söborg, découverts par Sa Majesté Frédérik VII, King of Denmark, with four plates.

* The transactions of the recent meeting have not, it is believed been published as yet. The late learned and lamented Col. Bliss, of the Army, has received high commendations from the Society, for his archæological researches, and for his contributions from the States of Georgia, Arkansas, Alabama, Louisiana, Mississippi and Texas; as have, also, Dr Gibbes, of Columbia, S. C., and Mr. Norman, of New Orleans. Dr. Macaulay, of this city, has lately been elected a member of this Society.

IV—Mémoires des Antiquaires du Nord,1845 to 1849, with five plates, among which is a paper of the Scottish and Irish names of places which occur in the Icelandic Sagas, by P. A. Munch, of the University of Christiana ; remarks on a Danish Runic-stone from the eleventh century, found in 1852, in the central part of London, in the ancient churchyard of St. Paul, and of the Runic inscriptions in Sweden and Norway, in which the western parts are alluded to, by Carl C. Rafn ; on the old French romance and the influence which the Northmen had in its development, by Gisle Brynjulfson.

V—*Lexicon poeticum antiquæ linguæ septentrionalis anscripsit Sveinbiorn Egilsson, Facs. II.*

His Majesty the King communicated a report of an inquiry, conducted under the direction of his Majesty, in the previous summer, in the royal vaults in the church of Ringsted in Sealand, and desired the Vice President, Casper F. Wegener, to read extracts, elucidating the interesting discoveries made at the opening of the tombs of King Valdemar, the Great, and the queens, Sophia, Princess of Russia, and Beengierda, Princess of Portugal. His Majesty exhibited and explained several drawings of the tombs, and objects found therein — which are subsequently to be published.

Counsellor Christian J. Thomsen showed several fac-similes of northern antiquities, and explained their nature and purpose.

Professor Carl C. Rafn gave an account of his exertions in promoting further discoveries as to the runic monuments, in various parts of the north, in addition to the treatises he has already produced in the Annals and Memoirs of the Society; he showed that, by correspondence with several associates and coädjutors, in Sweden and Denmark, he had obtained new confirmation of many remarkable runic inscriptions, which he was now engaged in preparing for the press. With regard to language, these had peculiar interest; showing the nature and character of the Old Northern or Danish tongue (donsk-tunga) at the end of the pagan period, and the first centuries of Christianity in the Scandinavian north.

New members were elected, and auditors chosen to revise the accounts of the preceding year. Of the members received into the Society, in 1855 and 1856, the following were entered in the class of Fellows and Founders, or, *Membres Fondateurs:* Sir John Bowring, Vice Admiral and Governor of Hong Kong ; Professor John E. Holbrook, of Charleston, South Carolina; Count L. Holstein, of Holsteinborg, in Sealand; Major Andrew Lang, St. Cruz, West Indies; Count Alexander Przezdziecki, of Warsaw; James W. Pycroft, Esq., London; Ambrosiodi Stefano Ralli, of Trieste; and Baron Edward Rastaweicki, of Warsaw.

Prince Michel Obrenovitch, of Servia.

REVIEW.

Review of the Report of the Board of Health for 1857.

The Report of the Board of Health for 1857, by its able President, Dr. A. F. Axson, contains many passages of interest to the public and profession.

The attention of the Legislature is called to the transfer of sufficient land to the federal government to enable it properly to dispose of the $50,000 appropriation for the construction of "suitable warehouses, with wharves and enclosures," as of "vital importance to the efficient administration of a system of quarantine laws."

The action of the Philadelphia Quarantine Convention is reviewed; and it is regarded as a strong "indication of the shock medical doctrines have sustained by the incidents of pestilential yellow fever since 1853, that in the first public assembly of a congress of medical men, a point, formerly so controverted, is conceded, and yellow fever is classified with small-pox and typhus, as disorders susceptible of introduction into communities by the same vehicles and in the same modes. In contemplation of such a result, we must surely regard the Convention as having moved forward with a bold stride towards the frank and open admission of the doctrine of the transportability with all its consequences."

Dr. A. observes that, "in the spread of epidemic yellow fever, three agencies have been usually invoked by way of elucidation of the observed phenomena, viz.: telluric emanations, peculiar atmospheric conditions, and the effluvia escaping from the sick, and capable of exciting in a healthy person a similar disease. That the first, or telluric emanations can have no virtual participation, if they ever have any existence in the production of such a result, must be evident from the fact that the fever has prevailed in localities dissimilar in every respect from each other; in composition of soil; in growth of vegetation; as to dryness and moisture; in low and in elevated places; in pine barrens, and along the margin of water-courses; in city, town, and hamlet," etc., etc. As to peculiar atmospheric conditions, "it is a well settled law in physics, that vapors, which are nothing more than thin films of water, have the property of absorbing or preventing the passage of many calorific rays. They do not simply divert, but actually arrest them, and thus prevent their transmission. And according to the ingenious researches of Robert Hunt, it would appear that the very rays thus retarded, belong, for the most

part, to the class which have the peculiar scorching and de-oxidizing power attributed to the parathermic rays by Herschel. Were it not for this wise provision of nature in the form of vapor in the air, the latter would cease to be the great equalizer of solar heat, and the face of our globe now so enriched with vegetation and animal life, would present the desolate aspect of the burning wastes of Africa."

"Just as we have seen the fever to spread in localities, exhibiting every diversity as to soil, organic remains, humidity and dryness, so we know from abundant testimony collected in two hemispheres, on both sides of the Equator, in the interior of continents and on insular positions, that the conjunction of any two of its properties in excess, and especially of its temperature and moisture, are not the main or essential factors in the production of its epidemic equation. Professer Gardener, recently of the University of Virginia, in a well considered and able paper, after elaborate experimentation, conducted with skill and judgment, thus concludes: 'The doctrine that a high dew point is productive of marsh diseases, is altogether untenable, for it will be found that the mean dew point of the summer is, in the United States, upwards of 15° above that of the autumnal season, and that of 60° F. is often experienced months together in the United States without ill health. On the other hand, the fens of Lincolnshire, Walcheren and the marshes of Holland, are pestiferous with a dew point of less than 50°.' In this state of ignorance and uncertainty as to the nature and influence of atmospheric changes, we cannot regard them, as at the best, more than auxiliary means and certainly not to be ranked among the foremost or essential agents in the production and spread of pestilential fevers. In protesting, therefore, against the dogma of an elevated temperature and a high dew point as the peculiar meteorological conditions requisite to originate yellow fever, it is not to be inferred that the hypothetical suggestions as to the possible connection of the parathermic rays of Herschel, thrown out by us rather as hints, provocative of further inquiry, are to be received for more than they are worth. Wrong as they may be, we regard them as a rationale of the cosmical phenomena involved in the causation of disease, more approximative to truth and in stricter analogy with known adjustments of the physical world. We mean them as simply conjectures and worth just the consideration they will receive."

The third agency, "Effluvia escaping from the sick," is regarded by Dr. A. as the *sine quâ non*. "That the cause of yellow fever is a specific morbid poison, is hardly disputed in our day; equally indisputable is the fact that this cause can attach itself to substances that have been about the sick and has been thus preserved and transported from place

to place, and is endowed with power to infect far beyond the sphere of its original birth-place — a power capable of perpetuation, potent, immutable and precise in its action. There can be no denial of these often attested facts. The doubtful and disputable feature arises only at the point of the connection of this cause with the atmosphere."

The victims to yellow fever the past year numbered 199. Some five cases were reported prior to the two considered by Dr. A. as the first indisputable cases—one, Christopher Miller, dying on Sept. 20th, the other, Franco Savali, on the 21st. A large proportion of the yellow fever cases of this year originated in this locality,* in which there existed neither telluric emanations, nor peculiar atmospheric conditions, which did not exist in as great a degree in some places, and much greater in others than in this. These two causes, therefore, being insufficient to account for the origin of the disease, it is *inferred* that its cause must be looked for in its importation—the "effluvia escaping from the sick." No way is known by which this could have occurred, unless through Franco Savali, who was admitted into the city via Mobile, and who "had recently been in Havana, and only eleven days before his death in the city." Such is asserted to have been Mrs. Rose's testimony, whom Dr. A. regards as a more credible witness than either her husband, or a Larose, each of whom stated that Savali had not been in Havana for six months and more. On referring to appendix E for Mrs. Rose's testimony, it will be found that she says, "that Franco (Savali) had been *in the city about eleven days* before he was taken sick; that he had *recently come from Havana;*" but not "eleven days before his death in the city." Christian Miller, a neighbor of Savali's, was taken sick on the 14th, *up the river*, having left New Orleans on the 11th; it is difficult to understand how his disease could have been communicated to him by Savali, who died on the 21st, and who, according to the Report, "had recently been in Havana, and only eleven days before his death in the city."

One committed to the doctrines of neither the contagionist, nor the non-contagionist, will scarcely find, in the facts and testimony so far as collected, much to discredit the opinion that the fever of 1857 originated in New Orleans.

The Report urges upon the Legislature the enactment of adequate laws for "the registration of births, deaths and marriages." "The chief object of a correct civil registration of births, deaths and marriages, considered physically, is to aid in disclosing the causes of disease; considered legally, to provide the means of tracing descent and proving personal identity; and politically, to aid government in arriving at correct conclusions with regard to measures of internal economy, the growth

* Corner of Girod and Tchoupitoulas streets.

of population, etc. In the first, it involves the whole science of vital statistics, and the social dynamics of a people; under the second, the personal and civil rights and status of the individual; and, under the third, their economical and industrial labors, and the true relation between these and the government they support."

"All sanitary reform reposes on vital statistics whose legitimate functions are to define and indicate the sources of disease and death. In older and more advanced States, their relation to the material well-being and happiness of a people, is made a subject of special care on the part of government; and they are collected with particular exactness, and analyzed and explored with careful scrutiny. Out of these have sprung many of the important reformatory improvements of the age, in which the poor and the improvident—those fatal cankers in all free social bodies, have been largely the beneficiaries.

"The value of drainage as a means of security for life and health, the dispersion of the poor, the improvement of their food and water, the regulation of their labor, and the construction of model lodging-houses, are a few of the humane consequences flowing from the startling records of the vital statistician. True it is that we have no Poor Rates to support nor to serve as a monument, less perhaps of the charity and good will of society, than a proof of a fatal malady exacting its reluctant tribute to avoid the dire alternative of plunder and spoliation, yet an enlightened patriotism should prompt us not to rest satisfied with this negative advantage, but to show that in our social organization, labor and capital are so harmonized, that the former feels none of the wretchedness, want, destitution and physical degradation, nor the latter any of the feverishness, anxiety and gloom of a boasted free society. These two interests, represented by the two races that compose the stratum upon which our social fabric is reared, move together harmoniously, the profits of the one contributing to the comforts of the other, and in these mutual services proving the naturalness of the relation and the stability of the connection. Thus socially organized and living under the same climatic influences, and exposed to the same causes of disease and death, our vital statistics should be singularly instructive, if collected on a sufficiently broad and comprehensive scale. As far as this has been attempted, fragmentary and incomplete as it is, it has established two remarkable truths, viz: that freedom to the negro, in the midst of the civilization of the 19th century, is a curse, for it entails on him insanity as its consequence, in a degree altogether without a precedent in his state of servitude; and secondly, that the vital energies of the negro in the relation of a bondsman are extraordinary.

"Mr. Carey, in his work on the slave trade, has shown the increase in the Southern States of the slave population to be in a ratio of 8 to 1 over the numbers imported; while in the West Indies, where he is left to his own care, the scale has been reversed and his numbers have declined in the proportion of 2 to 5 of the original importation. These are curious and instructive disclosures, suggestive of the uses, a well ordered statistical survey, made periodically, and embracing a wide scope of details, might lead to."

The total mortality for 1857 was 6,067. "Our records show a mortality, caused by trismus nascentium, for the last eight months of 1855, of 143 children; of 346 in 1856, and 199 (should be 188) in 1857. It is to be remarked here that on an average of years more than four-fifths of this mortality occur among the subjects of midwives." The whole number of still-born children was 358. Dr. A. writes, "the astounding proportion of still-born children for the year 1857 cannot fail to arrest the notice of persons accustomed to examine the vital statistics of communities. Were not the fact and its causes well known to the medical practitioners of the city, this waste of human life would be a grave impeachment of their skill, knowledge and efficiency in one of the most trying and momentous crises of female life. Fortunately, however, for the high character of the profession, it is found that the mortality, when traced to its source, occurs in the large ratio of eight out of every ten among the midwives of our city. Indicating, as this does, a fastidious and reprehensible social prejudice, which patronizes and sustains a mischievous and ignorant body of women, whose fitness for such responsible offices is unfortunately too often measured by their hardihood and effrontery; the existence of the evil is referable to the unwarranted and unwise removal, by a former Legislature, of the safeguards thrown around the public by the license exacted by the law for the privilege of practising this branch of the medical art. In the desire to realize an airy nothing and an abstraction, in the destruction of what was called in a vein of grinning mockery, a monopoly, your predecessors, in the solemn responsibility of guardians of the public weal, confounded protection with monopoly; protection against temerity and ignorance, by throwing down all the legal barriers a wise and conservative spirit had erected; by discrediting education, industry, and science in their wholesale reduction to the vulgar level of the shallow and the uneducated; and by discountenancing the just distinction between the properly qualified, and the unqualified, thereby leading to the confusion in the popular judgment, that in the eye of the law and the law-maker, the one was as respectable as the other, as safe, reliable, and as useful."

The report urges upon the Legislature in foreible language the prosecution of its design, already begun, in appropriating "the sum of five thousand dollars for the purpose of a preliminary survey of the swamp lands in the rear of the city, with a view to their more perfect drainage." And after eloquently advocating the benefits to the people, socially and morally, and to the city in point of health, Dr. A. well observes, that "the hygienic and salutary consequences of effective draining need not be enumerated. They are thrice told tales of which the dull ear wearies."

In reference to its pecuniary matters, "it affords the Board much satisfaction to observe that its affairs have been managed with such discretion as to not only exempt it from asking any further appropriations, but to demonstrate the self-supporting character of the institution, under a prudent and watchful administration." The receipts and expenses have been about $47,000, and of the receipts about $35,000 were quarantine fees upon vessels, the total number of which (of all kinds) were 1805. At the quarantine station, Mississippi river, 82 patients have been treated, of whom 27 died. Of the 82 patients 51 were sick with yellow fever. CHAILLÉ.

PROGRESS OF MEDICINE.

ART. I.—*Experimental Researches on Normal Albuminuria in Man and Animals:* By DR. CLAUDE GIGON, Physician to the Hospitals and Prisons of the City of Angoulême. Translated for this Journal, from *L'Union Médicale*, by M. MORTON DOWLER, M. D. (Continued from the January No.)

THE chloroformic precipitate acts in very nearly the same manner in the presence of potassa. After the decantation of the supernatent liquid from the chloroformic precipitate, if a solution of potassa be added, (20 grammes to 100 grammes,) the precipitate remains undissolved; but if an excess of potassa be added to supersaturation, the precipitate is completely dissolved, without the aid of heat, even at the 1–20 dilution, and the chloroform is disengaged.

Chloroform is not only an excellent reägent for albumen, but it has further this advantage in testing the presence of this substance in the complex liquids, which contain a great number of salts, as urine, the

serum of the blood, etc., that it exhibits no reäction with the saline fluids of the animal economy, as I have proved by experiment, acting only on the albumen, which it coagulates and precipitates; while the saline tests before enumerated yield precipitates with the chlorides, the phosphates, the sulphates, and the carbonates of potassa and soda. I have assured myself, by repeated experiments, that chloroform yields no precipitate with either the solutions of the chlorides, the carbonate, the phosphate of potassa, the phosphate of soda, or with a solution of urea, that is to say, with most of the constituents of normal urine.

If, after having precipitated albumen in a solution, by means of a few drops of chloroform, the supernatant fluid be so decanted as to leave the precipitate alone in the bottom of the tube, and if a great excess of chloroform be added to the precipitate after a strong agitation of the contents of the tube, it will be seen that the precipitate will not be dissolved; but on the contrary, the albuminous molecules will be rapidly disengaged, and will arise above the chloroform, or they will, by degrees, form a cylindrical clot or coagulum, of a thickness in proportion to the quantity of the albumen, or a sort of membraneous pellicle, if the albumen be in a very minute quantity. The water which remains after the decantation, rests above the chloroform and the albuminous coagulum in such manner that the latter seems to float between two waters.

On the different solutions of which I have spoken, I have practised this experiment, and I have seen cylinders of albumen decreasing in succession, from the one-twentieth to the one-ten-thousandth dilution; in such manner that it were easy, approximatively, to state the quantity of albumen in any albuminous fluid, by measuring the thickness of the cylinder. The albuminous coägulum is affected, as I before stated, by the presence of nitric acid; that is to say, it is dissolved when the coägulum is in small quantity, and it remains undissolved when the coägulum is abundant. I wished to try the power of chloroform on concentrated albumen. With this view, I took six grammes of the white of eggs, which I placed in a test tube, and added six grammes of chloroform, strongly agitating the mixture. The white of egg, which was almost fluid, was solidified and consolidated at the bottom of the tube; but it was necessary in this case, in order to coägulate the albumen, to agitate the mixture for a much longer time than in the case of the largely diluted solutions. It will be readily conceived that it requires a much longer time for the chloroform to combine with the molecules of a thick viscous body, than with those in a complete state of solution. I added a great excess of nitric acid, and the coägulum remained undissolved: but this mixture having been submitted to a few moments' ebullition, the coägulum was completely dissolved.

Next to chloroform, as a reägent, we may place creosote, which, up to the one-five-thousandth dilution, exhibits reäction; but it is quite inferior in power to chloroform; for, whilst the latter always yield a well-defined precipitate, not only up to the one-ten-thousandth, but even up to the one-twenty-thousandth dilution, creosote, beyond the one-three-thousandth dilution, produces nothing more than a dirty white turbidity, which is almost without character.

Nitric acid, tannin, and the metallic salts, stand very nearly in the same rank, and exhibit reäction up to the one-thousandth dilution; but when the dilution is increased to the one-three-thousandth, they no longer exhibit any clearly recognizable sign, except in the case of tannin and the subacetate of lead, which only lightly display their power on this minute quantity of albumen.

Nitric acid is one of the reägents the most generally employed to test the presence of albumen, and indeed it has been the usage to regard urine, or any other liquid, as non-albuminous, which does not yield the precipitate to nitric acid. The preceding facts show clearly that this opinion is erroneous, since we have shown that chloroform reveals the presence of albumen in fluids which yield no sign of this substance to the action of nitric acid. Thus, if distilled water, charged with the 1–5000, or even with 1–3000 part by weight of albumen, (white of egg) be treated with a few drops of nitric acid, no precipitate is produced, and no turbidity; but if in this same fluid, and *on the nitric acid*, we add a few drops of chloroform, and agitate strongly, there will be seen formed, in a few minutes, the albumino-chloroformic precipitate of which we have spoken.

The precipitates obtained from albuminous solutions by nitric acid, have exactly the same habitudes as the albumino-chloroformic precipitates. Thus in solutions of the 1–20 dilution and less, after decantation, the precipitate of the white of egg is not dissolved by the addition of excess of nitric acid without heat; but if the precipitate be heated in a little globe, by means of a spirit lamp, it is dissolved long before arriving at the point of ebullition. As to the other precipitates forming between the 1–100 and the 1–1000 dilutions, if we decant, and pour on the precipate nitric acid in large excess, the precipitate is immediately dissolved without the aid of heat. I have even seen the precipitate of the 1–50 dilution dissolve in the same manner.

Hitherto authors have manifested great difference of opinion on the experimental question: Is albumen dissolved by the action of nitric acid? Becquerel holds the affirmative of this question in the following terms: "When urine contains but a *small quantity of albumen*, the employment of nitric acid begins by precipitating it; but if the acid be added in excess

it *is capable of dissolving it.* Concentrated nitric acid, at the boiling point, dissolves it even *in great quantities.*"

Mailhe, *(chimie appliquée à la physiologie, etc.)*, on the contrary asserts, in the most positive manner, the insolubility of albumen in nitric acid, and says, "normal physiological albumen enters largely into the composition of the blood; is identical with the albumen of the white of eggs, and is precipitated by heat and nitric acid, without the precipitate being soluble in excess of that acid," (page 151,) an opinion which is reported at page 162, and still further at page 172, where the author refutes Bérard, Martin Solon, and Becquerel, who had declared to their having seen nitric acid, after having precipitated the albumen of urine, redissolve the precipitate in excess of the acid; and finally, adds M. Mailhe, "*normal* albumen is not dissolved, but *modified* albumen is dissolved with great facility, when the acid is in excess."

The experiments we have reported appear to definitively solve the difficulty. The egg-albumen, and the blood-albumen—*the latter being identical with the former*, (Mailhe), and not being modified albumen—have the same reäctions. When albumen exists in a considerable quantity, as in solutions of 1–20 to 1–10, or in greater proportion, it is precipitated and coagulated by nitric acid which does not redissolve it without the aid of heat, but which very effectually dissolves it by the aid of that agent, before reaching the point of ebullition. But when, on the contrary, the albumen is minute, ranging from the 1–100 to the 1–1000 part—the latter being the limit to the power of the nitric acid reäction—the precipitate is constantly dissolved, without the aid of heat, but nitric acid in excess.

Next in order, after nitric acid, we may speak of tannin and the metallic salts, the former and the subacetate of lead being the most efficient of the group. They give some traces of reäction as far as the 1–3000 dilution, at which point the effect is almost imperceptible. The neutral acetate of lead is feebler in its reäction, showing but little evidence of action at the 1–500 dilution.

Alcohol is also a good reägent in the presence of albumen. The coägulations and reäctions that it determines, take place in a different manner from those which occur with chloroform. The latter being heavier than water or urine, forms its precipitate at the bottom, and yield the former its coägulation in the form of a white coloration on the surface. Thus, when the albuminous solution is from the one-tenth to the one-twentieth dilution, the fluid forms with alcohol a white and very thick deposit in the upper portion. At the 1–100 dilution, it is very sparingly exhibited; and at the 1–500, there is but a slight annular cloud, almost imperceptible, a trace of which is barely visible at the 1–1000 dilution.

The last, in order and efficacy, as a means of detecting albumen, is heat. If a fluid, containing one-twentieth part of albumen, be heated to the boiling point, it will be seen that the whole becomes turbid much below that point, forming at sixty-five degrees flakes which swim on the top, and the body of the fluid acquires an opalescent tint. If there be present one-fiftieth to one-hundredth of albumen, it becomes opalescent, and the flakes are of very small dimensions. Lastly, if the albumen be in the proportion of one-one-thousandth, the fluid, though subjected to prolonged ebullition, remains free from turbidity, exhibits no reäction; and we see only above the liquid, on the parietes of the tube, as far up as the fluid has been driven by ebullition, traces of coägulated froth, which is evidently albumen.

If fluids containing albumen in the proportion of one-one-thousandth, one-three-thousandth, and one-five-thousandth, be submitted to ebullition in a test tube, there results neither turbidity nor coägulation; but if, after ebullition, a few drops of chloroform be added, and agitated strongly, there forms at the bottom an abundant precipitate, of an alabaster white. This precipitate is also produced in albumen, at the one-tenth to the one-twentieth dilution, after the ebullition has separated all that it can, and when it gives no further turbidity to the menstruum.

The flakes of coägulated albumen, precipitated by heat, are insoluble in unheated nitric acid; but by the aid of heat, and much lower than the boiling point, they are completely dissolved.

It will be seen that in the exposition I have here detailed, I have not discovered or recognized any difference or distinction between the solutions of the serum of the blood and the white of eggs, having found the reäctions and the phenomena, in the two several solutions, to have been *identically* the same, with this single difference, that the former exhibits feebler reäctions than the latter, depending on the mere quantity of albumen present, for six grammes of the former contains necessarily less albumen than ten grammes of the latter. Dumas, (*chimie, physiologique et médicale*, p. 665,) gives, by experiment, ten per cent of albumen for the serum of the blood, and thirteen per cent for the white of eggs.

(*To be continued.*)

Art. II.—*Results of Treatment in Seventy-one Cases of Pneumonia Treated in the Glasgow Royal Infirmary; with some Remarks on Dr. Bennett's novel views on this Disease.* By Joseph Bell, M. D., late Physician to the Royal Infirmary, etc. (Read at the meeting of the Glasgow Medico-Chirurgical Society.)

[The following paper, by Dr. Bell, has been greatly mutilated, nearly

half having been lopped off, in order to adapt it to the limited space of the N. O. Medical and Surgical Journal. The most material portion, however, will be re-produced.]

Class I.—Acute and extensive affections of either one, or both lungs; high febrile excitement; great difficulty of breathing; rusty tenacious expectoration; minute crepitant râle, followed by extensive hepatization, as evinced by percussion and auscultation. Several of the instances in this class, on admission, had some amount of hepatization, conjoined with the stages of hyperæmia and effusion. The disease was of a sthenic character in twenty-three patients, and asthenic in other four. In all, twenty-seven cases, six of which were complicated with pleuritis.

Age.—Average age, 29 years; sex, 24 males; 3 females.

Treatment.—1. Bleeding, chiefly by cupping and leeching; 2. Mercurials; 3. Purgatives and diuretics; 4. Blisters; 5. Stimulants; 6. Tonics. The antiphlogistics in the sthenic, and the stimulants in the asthenic.

Result.—Mortality, none. *Duration.*—Average duration from commencement of illness to admission, four days; from admission to dismissal, eighteen days.

Class II.—Acute cases, but the disease limited to a small portion of lung; rational and physical symptoms less severe and extensive than in the first class. In several cases the hepatization had also partially taken place when admitted; they were, however, chiefly characterized by being in the congestive and effusive stages. We had nine cases of the sthenic form, and three of asthenic, in all, twelve; in four of which the pleura was conjointly involved.

Age.—Average, 30 years; sex, 11 males; 1 female.

Treatment.—In the sthenic—1. Moderate bleeding; 2. Mercury; 3. Purgatives and diuretics; 4. Blisters; 5. Hydriodate of potash. In the asthenic—1. Stimulants; 2. Mercury; 3. Blisters; 4. Tonics.

Results.—Mortality, none. *Duration.*—Average duration before admission, nearly four days; from admission till dismissal, nearly thirteen days.

Class III.—Cases in which hepatization existed on admission. In many of these a slight amount of crepitation was also detected, but they were principally characterized by the symptoms of consolidation. In some there was urgent dyspnœa, in others the oppression of breathing was exceedingly slight. I need not enlarge; suffice it to say that we had well marked instances of the disease in the first stage of hepatization; in some to a great extent, in others to a trifling degree. We had twenty-eight cases in this stage, twenty-three sthenic, and five asthenic; the pleura was implicated in ten cases.

Age.—Average, 31 years; sex, 25 males; 3 females.

Treatment.—1. Bleeding, viz., cupping in four cases; 2. Mercurials; 3. Hydriodate of potass; 4. Nitro-muriatic acid; 5. Blisters; 6. Tonics. In the asthenics—mercury, tonics, and stimulants.

Results.—Mortality, one death, which took place in a case of pleuro-pneumonia. *Duration.*—Average from commencement of illness till dismissed from hospital, 47 days; time under treatment, 22 days.

Class IV.—Cases in the stage of grey hepatization and softening. We had four instances of this character, the pleura being involved in each.

Ages.—47, 46, 32, and 66 years respectfully; sex, 3 males; 1 female.

Treatment.—Tonics, stimulants.

Results.—1. Mortality, three deaths; one improved. One of these cases will be found reported in the number of the *Glasgow Medical Journal*, to which I have previously referred. 2. *Duration.*—One fatal case, eight days before admission, twelve hours afterwards; another, 120 days before, and 14 days after admission; the third, eight days before, and four days after admission. The other case was of four months' duration.

It is important to remark, that none of these patients had any treatment, before admission to the Infirmary, except of a domestic nature.

Summary.—We have, therefore, out of seventy-one cases, a mortality of four—one dying from the absorption of purulent matter, and the other three patients were in a perfectly hopeless condition when admitted; one living only twelve hours, another three days, and the third lingering fourteen days.

Exclusive of these four cases, we have sixty-seven, thirty-nine of which were admitted in the earliest stages (viz., before hepatization); the disease being extensive in twenty-seven patients, and limited in twelve. In the first stage of hepatization we had twenty-eight admissions, the extent of lungs implicated being very various; and one case in the stage of grey hepatization. Therefore, in these sixty-seven cases we had no mortality, a result which, I flatter myself, may, at least to some extent, be ascribed to the plan of treatment, to which I will now briefly allude.

Treatment.—In discussing this part of the subject, I will confine my remarks to the principal agents employed, viz., bleeding and mercurials. Though the other remedies, especially blisters, are most valuable adjuvants, yet I regard them as quite subsidiary.

Though the practical rule should be to treat every case in strict accordance with its own individual conditions and peculiarities, yet we must bear in mind that the principles of treatment are the same in every variety and form of the disease, viz., *the suspension of inflammatory action, and the removal of the effects of that action;* or, to use terms more consistent with modern views of the pathology of inflammation, to employ means *to correct the abnormal nutritive relations of the part, and to remove the results of this altered nutrition.* Now, I hold that in cases of sthenic pneumonia, these important objects can be effectually secured by the judicious employment of *bleeding* and *mercury;* and that in the asthenic form *stimulants* must be substituted for the former.

In detail, I have to observe—

1. That BLEEDING was only employed in the more extensive, recent, and sthenic cases; the amount never exceeded twenty ounces, the average quantity being fourteen ounces; the mode of evacuation being generally either cupping or leeching. In two cases venesection was practised. In four others, this operation had been performed before admission. Cupping was preferred, because the cases had fairly passed into the effusive stage. Had they been admitted earlier, I certainly would have bled from the arm.

To prevent misconception, and to render repetition unnecessary, I beg to state distinctly, that I advocate moderate bleeding only, neither its excessive nor repeated employment. I may add that these opinions are not founded on my hospital practice alone, but from an experience of twenty years. I repeat, that it is in the early period of sthenic cases that the loss of blood is demanded.

2. MERCURY.—This remedy I consider much more extensively applicable than bleeding, in the treatment of pneumonia. Indeed, I consider it indispensable in all forms and stages, more especially when consolidation exists. As soon as the effects of the remedy become evident on the gums, the condition of the lung rapidly improves.

Bleeding and mercury are most invaluable remedies, and I maintain that the more severe forms of the disease will not be successfully treated unless both are employed.

3. STIMULANTS.—Whisky, brandy and ammonia, were employed in the asthenic cases; in two instances very freely and with marked success. To have bled such patients would have been fatal. Mercury was exhibited from the first. It seemed valueless, until the patients rallied under the use of stimulants.

Whatever objections may be founded on the paucity of my cases, must apply with equal, if not greater force, to the sixty-five cases, from which Dr. Bennett has drawn conclusions calculated to revolutionize all previous opinions entertained on the nature and treatment of this important disease.

Every right-minded person must deeply regret, when diametrically opposite modes of treatment are advocated with the zeal and dogmatism of political partisanship. Nothing tends more to lower the profession in the estimation of the public, and to obtain patronage for every false system. In reference to the present subject, I ask, is it not painful to read assertions made by teachers of medicine, that the high mortality in pneumonia arose, not from the disease itself, but from the antiphlogistic treatment of our predecessors?—that if the patients had been left to *nature*, the disease would have cured itself twenty times out of every twenty-one cases; but when subjected to the care of the *physician*, it proved fatal to seven out of the same number! In other words, under the doctors the deaths were $33\frac{1}{3}$ per cent.; under nature they would only have amounted to 5 per cent. I repeat, that it is most painfully humiliating to contemplate such pictures of the practice of the past.

The credit of the profession demands, that the truthfulness of such assertions should be submitted to the most rigorous examination, and that the principles on which such novel doctrines are founded, should be scrutinized with the greatest care and minuteness.

I think we may regard the recent paper of Dr. Bennett as containing the most able and formal arguments against the antiphlogistic treatment, and accept it as the best exposition of the views of those who advocate the natural method.

Dr. Bennett arranges his arguments under five distinct propositions. I intend to take a passing glance at the first four. I will leave the fuller exposure of their fallacies to his Edinburgh friends, whose philosophic

and literary attainments will enable them to do more ample justice to the subject. I will, however, direct your attention at some length to the fifth proposition, both on account of its importance, and also from the circumstance, that its consideration embraces all the practical bearings of the subject.

The first proposition is as follows:—"*That little reliance can be placed on the experience of those who, like Cullen and Gregory, were unacquainted with the nature of, and the mode of detecting, internal inflammation.*"

The total want of precision, and logical arrangement exhibited in this proposition becomes evident on the merest glance. In the premises we have two essentially distinct questions. What may be affirmed of the one, will not necessarily apply to the other; therefore, the deduction of any conclusion is entirely precluded. Let us reduce the argument to a syllogism.

By supplying the major premise, which Dr. Bennett suppresses, his argument stands thus:

Little reliance can be placed on the experience of those who are unacquainted with the nature of, and modes of detecting, internal inflammation;
Cullen, Gregory, and those like them, were so unacquainted, etc.:
Therefore, little reliance can be placed on their experience.

We see, therefore, that the major premise includes two distinct subjects—1st, The *nature of inflammation;* and 2d, The *mode of detecting* inflammation—propositions essentially different so far as the argument is concerned. In order to test their truth, they must be considered separately. Thus—

1st. Little reliance can be placed on the experience of those who are unacquainted with the nature of inflammation;
Cullen and Gregory, and others, were so unacquainted;
Therefore, little reliance can be placed on their experience.

I have to remark, in the first place, that it would have been of importance had Dr. Bennett defined the meaning which he attaches to the term "nature," the ambiguity of which is great. I presume, however, that he uses it as synonymous with pathology. At all events I will interpret it in this way. Now, I hold we cannot admit the truth of the major premise. To do so would be to ignore all medical experience. I would ask, when, and by whom, has a correct and well-defined knowledge of the pathology of inflammation been established? Such knowledge, though most desirable, is yet by no means necessary to enable us to form a correct opinion of *the effects of the remedies employed in the treatment of inflammation.* And this constitutes the meaning of the term "experience," contained in Dr. B.'s proposition.

I deem it would be a waste of time to enter into arguments to prove the absurdity of maintaining, that a man's experience of the value of a remedy in a disease is unworthy of reliance, unless he is acquainted with the nature of the disease. On this principle, Dr. Bennett's own experience must be viewed as of no importance, because it may be affirmed with great justice, that he is unacquainted with the "nature" of inflammation. He tells us that inflammation "*is an exudation of the normal*

liquor sanguinis," a definition of the disease opposed to correct notions of its pathology. Every tyro in medicine will tell Dr. Bennet, that *this exudation is the effect of inflammation,* but not the disease itself.

I think I have said sufficient to show that the major premise is perfectly untenable, and of course, though the minor should happen to be correct, yet the conclusion becomes invalidated.

It would not be doing justice to the memory of Cullen and Gregory to pass the matter over in this way. Any one who has read the works of Cullen must be well aware that he has described exudation of serum and blood as one of the most frequent effects of inflammation: "This, indeed, seems to be the most common termination of pneumonic inflammation, when it ends fatally; for upon dissection of almost every person dead of the disease, it has appeared that such an effusion had happened. You will find this matter sufficiently established by Morgagni, Lieutaud, and all later dissectors; and with this influence on our practice, that the great point of view in this inflammation is to obviate this effusion, which, when it has taken place to any considerable degree, may be considered as incurable."*

This quotation is sufficient to prove that Cullen and others were acquainted with the nature of inflammation, as defined by Dr. Bennett, viz: exudation of liquor sanguinis; nay, further, that they understood the pathology of the disease much better, inasmuch as they very correctly regarded the effusion as an effect of inflammation.

We therefore find that both premises of the first part of Dr. Bennett's proposition are inconsistent with fact.

2d. Let us next advert to the second question. It will stand thus:

Little reliance can be placed on the experience of those who are unacquainted with the mode of detecting internal inflammations:
Cullen, Gregory, etc., were so unacquainted, etc.;
Therefore little reliance can be placed on their experience.

I have, in the first place, to point out the want of a proper definition of a very important term in the premises, viz: "*mode*"—a very ambiguous phrase. Modes change in every age, and it does not follow that because the mode of detecting internal inflammation should be different in the present day from that employed by Cullen and Gregory, therefore they could not detect such diseases. But let us examine the proof that Dr. Bennett adduces to support his position. He attempts to show that internal inflammation could not be detected by the symptoms, without the assistance of physical signs, and that often a mistaken diagnosis resulted. Hence we perceive, that by the term "mode," Dr. Bennett means physical signs, or auscultation and percussion; and, as a matter of course, these "*modes* only apply to inflammations of the thoracic viscera; and not to internal inflammations in general. We must not forget that his proposition is stated in universal terms, and when we find that he only adduces in its support a limited number of particulars, even admitting their validity, the illogical character of the whole becomes obvious. I repeat, that though we should grant that he has established his assertions concerning the inability of Cullen and others

*Works of Cullen, by Dr. Thompson, edit. 1827, vol. ii., p. 57.

to detect internal pulmonic inflammations, yet this would not authorize him to infer the same of all internal inflammations. Excluding diseases of the chest, it is contrary to fact that we possess any mode of detecting inflammations superior to that which was practised by Cullen and Gregory. Dr. Bennett, therefore has failed to adduce any grounds for the admission of the major premise.

He is equally unfortunate in his attempts to prove the minor; because it is also opposed to fact, that Cullen and Gregory were unable to diagnose any case of pulmonary inflammation. A careful perusal of the writings of Cullen and his successors will convince every candid inquirer that they were perfectly competent to detect the more severe and acute forms of pulmonary inflammation. At the same time we cannot doubt but that many slight cases, such as Dr. Alison describes, were neither diagnosed nor treated as pneumonia. Their experience, however, I hold, is as fully entitled to our confidence as that of Dr. Bennett, at least so far as is essential to the discussion of this question.

The old proverb, that "those who live in glass houses should not throw stones," is very applicable to Dr. Bennett. If his first proposition be correct, then his own experience must be unworthy of confidence. In the thirteenth volume of the Edinburgh Monthly Journal, you will find that, in a clinical lecture, he brings under the notice of his students the difficulties which attend the diagnosis of pneumonia, "notwithstanding all the aid furnished us by a careful study of functional symptoms, combined with physical signs." He then narrates three cases, in one of which he diagnosed *pneumonia;* the patient died, and the post-mortem revealed *acute nephritis and hydrothorax.* In the second case pneumonia was also diagnosed. The patient died; on inspection, the scalpel could reveal no lesion to authorize the conclusion that inflammation of the lungs existed, but after a little special pleading as to the indications of certain microscopic appearances, he asserts that the case "*was* one of pneumonia." Most pathologists would have come to a contrary conclusion. The third case was diagnosed as *phthisis.* The patient died, and the post mortem failed to detect any tubercle, but *chronic pneumonia.* In consequence of such grave mistakes, I hold that Dr. Bennett is placed exactly in the position into which he has endeavored to fix Cullen and Gregory; and by a parity of reasoning, his experience is equally unworthy of confidence.

Proposition 2.—"That inflammation is the same now as it has ever been, and that the analogy sought to be established between it and the varying types of fever is fallacious."

In this proposition we have another question very ingeniously substituted for the one at issue. The subject for discussion is not whether inflammation *is the same* now as it has ever been, but whether *its type* or character has undergone important modifications. This is the real point at isuse. Dr. Bennett's reasoning is therefore lost on the mere man of straw that he has substituted. No person disputes that inflammation, in its "*essential nature,*" is unchanged; but few men of experience will deny, on the other hand, that the *type* has changed during the last half century. Dr. Bennett does not dispute the changes which occur in the

types of typhus and other fevers, but this he explains as resulting from variations in the intensity, or the nature of the exciting cause; and hence, he infers, the fact cannot be applied to explanation of the changes which Dr. Alison maintains have taken place in the types of inflammation. This is merely another attempt to substitute a different question in place of the one under consideration. Dr. Bennett's explanation, whether right or wrong, has nothing to do with the argument. It is an indisputable fact, that the types of typhus, and of all the exanthematous fevers do change—the essential nature, however, of these diseases remaining the same. It is of importance to remember that this change of type consists in variations from a sthenic to an asthenic condition. This *fact* has been ascertained by careful observation and experience; it has not resulted from any process of reasoning. With regard to inflammations, the change of their type consists in a departure from a sthenic to an asthenic state, and has also been ascertained by observation and experience. We are therefore quite justified to illustrate the one class of diseases by analogies drawn from the other. I say to illustrate, as the most ample *evidence of the fact abounds* in the recorded observations of almost every writer on the subject. We have Sauvages, Cullen, Dr. Mason Good, Dr. Bossack, and a long list of other excellent observers, describing an asthenic pneumonia—no doubt under different names, such P. maligna, P. notha, P. typhoides, P. typhus, asthenic, etc. Doubtless Cullen, and some other writers, regarded this form as rare—as constituting exceptional cases. It has, however, fallen to the lot of Dr. Alison to observe, that, at the present day, cases of the asthenic type predominate greatly over those of a sthenic character; whereas, at an earlier period of his career, this was not the case. Are we, I ask, to deny this, because Dr. Bennett's observations have been much less extensive?

I appeal to the experience of those who sit around this table, if their own observations do not fully accord with the statements of Dr. Alison. I appeal to you, if you have not found the same change taking place in inflammatory diseases of the brain, of the liver, peritoneum, and other internal organs, as well as of the lungs. From my own observation, I am most thoroughly convinced that a very important change has taken place — a change demanding a corresponding modification of the antiphlogistic treatment, but affording no argument against its previous extensive employment, nor its judicious use at the present day.

Proposition 5.—"That the principles on which blood-letting and antiphlogistic remedies have hitherto been practised, are opposed to a sound pathology."

Though the truth of this proposition were admitted, yet we would not be entitled to conclude that antiphlogistic treatment was improper. In fact, the real question at issue is completely evaded, and another of a totally different character substituted. The discussion is not regarding the *principles* on which bleeding and antiphlogistics have been practised, but respecting the *value* of these remedies in the treatment of inflammations. The solution of this question must be determined by clinical experience and observation.

(*To be continued.*)

Mortality Statistics of New Orleans, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans estimated at from 150,000 to 200,000

Time.	Total Deaths.	Children under 20 years.	United States.	Foreign.
January, (four weeks)	352	170	204	148
February, (two weeks)	185	95	131	54

Principal Diseases.	3d to 31st January.	1st to 14th February.
Still-Born	32	12
Tris. Nascent	22	4
Cholera Infant	4	4
Infant. Convulsions	19	11
Croup	13	7
Scarlatina	11	4
Rubeola	..	..
Variola	14	6
Diarrhœa and Dysentery	14	2
Gas. Enteritis	0	2
Inflammation of Liver	4	3
Inflammation of Lungs	13	10
Consumption	41	26
Apoplexy	8	4
Congestion of Brain	3	4
Fever Typhoid	6	7
Fever Miasmatic	7	0
Fever Yellow	1	0

The following Table from the Report of the Board of Health, may prove interesting as a Summary of the Mortality of the City for the past two years and eight months.

Table A.*—Total Mortality for Three Years.

Last 8 months of 1855.	1856.	1857
Whole mortality 7 705.	Whole mortality. 5,689.	Whole mortality, 6,067

Deaths from	1855.	1856.	1857.
Cholera	883	46	29
Fever, Yellow	2,670	74	199
Fever, Scarlet	19	26	86
Trismus Nascentium	143	226	188
Still-Born	199	346	358
Consumption	652	728	661

Years.	1855.	1856.	1857.	
Men	3,231	1,836	1,753	* This Table is
Women	1,400	904	1,010	complete up to
Boys	1,676	1,521	1,761	27th of December
Girls	1,414	1,337	1,533	for the year 1857.
Colored of both Sexes	720	1.610	1,096	

(Eight months of 1855.)	1855.	1856.	1857.
Ages.	Mortality.		
1 to 5 years, inclusive	2,644	2,556	2,661
20 to 30 years	2,060	794	709
30 to 40 years	1,195	654	756

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, United States Army, New Orleans.* New Orleans, La., Lat. 29 deg. 57 min. 30 sec. North; Long. 90 deg. West; Altitude of Barometer above the level of the sea, 35 feet.

1858.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
MONTHS.	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
January..	9 P.M. 8th. 30.400	2 P.M. 31st. 29.828	30.182+	9 P.M. 14th. 72	Several. 57	62.79	2 P. M. 14th. 73	7 A.M. 20th. 50	59.96
February.	7 A.M. 16th. 30.352	2 P. M. 12th. 29.772		Several. 68	Several. 52		9 P.M. 12th 70	7, A.M. 5th. 41	

1858.	HYGROMETER.			PREVAILING WINDS.	RAIN.	
MONTHS.	Max.	Min'm	Mean		Days.	Quant'y
January..	2 P.M. 14th 70	7 A.M. 30th 47	58.01	Northeast, East and Southeast.	12	3.71 in.
February.	Several. 69	7 A.M. 5th 40		Northeast, East and Northwest.	6	3.53 in.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER FOR THE YEAR 1857.

NEW ORLEANS, LA.—Latitude 29 deg. 57 min. 30 sec. North; Longitude 90° West; Altitude of the Barometer above the level of the sea 35 feet.—Taken from Observations at the Medical Purveying Office, U. S. A., at New Orleans, at 7 A. M., and 2 P. M., and 9 P. M., of each day.

1857.	Barometer.			Thermometer Attached.			Thermometer.		
Months	Max. of Ob.	Min of Ob.	Mean	Maxi'm	Mini'm	Mean.	Maxi'm	Mini'm.	Mean
Jan.	30.478 7 8	29.934 2 30	30.299	68° 9 28	44° 2 19	57.55	68° 2 2	28° 7 19	50.56
Feb.....	30.696 7 10	29.950 2 7	30.255	76 9 26	55 2 10	67.72	79 9 20	42 7 9	54.98
March..	30.374 7 7	29.930 7 11	30.195	75 s.	55 s.	66.08	78 2 29	43 7 8	52.35
April...	30.260 7 6	29.876 9 30	30.104	77 9 18	59 7 13	68.44	79 2 18	47 7 13	64.96
May....	30.272 7 21	29.794 2 3	30.076	84 2 15	67 7 4	74.70	85 s.	63 7 19	74.16
June ...	30.220 7 9	29.850 7 24	30.093	86 9 11	74 9 23	80.79	88 2 11	73 9 23	80.09
July ...	30.252 7 7	29.972 9 16	30.135	88 9 15	77 7 5	85.76	88 2 22	74 7 5	81.80
August.	30.270 7 14	30.000 7 11	30.156	86 2 19	79 2 11	82.27	87 2 27	68 s.	81.80
Sept....	30.350 7 30	30.076 9 21	30.184	86 2 18	72 7 25	80.17	87 2 17	70 7 23	80.15
Oct.	30.380 7 17	29.856 2 8	30.138	80 2 4	63 7 30	72.35	79 2 4	57 s.	69.77
Nov. ...	30.500 7 26	29.810 2 7	30.159	82 9 7	55 s.	66.95	80 2 7	39 7 20	61.10
Dec.....	30.501 7 11	29.900 2 24	30.191	77 7 9	53 s.	63.01	78 2 8	43 7 26	58.13
Y'ly Mn	30.379	29.912	30.165	80° 41	62° 75	72° 15	81° 33	54° 7 5	69.15

1857.	Hygrometer				Weather.		Rain.	
Months	Maximum.	Minimum.	Mean	Prevailing Wiuds.	No. fair Days.	No. cldy Days.	No. days	Quantity.
January	67° s.	26° 7 19	48.36	N E; E and W	8.33	22.66	7	2.68
Feb	73 2 26	38 7 9	62.23	S E; N and E	19.66	8.33	7	1.97
March.	74 2 29	41 7 8	58.90	N E and S E	24.33	6.66	7	2.86
April..	74 2 18	44 7 6	61.36	N; N W and S E	22.	8.	8	1.73
May...	79 2 15	58 7 20	69.57	S E; E and N W	21.66	9.33	8	7.33
June ...	81 9 12	66 7 19	74.95	N W; S E and S W	23.33	6.66	6	2.90
July....	81 2 16	68 7 4	76.99	S E; E and S W	20.	11.	16	5.86
August	82 2 19	71 7 30	77.97	S E; S W and E	16.	15.	19	4.64
Sept....	81 s.	60 7 30	75.24	N E; N and E	25.66	4.33	5	2.55
October	76 2 4	55 s.	66.09	N; N E and E	18.	13.	7	5.01
Nov	76 2 6	35 7 20	57.95	N; E and N W	20.	10.	10	3.05
Dec.....	75 2 8	42 7 26	55.95	E; N W and E	15.33	15.66	12	5.10
Y'ly Mn	76° 58	50.33	65.47		234.33	130.66		45.68

NOTE.—The first column of figures on the right of each observation is intended to indicate the hour, and the second column of figures the day of the month on which that observation occurred; and "s." in the same situation indicates that more than one observation of the same took place during the month. For instance, under the Barometer in January, you will find the maximum 34.378 inches occurred at 7 o'clock A. M., on the 8th day of the month; and the maximum of the Thermometer for January 68 deg. at 2 o'clock, P. M., on the second day of the month

Remarks on the Barometer.—The greatest observed height during the year was on February 10th, at 7 A. M., and the greatest depression occurred on May 3d, at 2 P. M. Therefore the greatest range during the year was 902 parts of 1 inch. The greatest monthly mean was in January, 30.299; and the lowest was in May, 30.076; and the greatest quantity of rain fell in May, 7.33 inches.

Remarks on the Thermometer.—The greatest observed height or heat during the year was 88 deg. which occurred at 2 P. M. the 11th June, and the 22 July; and the lowest or coldest was 28 deg., at 7 A. M., January 19th. Therefore the greatest range in the year was 60 deg. The greatest mean heat was during the months of July and August, both 81 deg. 80; and the coldest month January, 50 deg. 56.

THOMPSON HARRISON, Clerk.

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL

FOR MAY, 1858.

ORIGINAL COMMUNICATIONS.

ART. I.—*Practical Remarks on the Evidences of Pregnancy. (Continued from page* 20, *Vol. XV.)* By DR. J. E. THOMPSON, Roseville, Arkansas.

Compound Pregnancy.—Compound Pregnancy is that state of utero-gestation where more than one fœtus is contained *in utero.* Woman is constitutionally uniparient; though, like other uniparient animals, sometimes producing more than one offspring at a birth; and when the gestation is plural, twins are by far the most frequent. It is stated that the average of twin cases varies considerably in different sections of the world; though there is quite a discrepancy in the different statistical accounts given by different individuals even in the same country. This, probably, may be accounted for, in part at least, *by the influences of climate and race upon the fecundity of the human species.*

Thus, Dr. Denman found one case of twins in ninety-five labors in the Middlesex Hospital, London; in the "London Practice of Midwifery," it is calculated at one in every forty-eight; Dr. Conquest found one in ninety; Dr. Gooch, one in seventy; Dr. Blundell found, from the statistical accounts forwarded to Government, in 1801, one in every sixty-five cases to be twins; Bland of London, and Boer, of Vienna, found the average one in eighty; in the Maternity of Paris, one was met with in eighty-eight; in the *Maison d'Accouchemens,* one in ninety-one; Madame Boivin met with only one in every one hundred and thirty-two; Dr. Ramsbotham found, out of forty-eight thousand nine hundred and ninety-six cases that occurred in the Royal Maternity Charity of Paris, that from January 1, 1828, to December 31, 1850, there were 536 cases of twins—one in nearly every ninety-one and a half, while out of the whole number, only three were triplets; Dr. Collins found, out of one

hundred and twenty-nine thousand one hundred and seventy-two deliveries in the Dublin Lying-in Hospital, there were two thousand and sixty-two cases of twins—one in about every sixty-two labors, while only twenty-nine were triplets—one in four thousand four hundred and fifty, and only one case of quadruplets was found out of the whole number of cases. Dr. Dewees averages the frequency in the United States, as one in 75; Dr. Arneil found the average about the same; Dr. Moor found it one in seventy-six; Dr. Dewees thinks "that if the different tables can be relied on, it is certain that there are conditions and circumstances which give rise to more double births in America than in Europe." (Syst. Mid. ¶ 1321.) While on the other hand, Dr. Collins remarks, "It is singular that in Ireland the proportional number of women giving birth to twins is nearly a third greater than in any other country from which I have been able to obtain authentic records." (Prac. Treat. on Mid., p. 309.) Dr. Denman thinks that climate and the state or degree of civilization have their influence over the fecundity of human beings; of this, however, we have not sufficient facts to prove it beyond a doubt, although we are well aware that some of the lower animals, for instance, the *sow*, farrow more young at a birth, and also more frequently, when domesticated than when in a state of nature.

Preternatural fecundity is, to a certain extent, hereditary in the human female. I know an instance of a family of negroes belonging to a lady in Franklin county, Arkansas, where the mother and all her daughters produced plural births frequently; the mother produced triplets three times; one of the daughters produced both triplets and twins; one produced twelve births, three of which were twins; the remainder produced twins, though none of them consecutively. Dr. Dewees states that "some facts within his own knowledge would seem to countenance this supposition; but they are not sufficiently numerous and strong to confirm it." However, he regards it as being constitutional in some females at least, for he speaks of a woman whom he knew that five times produced twins, and never brought a single child; and another who twice brought forth twins, though not consecutively.

Gottlob speaks of a woman "who blessed her husband with eleven children at three births." (Elliotson's Notes to Blumenbach, p. 487.) When there is more than one fœtus in utero, each is generally smaller than in single births, and in proportion to the number will their size be less. Dr. Joseph Clark's estimate of the weight of twins is twelve pounds and a half to the pair. I have often noticed that even in twin gestation, one fœtus at birth is sensibly smaller than the other. When the uterus contains more than two fœtuses, the full term of utero-

gestation is rarely completed, consequently, the children are seldom reared. In comparison with twin cases, triplets are rare; quadruplets are still less frequently met with; yet there are cases on record of five, six and even more, being expelled at one birth. There is a preparation in the Museum of the College of Surgeons of London, of five fœtuses, which were expelled at one birth under the care of the late Dr. Hull, of Manchester; they had advanced to five months intra-uterine age. (Ramsbotham's Syst. Obstet., p. 462.) Chambon relates a case of five at one birth that lived long enough to be baptized. (Campbell's Mid., p. 291.) Two similar cases are related in the *Gentlemen's Magazine* for 1736–39, the latter occurring at Wells, Somersetshire. Two cases are spoken of in the *Gortshore Philosophical Transactions* for 1787; the first occurred in Upper Saxony, and the second near Prague, in Bohemia. In the *British and Foreign Medical Review* for 1839, a notice is given of a woman at Naples being delivered of five children at seven months intra-uterine age; in the *Dublin Medical Journal*, for January, 1840, there is an account given by Dr. Kennedy of his having shown to the Dublin Pathological Society five ova of between two and three months intra-uterine age, which were expelled at once under the care of Dr. Thwaites. They were all male children. In the *London Practice of Midwifery* it is stated that Dr. Osborn met with six distinct ova thrown off at one abortion. Mr. Wardleworth gives, in the *Medical Gazette*, for June 11th, 1841, a case of five children at one birth, in which three of the umbilical cords arose from the placentæ by one common origin, and the other two cords by another. The three umbilical veins united to form one, and it would seem that there were but five umbilical arteries.

Paré tells us that in his time, the wife of the Lord of Maldemeure, in the parish of Sceaux, near Chambelly, produced six children at a birth, after which she died; and that the present Lord of Maldemeure was the only surviving one. (*Lib.* xxv, *cap.* 3.) The wife of a cannon-founder at Luginski, in Russia, was delivered of five girls on the 22d of May, 1836, of whom four were living and likely to do well. (*Satirist*, August 7, 1836.) The *Giornale del Due Sicilie*, states that a woman was safely delivered, on June 21st, 1838, of a boy and four girls, all of whom died at the expiration of half an hour. (*Times*, July 23d, 1838.) The wife of a land proprietor at Altruitweida, near Mitweida, in Saxony, was recently delivered of five daughters, who, though perfect in their conformation, died in about half an hour after their birth. (*Ibid.*, August 29th, 1838.) A woman in New Hampshire was delivered of five living children at a birth, in the beginning of the year 1841. (*Ibid.* June 4th, 1841.)

Mrs. Manger, of St. Owen, Jersey, had five daughters at a birth, at six months intra-uterine age, on the 20th of May, 1840. Mrs. Wright, the wife of a shoe-maker, No. 11 Rochester-terrace, Rochester Row, Westminster, was delivered of five boys at a birth, of seven months intra-uterine age, on June 14th, 1849. The fœtuses were only *seven inches long*. (Ramsbotham's Syst. Obstet., p. 626.) On the 20th Jan., 1856, Mrs. C., of Franklin county, Arkansas, was delivered of three well formed male children, of five pounds weight each, with each umbilical cord arising from one placenta. One of the children was still-born, and the other two lived about four days. This lady had twins in 1853. She is at this time pregnant, being the fourth time.

In June, 1855, Mrs. M., of Scott county, Arkansas, produced triplets, at the full period of utero-gestation. Each child had its own placenta and umbilical cord. The most miraculous instance of supposed fecundity in a human female is that of the Countess Henneberg, recorded on a marble tablet, which still is, or at least was, in the church of Lonsdunen, near Leyden. The monument bears the following inscription :—

> *"En tibi monstrosum nimis et memorabile factum,*
> *Quale nec a mundi conditione datum,*
> *Ostendam."*

After which, follows an account of the miracle, to-wit: "That Margaret, wife of Hennan, Earl of Henneberg, and daughter of Florence, the fourth Earl of Holland and Zealand, being about 40 years old, upon Easter-day, 1276, at 9, A. M., was brought to bed of 365 children, all of which were baptized in two brazen basins by Guido Suffragan of Utrecht. The males, how many so ever there be of them, were christened John, and all the daughters Elizabeth, who, altogether with their mother, died on the same day, and with their mother lie buried in this church of Lousdunen." This supernatural infliction is accounted for on the principal of retributive justice; for we are informed that the Countess, being solicited for alms by a poor woman who was carrying twins, shook her off with contempt, declaring that she could not have them by one father; whereupon, the poor woman prayed to God to send her as many children as there were days in the whole year; "which came to pass as is briefly recorded in this table, for perpetual recollection, testified as well by ancient manuscripts as by many printed chronicles." (Evelyn's Discourse on Medals, fol. 1697, p. 267.) These 365 children were no doubt merely a collection of watery cysts, which were looked upon as ova, as is clearly shown from the declaration that "they were baptized in two brazen basins." The death of this noble lady was most probably caused from flooding.

Twins may possibly proceed from one ovarium, or the rudiments of one fœtus may be furnished by each gland. When the conception, however, is more than duplex, it is clear that one ovary must supply two; for no instance has yet been met with where these organs were in excess. It is commonly supposed that twins are the result of one connection; but it appears by no means impossible that a second impregnation may take place soon after a former one has occurred, especially where the second *coitus* happens prior to the uterus becoming lined with the secretion afterwards converted into the deciduous membrane; or until its mouth is plugged with that viscid mucus which divides its cavity from that of the vagina, and which, after its formation, would entirely prevent the emission of the seminal fluid *in coitu*. A case is recorded of a white woman at Charleston, S. C., who was delivered in 1714, of two children—one black and the other white. Upon being interrogated, she confessed that on a particular day, immediately after her husband had left his bed, a negro entered her room, and, threatening to murder her if she did not consent, forced her to submit to his will. Dr. Mosely records another instance which occurred within his own knowledge, on the Shotwood Estate, Jamaica. A negro woman brought forth two children at one birth, one black and the other a mulatto. On being questioned, she admitted that a white man belonging to the estate came into her hut one morning, before she was up, and she suffered his embraces almost immediately after her black husband had left her. (Tropical Diseases, p. 111.) A similar case is related by M. deBoillon in the *Bulletin de la Faculté et de la Société de Médecine*, for 1821. Dr. Dewees relates the case of a servant in Montgomery county, Pennsylvania, who was delivered of a black and a white child at one birth. When it was ascertained that she was pregnant, both a black and a white man disappeared from the neighborhood. (Philadelphia Medical Museum, vol. 1.) Dr. Elliotson in his *Notes to Blumenbach*, p. 485, records the following instance related to them by Mr. Blackaller, of Weybridge:

"A white woman of very loose character left her husband, and sometime afterwards returned, pregnant, to the parish, and was delivered in the work-house of twins, one of which was born of a darker color than is usual for infants of the negroes in the West Indies to be; the other had all the appearance common to white children." I know a white girl of loose character, in Hickory county, Missouri, who had a white child and a mulatto at the same birth. She confessed that a negro had connection with her immediately after she had received the embraces of a white man.

With the knowledge, then, of such occurrences, we are warranted in

believing that twins might possibly be the result of two separate *coitions*, if only a short interval intervened between them. The possibility of this occurrence did not escape the ancients, for we read in Pliny (*Lib.* vii, *cap.* xi:)

"*Ubi paululum temporis inter duos conceptus intercessit, utrumque* [*puerperium*] *perfertur. Ut in eâ apparuit quæ gemino partu, alterum marito similem alterumque adulterio genuit. Item in Procounesiâ ancillâ, quæ ejusdem diei coitu, alterum domino similem, alterum procuratori ejus.*"

In regard to multiparient animals, whose uteri are cornuated, it is a well known fact that more than one connection may be fruitful, if they follow each other in a short time. Thus, if a bitch, while in heat, receives two or three dogs of various species in succession, she may bring forth mongrel puppies of different sorts, some partaking of the character of one dog, and others of the rest.

As twins are by far the most frequent among plurality of births, my remarks, with reference to the *evidences of compound pregnancy*, will be confined to twin gestation; as the signs which would detect twins are, in a majority of cases, sufficient to detect triplets, or even quadruplets *in uterio*. The insufficiency of the commonly received evidences of twin gestation, is generally admitted; and the most expert and scientific accoucheurs confess themselves unable to pronounce with any calculable certainty upon the presence of two or more *fœtuses in utero* by their assistance. Dr. Ramsbotham remarks: "There are no symptoms during pregnancy which positively indicate to us that the womb contains more than one fœtus." (Syst. Obstet., p. 463.) Those generally mentioned by authors are, the greater size and earlier development of the abdominal tumor; the flattened appearance which it assumes, and its divisional line; inequalities on its surface, peculiar sensations, and a feeling of great weight and distention in the abdomen, together with œdematous extremities. The motions of the fœtus being felt in different parts of the uterus is relied on by some, while others contend that they are little or scarcely at all perceptible, from the bulk of the fœtuses so filling up the uterine cavity, so as to prevent their moving. It is also said that females carrying two or more fœtuses *in utero*, seldom go to the full term of utero-gestation, and that labor is usually slower in its progress. However, all these signs are seldom available, and are liable to prove abundantly deceptions.

The most likely method that will enable us to detect the existence of twins *in utero*, among the *tangible evidences*, are those ascertained by manipulation of the abdominal tumor, and those by *le toucher*, or vaginal examination. We shall find by a close manual examination, that *bal-*

lottement, or the passive motions of the fœtus, are much less than in single pregnancy, and in fact they are often not at all evident; we may sometimes succeed in moving one fœtus in the uterus by gently pressing on it, while we will notice another bulky body remaining stationary. Searching for the tumor caused by the head of one fœtus, and then seeking for another will often assist us very much in finding twins. Though there may be cases so nearly simulating these tumors caused by the fœtal heads, that we may be led into gross error. The fœtal pulsation, however, will afford us more assistance, though in some cases, even this may lead us into error; yet, in others, it furnishes us with such satisfactory proofs of the existence of plural fœtation as amounts to actual demonstration. When the fœtal pulsation is audible over a greater extent of surface than in simple pregnancy, and particularly, if it be detected at two points of the uterine tumor distinct, and at a considerable distance from each other, we may strongly suspect the existence of a second fœtus *in utero*. But, from what we have heretofore said while treating of the fœtal heart and funis, it will be recollected that such pulsations may exist under certain circumstances when there is but one fœtus *in utero*. What, then, shall be our guide?

The sounds being heard at each point, indicating the characters observed in the immediate vicinity of the heart, and the pulsations heard, not corresponding in time and frequency—for instance, where the hearts of the fœtuses are situated — one over the pubis, and the other to the right side and near the fundus. The pulsation of the fœtal heart will be distinctly perceivable in the left side, particularly between the crest of the left ilium and the umbilicus, extending up the side; while there will be a similar pulsation audible in the right side, and above the umbilicus, not at all synchronous with the pulsation on the left side; though, the pulsations extend towards the umbilicus from each of the points where it is most distinctly heard, becoming, as they approach nearer the umbilicus, less distinct — in fact, there is a spot at the centre where it is not at all perceivable. Yet, we must guard against being led into error; for, though there may be two fœtal hearts distinctly audible, they may be so nearly synchronous with each other as to deprive us of the assistance derived from their comparison in this respect, which is certainly a most important criterion. Again, we may be led to suppose twin gestation from the momentary change not uncommonly observed to occur in the frequency of the fœtal heart's action, when actually there is but one. One fœtus may also be so situated in the uterus as to place the region of the heart at such a distance from the surface as to render the peculiar signs of the heart's immediate vicinity little, if at all, observable.

The evidences of twin gestation elicited from the placental *souffle* are still more fallacious. In simple pregnancy the *souffle* is audible over a considerable extent of surface, yet it is more sonorous and distinct at one spot. In twin gestation it is heard generally over a greater extent of surface; it may either be continuous, or if the placenta be distinct and separated, it may be heard at distant parts of the uterine tumor, there being two sonorous spots observable, one on each side of the tumor, one above the pubis and near the crest of the left ilium, and the other to the right, and above the umbilicus in the region of the fundus.

Every accoucher knows how easy it is to be led into error, however expert we may be in the use of the stethoscope; yet, we are well aware that there are many cases that do occur in which the proofs of the existence of twins *in utero*, by the assistance of auscultation, are quite palpable.

Dr. Kennedy is said to have, by the assistance of the stethoscope, frequently pronounced with accuracy upon the number of kittens a cat, or of pups a bitch would produce. Dr. J. E. Taylor, in view of the great uncertainty in the diagnosis of twin gestation, recommends "that two individuals should auscult at the same time, and should two fœtal pulsations be audible, there could be no doubt that there existed twins *in utero*. However, should the pulsation be audible in the left inguinal region, and another in the right hypogastrium, beating the same in frequency and strength, or should there be a strong pulsation in the right side, and a feeble one in the left, and each of these cases having an intermediate space free from sounds, and the other evidences being equal, the existence of twin gestation would be sufficiently conclusive to warrant a positive diagnosis. After the waters have been discharged which will cause the sounds to become more distinct, as well as to elicit other signs, our diagnosis will be greatly strengthened; though, as Dr. Kennedy remarks, "there can be no advantage gained previous to the delivery, or during the delivery, of the first child."

But the circumstances under which auscultation will be most likely to afford us the most advantage are, in such cases where one fœtus has been expelled, and it remains a matter of doubt whether there exists a second fœtus *in utero* or not. It sometimes happens that the second fœtus remains *in utero* not only for hours, but even for days, weeks or months. The following case came under my observation in Missouri: In April, 1856, Mrs. Burns, of Bates county, Missouri, produced four still-born children; the first, a girl, was born on Saturday, the 10th; in twenty-four hours another girl was expelled, and in the same length of time thereafter two boys were born. They were of the full time of intra-

uterine age, only weighing 3½ pounds each. Each child had a separate placenta and umbilical cord. Up to March, 1857, she was doing well, and was again pregnant.

In the *St. Louis Medical and Surgical Journal* (vol. xv, p. 288,) a singular case is recorded by Dr. A. S. McGregor, of Gasconade Ferry, Missouri, "of a Mrs. G., of Gasconade county, who was delivered of a still-born child on the 10th of August, 1856; twenty-one days after this, she gave birth to a second, and in the same length of time thereafter, of a third. The last two lived about six hours each. The mother did well, and is again pregnant."

Dr. Ryan mentions a case of a lady who traveled thirty miles after the birth of her child; as she complained of much swelling in the abdomen, an examination was instituted, when another child was found." (London Med. and Sur. Journal, July, 1829.)

Dr. Ramsbotham met with the case of a lady who was delivered of a living child, and at the same time with the placenta, there was expelled a dead and somewhat flattened fœtus of about four months intra-uterine age, attached by the funis to an after-birth of the appropriate size. He also met with another case in which a dead fœtus of five months intra-uterine age was expelled about thirty hours after the birth of a mature infant." (Syst. Obstet., p. 469.)

Dr. Maton records a case in the *Transactions of the College of Physicians of London*, (vol. iv, p. 161,) of a lady who was delivered on the 12th of November, 1807, of a strong and healthy boy, and on the 2d of February, 1808, (not quite three calender months,) gave birth to another boy at full time. Dr. Granville thought that the first child in the case of Dr. Maton's was prematurely expelled, and the other carried to the full period; but this is successfully refuted by Dr. Maton in Paris and Fonblanque's Medical Jurisprudence, (vol. i, p. 264.)

A very extraordinary case is recorded by Dr. Desgranges, of Lyons, France. He reports that the wife of Raymond Villiers, of that city, married at twenty-two years of age, became pregnant five years afterward, and on the 20th of May, 1799, was delivered at the seventh month. In a month after this time she conceived again; and seven months after brought forth a living child. This delivery, however, was not followed by the usual changes, such as milk, lochia, diminution of the abdomen, etc. Two surgeons visited her, but being unable to account for these circumstances, they called Dr. Desgranges in consultation, and he gave as his opinion that there was another fœtus in the uterus. Three weeks after this opinion was given, the movements of the child confirmed it; *and five months and sixteen days after the birth of the first*

infant Madame Villiers *gave birth to another living daughter!* The milk was now secreted, and the mother enabled to nurse. (Fodéré, vol. i, p. 484.)

M. Velpeau, in his *Elements of Midwifery*, relates the case of a Madame Biguad, who, on the 30th of April, 1748, produced a living male child, and, on the 17th of September following, another infant, which was also alive and healthy. He also speaks of a woman named Artes, who, in 1796, gave birth to a child at the full term, and five months after, to another at its full time.

M. Cassan gives a case that occurred to Madame Boivin, formerly chief midwife to the Maternity Charity of Paris. On the 15th of March, 1810, a woman æt. 40, gave birth to a female infant of four pounds weight. As the abdomen still remained large, Mad. Boivin introduced her hand into the uterus, but found nothing. The examination, however, led her to suspect that there was another fœtus, either extra-uterine or contained in a second cavity of the uterus. So, on the 12th of May following, she was delivered of a second girl not weighing more than three pounds, feeble, and scarcely able to support respiration. (Velpeau's Mid., p. 193.)

In the *Transactions of the Veterinary Medical Association* for 1849, (p. 299,) there is a case of superfœtation in the *ewe*, recorded. One lamb was born on the 12th of March, and the other on the 9th of April —both alive and well, and of full size. The ewe gave no milk till after the second was born.

The only rational way of accounting for these cases is, on the supposition of a *double uterus*, or one divided by a longitudinal septum, that one chamber becomes impregnated at first, and the other subsequently.

Prof. Lobstein, in the *London Medical and Physical Journal*, tells us that he "actually delivered a woman of two infants, *one a month after the other;* and was able to convince himself that she had *two uteri*, and to each *a distinct and separate vagina.*"

Dr. Purcell found a woman who had *a double uterus*, in one of which he found a fœtus. There was but one tube and ovary to each uterus.

Dr. Norton delivered a woman of a "*black fœtus of the eighth month, and of a white one of the fourth month.* Having attempted to extract the placenta of the first before the second was born, he discovered the passage was so narrow that it would not admit his hand; so that this woman must have possessed a *double uterus*, and to each a vagina."

There is a preparation of a *double uterus* in Prof. Rokitansky's Pathological Museum at Vienna, where each chamber presents the appearance of having been pregnant. There is also a similar one in the London

Hospital Museum, in which only one chamber has been impregnated. Another instance is given by Dr. Lee in the *Med. Chirurg. Transactions* for 1832, (vol. xvii, p. 473,) and another in the *Medical Gazette* for March 15th, 1834, (p. 898,) in which there existed two chambers, neither of which had been impregnated.

While, therefore, we admit that superfœtation is possible, yet the majority of supposed cases, we doubt not, will be found to be either the result of a *double uterus*, the expulsion of a twin at seven months, while the other is carried to full time, or even beyond, or a blighted ovum, in a double or triple conception, which has ceased to grow, and presents, when born, the appearance of a fœtus several months, it may be, younger than the others. Although such cases are comparatively rare, yet, it is wise for every accoucheur to fully prepare himself for such cases, and if any suspicion should arise of a second fœtus remaining *in utero*, we should carefully examine for it. Here, the stethoscope will prove invaluable, as by its assistance we may arrive at the true nature of the case, without subjecting our patient to an indelicate as well as painful examination, which might produce such an amount of irritation as to cause the expulsion of the second fœtus — an event which might be proper for us to prevent. It is, however, more usual for us to meet with cases in which one fœtus has died at an early period and been retained *in utero* until the other has arrived at maturity, when both are expelled simultaneously. Cases will also frequently present themselves to us, in which a second fœtus apparently continues in the uterus after delivery has been completed; especially, when considerable time has elapsed and pressure on the abdomen and bandaging has been neglected, the intestines often become rapidly distended with air, and the abdomen almost as much enlarged as it was previous to delivery, while the motions of the flatus impart to the patient the idea of the movements of a second child. But by applying the stethoscope carefully we shall find that none of the usual phenomena of pregnancy can be discovered. We shall notice, however, a distinct intestinal murmur with borborygmus over the whole abdomen, which will be quite tympanitic on percussion, emitting the *son humorique* in some parts, particularly in the epigastrium. This distention does not usually continue from the period of delivery, some time being required to allow the flatus to collect in sufficient quantities to distend the abdomen in such a degree; if, therefore, we have been with our patient during her confinement, such a mistake is not likely to occur, provided we use the precaution to bandage, which we cannot too strongly recommend, and that, not merely immediately after delivery, but as long as any unnatural distention remains, assisted by the free administration of castor oil and

oil of turpentine. The abdominal distention may, however, depend upon other causes, as, for instance, enlargement of the uterus itself, which is sometimes so considerable as to cause it to fill up the greater part of the abdominal cavity.

There is, however, a criterion between this state of *atony* of the uterus, and distention depending on the presence of a fœtus, that will seldom fail, to wit: in the former state the *uterus never rises above the umbilicus;* while in the latter *it does.*

This inordinate distention of the uterus after delivery may occur either before or after the expulsion of the placenta; in the former case it may depend upon a collection of false waters. Dr. Dunn, of Pilkington, mentions a case of this kind in the *Med. Chirurg. Transactions,* (vol. x, p. 396,) in which, after sitting by his patient four hours expecting the expulsion of a second child, he at length ruptured, as he thought, the membranes of that which remained, when a free discharge of waters took place, and on introducing his hand into the uterus he found nothing but the placenta of the child that had just been expelled.

In cases where we suspect a second child remaining in the uterus, we should never disturb the placenta of the child that has been expelled till we become perfectly satisfied that there is not another child remaining. Dr. Ingleby mentions a case where the placenta of the two first fœtuses, in a case of triplets, was removed by the accoucheur, he being ignorant that a third child remained in the uterus, which nearly deprived the patient of life, from the copious flooding that ensued. (On Uterine Hæmorrhage, p. 174.) Where dropsy coëxists with pregnancy, the abdominal distention, although considerably diminished by the birth of a child, might still so far remain as to sanction the notion that the uterus contained a second child; though fluctuation, together with the absence of the main evidences of pregnancy, will soon put this question at rest. We may, also, unless on our guard, be deceived by the presence of ovarian or other abdominal tumors. But careful stethoscopic examination will enable us in all such cases to distinguish between the presence of tumors in the uterus, abdominal distention caused by dropsy or flatus, and the presence of one or more fœtuses *in utero.*

It might not be considered out of place here to mention a curious phenomenon observed by some—i. e., *the power of the fœtus to breathe and cry in utero.* This is a circumstance, the possibility of which, one might with propriety question; yet, several writers, whom we cannot doubt for a moment, have repeatedly observed this phenomenon. Dr. Kennedy mentions the case of a lady who was delivered on the 2d of December, 1830, in the Dublin Lying-in-Hospital. During the progress

of the labor, his attention was directed to the phenomenon in question by an intelligent pupil of the hospital. And on approaching within six feet of the lying-in-bed, he distinctly heard a low moaning whine, resembling the faint and painful cry of a delicate seven months' child; this sound became more distinct the nearer he approached the patient. Still being skeptical, he applied the stethoscope, "when the fact was proven beyond a doubt, as not only the cry mentioned, but the labored respiration of the fœtus was perfectly audible." The head was found presenting, but high up in the pelvis; the parts were only partially dilated, though the membranes had been ruptured some time. This woman was not delivered for four days after the above phenomenon was noticed, and it was noticed by the pupils up to the birth of the child. (Obstet. Auscult., p. 159.)

Dr. Marinus, of Brussels, read a memoir before the *Society of Medical and Physical Sciences of Brussels*, in which he gave the history of a number of cases in which this phenomenon was noticed frequently. Dr. Lodes relates a case where the *vagitus* was heard, but the child was born dead; on dissection, the whole of the right lung, and the upper lobe of the left, was found dilated with air, the lower lobe of the left lung still being impervious to it.

The phenomena of *uterine vagitus* once being admitted, we are necessarily led to believe the child can, under certain circumstances, breathe *in utero*. The act of respiration may take place before the child is born, and the child still die, which has thus breathed before it is born. The circumstances, therefore, of only a part of the lungs being found dilated with air, may possibly be a *presumed* post mortem appearance in favor of intra-uterine respiration having taken place.

If the phenomenon in question be true, a powerful argument may be drawn against the phenomena exhibited by the lungs in cases of suspected infanticide. Hence, in a medico-legal point of view, this fact will forever put at rest the utter futility of some of the tests most relied on in infanticide.

Complicated Pregnancy.—Pregnancy may coëxist, in the same individual, with some morbid condition, which, from its either concealing or resembling pregnancy, or appearing incompatible with it, is calculated to lead the accoucheur into error, or at least render his diagnosis attended with considerable embarrassment. Such morbid condition may either exist within the uterus or external to it. The complications which depend upon a morbid state of the uterus itself, are those cases wherein we find pregnancy combined with an inordinate collection of liquor amnii, tympanites of the uterus, hydatids, dropsy of the uterine viscus, scirrhous

tumors, polypous growths, cauliflower excrescences, and ulcerations of the cervix uterii, as well as of the uterine cavity.

Of those conditions which render pregnancy complicated, which exist external to the uterus, depending upon some morbid state of the abdominal viscus, are abdominal and pelvic tumors, tympanites abdominalis, ascites, and extra-uterine conception. When either of these morbid states exist so as to render in any way our diagnosis difficult, the pregnancy is termed complicated; or, as the French have termed it, *grossesse compliquée.*

Where the secretion of the liquor amnii is increased in an inordinate degree, the fluid may also become collected without the amnion, between the membranes; however, this collection may escape from the uterus without inducing labor, which is backed by the authority of Dr. Ashwell, who remarks that, "discharges of water from the uterus are not unfrequent during gestation." (On Diseases of Females, p. 355.) But when the fluid collects within the amnion in very inordinate quantities, the fœtus often does not survive beyond the sixth or seventh month, or is very weak and feeble. This seldom occurs only in cases where a venereal taint exists in the constitution of one or both parents. This hydropic collection in the amnion most probably depends upon the death or feeble state of the fœtus, whereupon, the circulation becomes, from venereal taint, or some other cause, languid and weak, a portion of the utero-fœtal blood, which, in the healthy state of the fœtus, should have gone to nourish it, now determines to the amnion, causing it to take on an increased action, in many respects resembling that which we observe in cases of dropsy in serous cavities, resulting in an inordinate secretion of this fluid. The inordinate secretion of the liquor amnii in the uterus may be mistaken for ascites, more particularly if the symptoms of pregnancy are at all equivocal, especially where extreme abdominal distention, with fluctuation, while the motions of the fœtus are either not perceivable, or rendered so obscure as to create a doubt as to their occurrence. However, should the distention be circumscribed, and the fluctuation exist within the circumscribed tumor, we may conclude that the distention is in the uterus, and arises from an undue secretion of liquor amnii between the amnion and membranes, and within it. This would be more likely to be confounded with ovarian dropsy, as the fluctuation observed here is also obscure and circumscribed. However, we will yet remain perplexed in our diagnosis unless we call to our aid the general evidences of pregnancy, endeavoring, if possible, by auscultation, to detect the pulsation of the fœtal heart, and placental *souffle* — and if the fœtus be alive, whatever difficulty there may be in detecting the

fœtal heart's action from the distention of the uterus, and the consequent distance of the fœtal heart from the surface, yet the *souffle* should be as distinctly audible as in ordinary cases, from the fact that the uterine walls and those of the abdomen are in immediate proximity. If, from the death of the fœtus, the auscultatory sounds are inaudible, we must endeavor to detect its passive motions, through the abdominal integuments, and also by tilting up the uterus with the finger introduced per vaginam. By this means, be the distention ever so great, if the head of the fœtus lies near the crest of the ilium, and the pregnancy be sufficiently advanced, we may feel the shock produced by the fœtus falling by its own gravity against the most dependent part of the uterine cavity.

According to authors, pregnancy may be rendered complicated by the existence of a tympanitic state of the uterus — i. e., the collection of gaseous fluids within its cavity—which may occur under various circumstances. *First*—It may be secreted by the lining membrane of the uterus; *Second*—It may arise from the decomposition of the of the jelly-like fluid found between the amnion and chorion; *Third*—It may arise from a relaxed condition of the vagina and adjacent parts.

With reference to the *pathology* of physometra as it coëxists with pregnancy, in the present state of our science, it must remain greatly a matter of mere speculation. It may depend upon a morbid state of the lining membrane of the uterus; though, John Hunter, Dr. Hooper, and others, have never been able to detect any morbid appearances on dissection, though they frequently saw cases of it in the living subject.

Peter Frank, on dissection, found the uterus "enlarged, hard, and elastic," while its interior surface was "ulcerated, hard, and corroded." Dr. Churchill regards it as depending either on subacute or chronic inflammation of the mucous membrane of the uterus.

We cannot conceive how conception could take place while the uterus remained thus morbid; neither can we imagine how gestation could be normally completed, when such a morbid state of the uterus coëxisted with it; for it is universally agreed that physometra causes a suppression of the catamenial discharge. Yet, M. Mauriceau has actually met with several cases in which tympanites coëxisted with the fœtus *in utero*. Ashwell states that he has never seen a case of true tympanitis of the uterus, and that he regards it as dependent on a chemical change in the secretions of the uterus. Hence, we may conclude that it is an exceedingly rare disease, and should it coëxist with pregnancy, the proofs must, from the very nature of the case, be obscure; however, in addition to the circumscribed distention, caused by uterine enlargement, we may expect to find a sonorousness in percussion, as the air or gaseous fluid, would of

course rise to the superior part of the uterine cavity, and perhaps, also, a sound similar to that produced in the chest by hippocratic succussion.

Intra-uterine hydatids sometimes coëxist with pregnancy, and continue to increase with the progressive development of the ovum; though, this complication occurs more frequently where the ovum has been blighted. Some regard "impregnation as being essential to the production of uterine hydatids,"—among whom are Valesneri, Lamzweerde, Puzos, M. Desormeaux, M. Velpeau, M. Nauche, Madame Boivin, M. Dugès, Capuron, Manning, Denman, Burns, Ashwell, Montgomery, Churchill, and Dewees. Others, on the other hand, assert that "hydatids may occur in virgins,"—among whom are Ruysch, Douglas, Fox, Sir C. M. Clark, Gardien, Blundell, and Kennedy. It is very desirable indeed that every controverted physiological point admitting, as this does, of a settlement by the observation of facts, should be established by physiological research.

As the symptoms of hydatid growths resemble very much those of incipient pregnancy, the only sure means of ascertaining whether the uterus also contains a fœtus in connection with these growths, are those elicited by auscultation. With the uterine enlargement, if we discover the pulsation of the fœtal heart and placental *souffle*, we may rest assured that pregnancy exists.

Recent pathologists have found that the mucous membrane of the uterus may be thrown into certain morbid action, that it may, both in the natural, impregnated, and puerperal states, *as a disease of function*, secrete and pour forth large quantities of watery and other fluids. Such being the fact, it is only further necessary that the os uteri become closed, either by adhesive inflammation, or by some other means, to constitute a case of *uterine hydrometra*.

Uterine dropsy may be divided, first, into *idiopathic dropsy*, where the fluid is secreted by the otherwise healthy mucous membrane; and, second, into *symptomatic*, where the secretion, whatever it be, is the product of tumors, fungi, ulcerations of the mucous membrane of the uterus, or where the fluid is the consequence of pregnancy or parturition. The excess of the liquor amnii, and its collection in the amnion as well as between it and the chorion are also, as we have before stated, species of uterine dropsy, and so classed by Churchill. Ashwell speaks of a *peculiar form of leucorrhœa* which is closely allied to hydrometra. It occurs principally in married females during the period of fecundity, and it has no doubt a remote connection with the function of generation. The early symptoms of hydrometra are merely those of irritation, such as indigestion, nausea and vomiting, flatulence, pain, and costiveness.

When the dropsical accumulation becomes considerable, there will be weight and painful tension about the pelvis, distention of the uterus and abdomen, suppression of the catamenia, secretion of milk, and it has been stated that there is sometimes a sympathetic irritation of the mammæ.

Where we suspect pregnancy coëxisting, we shall be able to detect this by the quickening of the fœtus, its passive and active motions, the placental *souffle* and pulsation of the heart. M. Nauche states "that the distention is more uniform, and the uterus rounder and softer than in pregnancy.

Scirrhous tumors of the uterus occasionally render pregnancy complicated. These tumors are usually lobulated, and almost of a stony hardness; their surface is irregular and uneven, composed of separate knobs of induration; their growth is generally slow, accompanied by a continued mechanical pressure on neighboring organs.

Comparing these with the general evidences of pregnancy, especially those elicited by auscultation, and all doubts will be put to an end. Dr. Ashwell remarks: "To some who have not studied and observed the cause and results of these tumors, it may appear singular that conception should occur; but let it be remembered, that they interfere only slightly with the general health, menstruation being almost as regular as when the uterus is not structurally diseased. * * * Pregnancy occasionally takes place when these growths have existed for years, and when that period has arrived at which the faculty of conceiving might be supposed to have almost ceased." (On Diseases of Females, p. 212–3.) Dr. Churchill remarks: "It is very remarkable that so grave a disease should not preclude the possibility of conception." Zeppenfeld, Siebold, Mad. Boivin and M. Dugès record several cases, in some of which the children were delivered by the unaided efforts of nature, in others by version or forceps. Madame La Chapelle (*Pratique des Accouchemens*, vol. iii, pp. 368, 371,) records seven cases, and out of this number four of the mothers recovered from the delivery. Lambreis also, in the *American Journal of Med. Sciences*, (vol. v, p. 233,) relates a number of cases, several of which recovered.

The occurrence of pregnancy in such a condition of the uterine viscus, is attended with imminent danger; and should the female survive the period of gestation, she is apt to sink in the act, or soon after parturition. Dr. Ashwell, on this point, remarks: "The tumors soften during the latter months; the increased vascular supply leads to inflammation; unhealthy suppuration is established, and death generally occurs, if the

tumors be of large size, soon after delivery." (On Diseases of Females, p. 213.)

Uterine polypus, under certain circumstances, renders pregnancy complicated. Dr. Ashwell defines it as "a firm and insensible tumor, usually round and smooth, and growing by a stalk either from the mucous lining of the uterus, or the structure beneath; its chief symptom being hæmorrhage. It commences in the cavity of the womb, in the channel of the cervix, or from the os. It is often of fibrous texture, not malignant, and rarely ulcerates. It is covered by mucous membrane, and sometimes by an adventitious, the product of inflammation. There is little pain, menstruation is excessive, and conception may occur." (Disease of Females, p. 330.)

"When a polypus grows within the uterus to any size, it dilates its cavity as in pregnancy — but here the similarity ceases. For, even before it descends, the os will not be sealed as is the case in conception, but appears more than usually open; hence, conception may occur, unless by the size of the growth the os, or uterine extremities of the fallopian tubes, become closed.

"Pregnancy rarely occurs after the removal of the polypi; and even in cases where they had previously existed, Ashwell thinks that conception is not likely to recur."

Dr. Churchill says: "The presence of a small polypus does not prevent conception, although it renders the continuance of utero gestation very doubtful, inasmuch as abortion is very frequently caused." (Diseases of Females, p. 215, Ed. by Condie.) The symptoms of uterine polypi are irregular and frequently excessive hæmorrhages, producing an anemic condition of the system, depraved appetite, relaxed bowels, frequent vomiting, dyspeptic symptoms, palpitation, œdema of the extremities, and frequently great emaciation. The patient also complains of weight in the pelvis and pressure upon the vulva, of a dragging sensation about the loins and groins, of aching in the back and weariness; when the tumor is large, there is considerable pressure on the bladder and rectum, creating a desire for the evacuation of these viscera, while the performance is impeded. On a vaginal examination we shall at once perceive the tumor, provided it is not retained in the uterine cavity. A rounded, pear-shaped, smooth and insensible tumor will be found in the cavity of the pelvis. Should the tumor be quite small, and still within the os uteri, there will be no perceivable enlargement of the cervix; but should it be large and within the cavity of the uterus, we will find that the womb will be enlarged in proportion to the magnitude of the polypus, and the projection of the cervix modified according to the down-

ward pressure of the tumor. If we suspect that pregnancy coëxists in addition to the polypi symptoms, we must look for the peculiar form of the impregnated uterine tumor, the quickening of the fœtus, the stethoscopic phenomena, together with the passive and active motions of the fœtus, and if they are all present, the existence of pregnancy will be conclusive, and the accoucheur must act accordingly.

Cauliflower excrescence of the uterus is by no means incompatible with pregnancy. Dr. Churchill remarks: "This disease is met with in females of all ages, married or unmarried, without regard apparently to temperament, habits or residence." (Diseases of Females, pp. 229–30.) Dr. Gouch, however, comes to quite a different conclusion; he remarks: "Where we see one case of cauliflower excrescence, we see ten, or even twenty of common polypus, and fifty of carcinoma, or malignant ulcer of the uterus." (Diseases of Females, p. 309.) Dr. Ashwell regards "this as a rare disease; not so uncommon as corroding ulcer, but far less frequent than cancer." (Diseases of Females, p. 308.) Dr. Dewees thinks, probably, he has never seen a true case of this disease. I have met with one case which, however, was never noticed till I discovered it in an examination per vaginam in labor. Should we suspect that pregnancy coëxisted, in addition to the softness, granular surface, and watery and sanguinous discharges, its distinctness, mobility and peculiarity of origin, we must have associated a considerable number of demonstrable evidences of pregnancy to assure us that the complication actually exists.

Ulceration of the cervix uteri occasionally coëxists with pregnancy. The following case came under my observation: On the 28th of October, 1857, I was called to see Mrs. C., æt. 26, who was supposed to be in labor at seven months. She complained of "false pains," aching in the hips, and shortness of breath. By a hasty vaginal examination I found the os dilated sufficient for me to introduce my index finger; the os appeared to be relaxed and flabby. The presentation of the child was natural; I could feel its passive motions, but could not detect the heart or *souffle*. As she did not appear to suffer much, and as I thought the labor had not actually began yet, I gave an opiate and left to visit other patients, expecting to be back in four hours. On my return (four hours had not yet elapsed since I left,) I met a messenger in haste who said she was dying. When I arrived, she had been dead three-quarters of an hour. Being somewhat surprised at her sudden death, I made an examiuation to ascertain if possible, the cause of so sudden a death. The labia were very much swollen, and on separating them I found quantities of bloody purulent matter; on introducing the speculum into the

os, I found two large ulcers on the cervix, one of which had eaten through nearly to the peritoneal covering. Large quantities of bloody matter escaped from the os of a very fœtid smell. On learning the history of the patient, I found that she had been laboring under "this disorder" for about one year; she was a widow, and of bad character. The ulceration, no doubt, was of syphilitic origin. Leave to hold a post mortem examination could not be obtained.

With reference to ulceration of the cervix uteri, Dr. Ashwell remarks: "Sometimes they are mere erasions of the mucous surface, redder than the sound membrane around, and the edges sharpe and well defined. Such are not unfrequently seen after acrimonious leucorrhœa, the consequence of high living and excessive sexual indulgence.

"The ulcers of the cervix are occasionally numerous, varying in size from a pea to a sixpence or shilling, the larger ones being evidently formed by a coälition of the smaller; and long neglect may have induced roughness of surface and great depth, and the color may be a darker red. In such sores there is commonly pain on pressure, and the speculum causes bleeding. The discharge, too, may be sanguincous and of a yellow or dirty white color; but usually, where there is no want of cleanliness, without fœtor.

"There is an ulceration following protracted local irritation, from pessaries, sponge, and *contrivances to prevent conception*, in which the cervix is enlarged and spongy, with increased heat and great tenderness on pressure, and an open state of the os; in such cases, bleeding frequently. * * * I have rarely, if ever, seen the cervix ulcerated prior to natural or vicious sexual intercourse." (Diseases of Females, p. 303, 2d Amer. Edit., by Goddard.)

Dr. Churchill says: "They are common at all ages, but particularly after marriage, and are often a cause of sterility. If they occur after conception, or if conception take place in spite of them, absorption not unfrequently occurs, as I have found, and as has been shown by Mr. Whitehead, and Dr. Bennett." (Diseases of Females, p. 189, New Amer. Edit., by Condie.)

M. Cazeau states that, "in primiparæ as well as in multiparæ, the vaginal portion of the neck of the uterus is of a dark red color, and in primiparæ, its whole surface presents a smooth appearance. * * * * The free portion of the neck, in very few cases, exhibits ulcerations, and more frequently granulations of a cherry red color, and bleeding easily. Among the multiparæ the neck is larger; the opening is divided into different portions; the walls of this portion of the organ are irregular, and present a number of fungous elevation, which bleed readily, in the

hollows between which are occasionally observed ulcerations of a linear shape, and more or less deep." (Amer. Med. Jour. New Series, vol. 31, p. 249.) M. Cazeau regards these ulcerations as of little importance.

MM. Boys de Loury, Costillies, and Coffin, consider the ulcerations of the cervix in the first period of pregnancy as having a marked influence in the production of abortions.

M. Gosselin does not consider ulceration of the cervix of the impregnated uterus so frequent as M. Cazeau is disposed to think it is; nor does he think that it exerts any unfavorable influence on the progress of utero-gestation. He also thinks it is very doubtful whether ulcerations of the cervix uteri has any influence in the production of abortion. M. Danyan noticed, in the Maternity Hospital, the cervix uteri in twenty-two pregnant women, eight of which presented erosions on the surface, and a fungous condition of the cervix ; in six others, the erosions occupied the os tincæ, and the remaining eight exhibited no ulcerations whatever, and had reached the full period of utero-gestation.

M. Cloquet considers ulcerations of the cervix more frequent in the last month of pregnancy.

M. Voillemier has found no ulcerations in the cervix in a large number of women at the eighth month of pregnancy, that he has examined with the speculum.

M. Gerdy has only observed soft fungous erosions on the neck in pregnant females ; these erosions disappeared after accouchement. He considers that the engorged state of the veins from the sinking of the uterus, accounts for the violaceous color of the vulva, the vagina, and the cervix.

M. Huguier considers ulcerations of the neck of the uterus rare during pregnancy, but simulated by other states of the os and cervix, from the muco-purulent fluid often lodging about it at this period. He regards the fungous state of the cervix inherent to pregnancy. *(L'Union Méd.*, April 21, 1855.)

Dr. Bennet observes, "This great development of the granulations, the luxuriant fungosity of the elevated surface, is so marked in some cases, and so seldom observed in the non-pregnant state, that when it is found it may be said in itself to constitute a symptom of pregnancy." (On Inflammation and Ulceration of the Uterus, p. 113-114.)

Ulceration of the mucous lining of the uterine cavity is of rare occurrence. It is not probable that such a morbid state should coëxist with utero-gestation, from the fact of its rarity. Drs. Ramsbotham and Gooch met with a case in the person of a lady who considered herself in the third or fourth month of pregnancy. But on introducing a catheter within

the uterus for the purpose of evacuating the liquor amnii, the instrument passed high up without encountering any impediment or obstruction; it could be "moved about as if *in vacuo*." This lady died, and on dissection "the whole uterine membrane was found destroyed."

Of those conditions which render pregnancy complicated, which exist external to the uterus, depending upon some morbid state of the abdominal viscus, might be considered as solid, fluid, or gaseous adventitious productions occurring within the abdominal parietes. The principal difficulty in detecting pregnancy, when coëxisting with tumors or enlarged visceral growths, will be in the early stage, before the uterus emerges from the pelvis. However, if we attend to the history of the patient, trace a divisional line or mark of separation between the growth and the fœtus, and by *le toucher* ascertain the relative position of the tumor with that in which it first appeared, as well as the direction in which it extends itself; whether it be movable or fixed, whether it is soft or firm to the touch, the length of time it has existed and the rapidity of its growth. On percussion, what sounds are emitted, as well as the phenomena observed by auscultation? By these means we shall generally be able to arrive at a solution of these difficult and properly termed complicated cases.

When the pregnant state is complicated with *tympanitis abdominalis*, it is often marked by an enormous general distention of the abdomen, which keeps the integuments so much on the stretch, as to render any abdominal examination, with a view of detecting the uterus, particularly in the early stage of pregnancy, quite useless. One of its most distinctive marks is, the variance of the degree of distention and elastic feel of the abdomen. The patient usually states that the swelling is much greater at certain times of the day than at others; generally, sometime after eating or in the evening, the swelling is worse. Percussion will elicit a sonorous noise, resembling that emitted by striking an inflated bladder, which we shall find general over the abdomen, unless at the lower part, where, if the uterus be sufficiently enlarged to rise above the pelvis, the sound elicited will be dull. On applying the stethoscope, in these cases, we shall have the intestinal murmur, more or less strong over the whole abdomen, and occasional puffs caused by flatus passing from one intestine to another with considerable force, giving us the idea of its passing up the tube of the stethoscope. However, to arrive at more accurate conclusions, with reference to the existence of pregnancy in such cases, it will be necessary previous to examination, to administer a few doses of castor oil and spirits of turpentine; preceded, if necessary, by a small dose of calomel, with a view to clear the intestines of flatus.

After the purgative draughts have emptied the intestines, and the integuments become flaccid, we shall be able to satisfy ourselves much better by manipulation, whether the uterus be enlarged or not. It is astonishing with what facility we can detect pregnancy even in the most complicated cases, by adopting these precautions prior to our examinations.

Ascites occasionally is found coëxisting with utero-gestation. Where the female has conceived in the progress of the disease, or after it has been fully formed, the pregnancy may escape our notice. In chronic ascites, females have been known to bear several children without any great inconvenience, farther than the extreme distention caused by the enormous fluid and solid mass contained within the walls of the abdomen. Dr. Baker, in the *Transactions of the College of Physicians of London*, (vol. ii., p. 257,) relates the case of a woman who bore triplets without any difficulty, whose abdomen was enormously distended by ascites. In general, however, the termination of these complicated cases is by no means so fortunate; as patients so circumstanced, seldom survive their delivery more than a few days, or weeks at farthest. When pregnancy occurs in the course of ascites, the patient, if her catamenial discharge does not return at the usual period, naturally ascribes this to the effects of the disease, and never once conjectures herself pregnant, till the quickening period causes her to suspect her real state. If the hydropic effusion be considerable, and the walls of the abdomen much distended, we may, by careful manipulation, detect a circumscribed tumor over the pubis, which we can recognize as the enlarged uterus; when we cannot distinguish this, we shall, on percussion, find, if the uterus be sufficiently enlarged, a dulness at that part where it approaches the integuments, and a want of the fluctuation which exists throughout the rest of the abdomen. By *le toucher* we shall find the uterus enlarged, and the cervix developed; and by *ballottement*, we may distinguish the passive motions of the fœtus. Should all these means fail, we must then have recourse to auscultation, and if we detect the fœtal heart and the placental *souffle*, the evidences of the coëxistence of a fœtus *in utero* are positive. Ascites is occasionally a natural attendant on pregnancy, and when this is the case, it usually commences about the third month after impregnation; here we shall have the general abdominal enlargement, progressing and increasing with that of the uterine tumor. When the accumulation of the fluid is not very rapid, the uterine tumor can easily be distinguished rising out of the pelvis. The reverse may also take place, where the effusion comes on suddenly, or when we do not see the patient until the accumulation of the fluid has so increased that our diagnosis is greatly interfered with.

Precision, here, becomes a matter of the utmost importance, not merely from the effect which any inaccuracy will have upon the mind of our patient, or upon our own professional character, but because it leads to the worst possible consequences in practice. For instance, did we treat our patient with repeated drastic purgatives, or active remedies, for ascites, without regard to her being pregnant, what must we expect? Or, did we recklessly force the trocar into the integuments, while the enlarged uterus was behind them, what is more likely than, that we should drive the instrument in the uterus and consequently into the fœtus? Whereas, had we been aware of the existence of pregnancy, properly and well timed treatment might have saved both mother and offspring. We should be aware that pregnancy may be mistaken for certain morbid states, such as dropsy, visceral enlargements, etc., as well as they for it.

Sir Astley Cooper relates a case, where the pregnant abdomen was attempted to be tapped by an ignorant practitioner. Lowder, in his lectures, mentions a somewhat similar case, in which pregnancy was rendered complicated by the flattening and extending upwards of the bladder. Whereupon the practitioner pronounced it dropsy, and tapped, which resulted in the death of the patient. On an examination, the trocar was found to have penetrated both walls of the bladder, and to have passed through the uterus into the head of the fœtus.

We cannot, therefore, be too cautious in arriving at conclusions in these cases; and while a doubt at all exists upon our minds, as to their true nature, no plan of treatment that may be attended with hazardous or questionable results should be adopted, but should scrupulously be avoided.

Extra-uterine fatation occasionally complicates normal pregnancy. This morbid state, if such it may be termed, is supposed by some pathologists to depend upon a "morbid condition of the fallopian tube, through which the ovum is passing; such as an impervious state taking place after conception, a contraction of its calibre owing to *engorgement* of its lining membrane, or spasm of its fibres, paralysis affecting its structure, or disease of the coats of the ovum." Dr. Montgomery, in a case reported by Dr. Jackson in the *Dublin Med. Journal*, (vol. ii., for 1833, p. 137,) "thinks that a blow received on the abdomen, shortly after conception, produced such a degree of inflammation and *engorgement*, as to arrest the ovule in its transit from the ovarium." All pathologists, I believe, allow the existence of extra-uterine fœtation, yet none are able to fully explain why nature thus deviates from her esteblished laws. Coste, states, "that among all animals, woman is most subject to this affection." Dr.

Ramsbotham, however, states, that "instances of it have been observed in the cow, sheep, bitch, and hare, besides others. Dr. Campbell has also met with it in the lower animals.

Extra-uterine pregnancy exists under four forms, i. e., ovarian, tubular, abdominal, and parietal. In the first, the fimbriated extremities of the fallopian tube have embraced the ovarium, and impregnated the ovule without detaching it from its bed. In the second, the ovulum has become impregnated and arrested in its transit through the tube. In the third, the fimbriated extremity of the fallopian tube has grasped the ovarium, but has let the impregnated ovule fall into the general peritoneal cavity. In the fourth and last, the ovule, after having been impregnated, has passed through the fallopian tube, until it has arrived at the portion of the canal which transverses the structure of the uterus, and has there become embedded.

This condition may occur, and yet the female may conceive or bear several normal children; consequently, it may be said with propriety, to complicate pregnancy in all such cases, there being a tumor observed besides that produced by natural pregnancy, which is observed to remain even for months or years after delivery. In such cases, the accoucheur is often perplexed; and was it not from a knowledge of such extraordinary and unaccountable freaks of nature, we might precipitate ourselves into a dilemma that would by no means be desirable.

In the *Philosophical Transactions*, (vol. v., p. 246,) a case is mentioned, where an extra uterine fœtus was retained twenty-eight years; the patient in the meantime having given birth to three children. Dr. Denman met with a woman who carried an extra-uterine fœtus thirty-two years, and enjoyed good health all the time. Dr. Ramsbotham met with a woman who bore three children after an extra-uterine conception took place. In the *Medico-Chirurgical Review* for January, 1834, there is an account of a woman who has borne three children since an occurrence of this kind took place. The extra-uterine fœtus, was carried eight years, and was finally evacuated by a fistulous orifice, near the umbiliens. In September, 1833, a case was communicated to the *Academy of Medicine* in Paris, of a woman who retained an extra uterine fœtus upwards of thirty years, and gave birth to three healthy children during the time. She died at the age of seventy-eight. Dr. H. P. Leigh, of Petersburg, Virginia, in the *Virginia Medical Journal*, reports the case of a negress, æt. forty, who carried an extra-uterine fœtus four years, and remained in good health. Dr. Geuth, of Berlin, reports the case of a woman who bore three children during the period of her carrying an extra-uterine fœtus outside the abdominal cavity. (British and Foreign Medico-Chi-

rurg. Review.) Dr. A. W. Heise, of Addison, Illinois, reports a case in the *North-West. Med. and Surg. Jour., August*, 1856, occurring in the person of Mrs. Yungels, of Aurora, Illinois. In 1851, Mrs. Y. conceived, and experienced the usual symptoms of pregnancy for a period of ten months; during which time the abdomen constantly enlarged, particularly in the right side. At the end of ten months, labor began, and a midwife was called, who, after expressing her fears that the child did not *lay right*, declared that she would not be delivered etc., etc. She was, however, partially relieved, by the discharge of "watery matter, having the appearance of beef brine, followed by coägulated blood. The abdomen now smartly diminished in size, which continued for three weeks; leaving still a tumor in the right side. From that time up to November, 1855, she conceived twice, and brought forth two healthy children. In May, 1856, there was an ulcer observed in the umbilicus, about the size of a half dime, with a bone protruding. Dr. Heise, assisted by Drs. Young and Allaire, of Aurora, extracted the skeleton of a fœtus, of the size of one of seven months. She recovered, and up to May 10th, was quite well. (*N. O. Med. and Surg. Journal*, vol. 13, p. 248.)

With reference to the *symptoms* of extra-uterine fœtation, they are frequently obscure; though, generally, those of incipient pregnancy. The menses are suppressed, the breasts enlarged, the areölæ are, if anything, more developed, morning sickness, and, in fact, all the signs of normal pregnancy are present; however, there is more local pain, generally referred to one point of the side, mostly low down, within one or other ilium. If the child lives, it quickens; but the motion is felt more on one side than the other, and is usually attended with a great deal of suffering. There is considerable enlargement of the abdomen, but it will be observed more to one side, lower or higher than usual. The child's limbs can be felt through the abdominal parietes. The bowels are generally constipated, and the urine is passed with difficulty and with pain.

Examining, per vaginam, will reveal an unimpregnated uterus, unless the extra-uterine fœtation is found to coëxist with normal gestation. A foot or hand of the fœtus may sometimes be felt through the coats of the vagina. It is not probable that the extra-uterine fœtus will be alive, and at the same time normal gestation in progress; but should this be the case, the situation of the two tumors, their relative size and position, one being detected *in utero* by its passive motions, and the other being found without the uterus, will aid us greatly. Auscultation will aid us much; should we detect the fœtal

pulsation and the placental *souffle*, being satisfied that it is *in utero*, while there remains a firm inert mass external to the uterus: we shall be able to diagnosticate the case without much difficulty, keeping in view, however, the early symptoms of the tumor, without the uterus, with the history of its occurrence. Should putrefaction take place, symptoms of general abdominal inflammation supervene, attended with the usual phenomena attendant on suppuration.

As we have already protracted our remarks to a considerable length, we fear to the displeasure of our kind readers, we shall bring the present paper to a close, and continue the subject of *pseudo-pregnancy* in our next.

(*To be concluded.*)

ART. II.—*Clinical Lecture of* PROF. TROUSSEAU, at the Hotel Dieu, Paris; from notes, taken by DR. CHASTANT. Translated by DR. CHAILLÉ.

SCARLATINA.

ANASARCA in a child who had scarlatina, fifteen days ago.—The disease conducted itself in the manner of benign scarlatina. This child is bloated, with all the symptoms of scarlatinous hydropsy ; little fever, bloody urine—containing consequently some albumen, etc. Besides this albuminuria, there is Bright's disease, which is chronic, and without remedy. The former albuminuria, the scarlatinous, is acute; the latter, which accompanies the disease of the kidney, is chronic; scarlatinous anasarca is little serious. But in some circumstances, there are complications, so rapidly mortal, as to surprise and discountenance one. I have often seen children attacked with eclampsia in scarlatinous anasarca. M. Paul Dubois says, that general anasarca in pregnant women is often followed by albuminuria. Convulsions must be feared above all during the parturition, when there is a strong chance of death. Those children who are naturally predisposed to convulsions ought then to be still more subject to them; and when they occur in the anasarca which has succeeded scarlatina, the prognosis is not favorable, even when there is no fever. Nothing so much resembles a dying child as one in convulsions. Scarlatinous anasarca, even when not followed by eclampsia, does not always terminate in so simple a manner. Some children, are afflicted with chronic anasarca, chronic albuminuria, Bright's disease, and die.

When scarlatinous anasarca has continued three weeks, the prognosis is less unfavorable, not for the convulsions, but for Bright's disease, which is almost always fatal. Scarlatinous anasarca affects the whole body, the eye-lids being often more swollen than the inferior extremities. In some cases, the anasarca affects the internal cavities, into which serum is effused. Effusion in the large cavities will be found by auscultation and percussion, which the enormous dilatation of the pupil also indicates. Other accidents also follow, as œdema of the glottis (improperly so called), or, preferably, œdema of the aryteno-epiglottic ligaments. I have seen one case, in 1833. Previous inflammation of the throat invites, in some measure, this œdema. Almost all afflicted with scarlatina have angina. This œdema is very different from that which is termed in the adult œdema of the glottis, which is a phlegmasia in which the serous element predominates, in consequence of the nature of the tissue occupied by the phlegmasia. The treatment is expectant; the diet, sweetened milk; syrup of orgeat, a little calomel, and, after the inflammation has subsided, a little digitalis. Is scarlatina the only eruptive disease followed by anasarca? So it has been said; and I have also said so for a long time. But I say so no longer. Last year I had, in my wards, four or five cases of rubeola, followed by anasarca. This anasarca has neither the character nor the gravity of scarlatinous anasarca. In none of the five cases was albuminuria present; nor did they have convulsions, although cases can be found in which they occurred; nor did the anasarca become chronic. In general, then, there is no danger; nor do they terminate in Bright's disease. Scarlatina is at this time epidemic in Paris; it is not very fatal, although there are some victims. Scarlatina is one of the most extraordinary diseases in its form and course. There is no physician, particularly outside of the large cities, who has not seen in some epidemics, excessively grave cases, by the side of others so benign that the physician was not even called in. In other cases, it kills one-third of the population, and more than half of those whom it attacks. Scarlatina is an exanthematous pyrexia, with general fever accompanying the modifications of the skin. The fever is contagious; and here let us enter into some details, upon the subject of

CONTAGION.

By a contagious disease is understood a disease which transmits itself from a sick individual to one well, preserving its specific character, that is to say, the character which renders it distinguishable in a common class. Contagion is rigorously then a generation, from a morbific seed, which resembles animal or vegetable seed, and is carried from an indi-

vidual diseased to an individual in health, by any way whatever. The great objections raised against contagion in disease, can also be made against the propagation (contagion) of the seeds of plants and animals. When certain vegetable substances are planted, it is necessary there should be particular conditions, of the weather, of light, of temperature, of depth, of the sun, of the hemisphere even if I may so speak. There are some seed which at a certain depth will never germinate ; for others, depth is nothing. If the morbific seed be compared to the vegetable seed, it will be comprehended that in certain conditions of its introduction into the economy, no evolution will longer take place, as we have just seen occurs with vegetable seed. Vegetable seed select their season: I will explain. The legumina sprout during every month, provided other necessary conditions be present, but there are some seed which do not so ; for example, plant in February a thousand cherry stones, in May two or three hundred will germinate, there will remain seven or eight hundred which will not budge. The months, the seasons will succeed each other with their variations of temperature; however, none of them will sprout until the following May, at which time four or five hundred more will germinate; and so again the seasons will roll by, but none of the remaining seed originally planted will sprout until the month of May of the third year.

Let us examine the animal kingdom. Réaumur *(Hist. des Insectes)* took some chrysalides, and preserved fifty of them under a cover in some of the dust of his inkstand. At the end of eight or ten months, in June, he perceived ten butterflies. The forty others remained chrysalides. The days, weeks, and months succeeded without causing their evolution. However, the chrysalides still lived, for at the end of nearly a year, again in June, thirty of the forty which remained were developed, and in the year after the ten chrysalides which remained were developed.

The evolution of organic seed does not occur at the same period ; it is the same with contagion. The individual who has received the morbific seed of variola, rubeola, etc., may always look for its development without exception of time, whilst other morbific seed are developed at a fixed time, as dysentery in this part of Europe, in August, September and October, and in those families where it reigns, it will appear only in the same month. Some contagions infect those brought in close contact with the diseased, as though they were burned by it; others fly far away, as the seed of the "*male du Palmier.*" One does not contract the itch or the small pox after having conversed with those afflicted with the former, nor after traversing a ward filled with the victims of the latter. Variola, rubeola, and also yellow fever, may infect those near to them.

Spallanzani has made some curious experiments upon the generation of the batrachian animals, the green frog, the toad, the salamander; and he has studied the manner in which the eggs of these animals are fecundated. When fish wish to fecundate, the female deposits her eggs in the grass upon the bank, and the male projects his semen over the eggs. Let us examine the experiments of Spallanzani, as to the batrachians: the toad places himself upon the back of the female, presses her with his feet, his claws upon her shoulder, applies his cloaca upon that of the female, and projects his semen over the eggs just as they pass out. The eggs of others are fecundated in a more immediate manner. For the salamander, Spallanzani took some of the eggs of the female, and some semen from the generative organ of the male; he poured this semen over the eggs of the female, they were not fecundated; the eggs of the frog, in the same circumstances, were fecundated. He observed the salamander in the fountain of his garden at the period of their amours. The female began to lay her string of eggs, at which moment the male, who remained at a certain distance, discharged his semen; here instead of an action immediately contagious, was an action disseminated. Spallanzani then diluted one drop of semen with a large quantity of water, and the eggs were fecundated.

The condition in which certain vegetable and animal germs are developed, are badly appreciated by us. The same holds good with contagion. Contagious diseases always come from a germ, although this germ may have slept inactive for years, and ages; exactly as those vegetables in the forest of America, which have slept for ages, and which appear so soon as the forest trees have been cleared out, admitting the conditions necessary for their growth. It is by this means that the development of rubeola, scarlatina, variola, in isolated villages is explained. Let us take the claveau (the rot), in a flock; the claveau is the variola of sheep. These sheep sleep in the same stable; in one word, live together. How does it happen, that this disease commences the first of April, and finishes the first of October? How does it happen, that upon eight hundred sheep being exposed to one having the claveau, they are affected each after his own manner? Some will take the disease at the end of eight, some at the end of fifteen days, some after a month; others will be refractory to the contagion of this year, but the following year, in running over a field upon which a different flock has just been pastured, these will be the first attacked by the disease. Let us return to the SCARLET FEVER.

This is, then, a contagious pyrexia with very different forms.

In the most ordinary and simple form, the individual without any previous symptoms has chills, vomitings, diarrhœa. Such is the ordinary prelude. These phenomena accompany the fever: The acceleration of the pulse is greater than in all other known diseases. Generally there are no nervous symptoms. Children sometimes have convulsions, but not so often as in variola, where they are common. After ten, twelve, or twenty-four hours at the most, the eruption appears. Of all the exanthematous maladies, it is in this that the eruption soonest appears. Never is its appearance delayed longer than forty-eight hours. Although the eruption appears so early, it yet lasts longer than that of rubeola; it may continue bright for ten, twelve, or fifteen days, while the eruption of rubeola does not remain vivid after the eighth day of its appearance. In the meantime invariably appears sore throat, which is pharyngeal. The laryngeal cough of rubeola does not exist in scarlatina, but the sore throat is characteristic of, and appertains to it. Engorgement of the ganglions of the angle of the jaw, of the submaxillary, and even of the cervical ganglions accompanies it. The eruption of scarlatina appears first upon the face, upon the internal surface and bend of the arm, upon the folds of the groin, upon the chest and abdomen. It is strongly marked upon the internal surface of the arms and legs, upon the chest and abdomen; whilst in rubeola it does not exist at the bend of the arm, it is not well marked upon the chest and abdomen, and is very confluent upon the face. The eruption of rubeola is a myriad of little red spots; of scarlatina a speckled redness forming a sort of *plaques*, as if the individual had been struck by the hand. When in rubeola, the hand is passed over the abdomen, little elevations (*boutons*) are felt as soft as velvet; in scarlatina, a sensation of hardness like that produced by the skin of shagreen, is felt by the hand when passed over the neck. It is very difficult to describe this disease, for although these contagious, exanthematous diseases have a specific character which can be described, still the peculiarity is more in the (*allure*) mode of contagion, or even in the accidents which supervene, than in the forms. Pneumonia may be described very easily and briefly; it is not so with scarlatina. We have said that in this disease there was a multitude of little spots separated at first the one from the other, and forming a sort of painting of the skin. It has been said, with some exaggeration, that the patient has the appearance of having been dipped in a bath of mulberry juice. The desquamation commences on the body, upon the limbs, and finishes

at the hands, ten and twelve days after the invasion of the disease, and the third, fourth, or fifth day after the desquamation has begun.

In some cases the falling of the nails is involved in the desqua, mation. The character of the disease is well marked in this respect. that the scales are not large upon the neck and the chest, whilst that they fall from the hands and feet retaining the forms of these extremities. In rubeola there is nothing like this, though such is generally the aspect of scarlatina. Let us enter into the details.

Invasion.—Of all diseases this is accompanied by the greatest acceleration of pulse. This is a leading point. In the first period of the attack, the adult has one hundred and sixty, one hundred and eighty, sometimes two hundred pulsations in the minute, which is so enormous that the clinical physician thinks life is about to be extinguished. The heat of skin is considerable ; like that which is met with in erysipelas. What characterizes the disease is the suddenness of its attack. In rubeola, for instance, there is some cough, coryza, fever ; whilst in variola, these symptoms are more severe ; but in scarlatina, the state of health is separated by nothing from the period of invasion, which is often so grave that many cases die upon the first day, which never occurs in rubeola and variola, unless convulsions coming on, they succumb with eclampsia. Whilst in scarlatina they die with delirium, somnolency, coma, uncontrollable vomiting, diarrhœa, and disordered movements of the pulse, which sometimes are so rapid that they cannot be counted.

When there is an epidemic one knows not what to expect. In 1828, a formidable epidemic of scarlatina broke out in the department of Eure and Loire. M. Bretonneau having learned that a woman of twenty years had died in the morning, he and I went to the house of the deceased. She had been sick eleven hours, dying after delirium, vomiting and diarrhœa. We examined the corpse and found but one thing, vesicles upon the abdomen and neck; at the autopsy, the membranes were dry and fishy, the blood black. This was the *début* of the disease. Three months after, every physician reported some cases of this kind. Here, then, we have an exanthematous pyrexia with a character of unwonted gravity.

Form of the Eruption.—It appears ordinarily upon the first day, which is rare in an exanthematous malady, and happens in only two others. The redness manifests itself upon the face and neck, and upon the sides of the abdomen upon the first day. On the second day it invades, generally, the whole body. The third day it is at its highest, and of a violet color. When it is severe,

there are numerous vesicles upon the neck and chest. The color of this red eruption is well marked, and lasts eight or ten days at the most ; it pales towards the ninth or tenth day ; it begins to decline the twelfth or thirteenth day, and ceases completely upon the fourteenth day. Sometimes, however, it persists almost to the twentieth day, and goes on increasing with the disease, still continuing after the eruption has ended. Upon inspecting the throat, the velum palate will be observed to be red upon its pendulous portion. The roof is less red, a violet red. There is swelling of the amygdalæ with small white spots upon them ; they then become covered with white concretions, unless colored by blood from the nostrils, or by the drinks. The sub-maxillary and cervical ganglions are swollen, and very painful upon pressure. The secretion of the amygdalæ augments for several days, and ceases only upon the tenth, twelfth, or the fourteenth day ; and sometimes leaves in its place the red amygdalæ slightly excoriated and bleeding, and the patient complains of pain in the ears. This is the form benign. The tongue is characteristic ; the first day it is charged with a yellow coating, which is slimy with vomitings, its edges are of a vivid, brilliant red, which is increased upon the third and fourth day ; the tip of the tongue is laid bare upon the fifth day, and the entire epithelium stripped off as if the tongue were burned, beneath it remains a cherry red, swollen, with projecting papillæ resembling a strawberry. The patient says that the drinks are a little sapid, causing him pain. This pain characterizes scarlatina. Soon the tongue becomes soft, its epithelium is restored, and it assumes its normal state at the end of ten or fifteen days.

Accidents of the Angina.—Scarlatinous angina, in its benign form, takes the course we have just described ; sometimes, however, there is only redness of the amygdalæ. But in many cases this phlegmasia of the tonsils persists as long as the eruption lasts, sometimes without great gravity. Again, towards the tenth or twelfth day, when the fever has ceased, it all at once rekindles, and is accompanied by enormous swelling of the ganglions in the angle of the jaw, cerebral symptoms, fœtidness of the tonsils, and when the coating is removed with a sponge from the amygdalæ, ulceration, and sometimes sphacelus is found, which is always followed by a fatal result ; there is incapacity to swallow ; the ganglions of the neck are enormously swollen, the mouth remains half open, a fœtid liquor oozes from the nose, and the food is rejected ; scarlatinous buboes are formed, and the patient dies, not from the angina,

but accordingly as the abscess opens or not. In other cases still more serious, all the neck swells uniformly, and upon opening it small cancerous bodies are found, and the patient dies almost always.

The eruption is often wanting. The question has been discussed whether an individual could have variola or rubeola without the eruption. A majority of physicians have said no. In regard to scarlatina, the same has been said; however, it does exist without the eruption, which fact has been reported by authors who have seen epidemics of this disease. The following facts are cited by Graves, in his clinical lectures: Scarlatina existed in the family of a physician; the daughter was not sick; whilst attending upon her relations she had some pain in the throat, which passed off. The physician ordered his daughter to go into the country, and in eight or ten days after, this young girl who had been examined every day by her father, took anasarca with albuminuria, a certain sign of scarlatina. Facts observed by M. Trousseau: In 1829, an epidemic of scarlatina reigned at Villercy, (a village in the environs of Paris,) among the neighbors of a chateau inhabited by eighteen persons. In August and September the disease attacked several dwelling in the chateau. The larger number had the eruption; about one quarter of them, who did not go to bed, so (slight was the fever) had sore throat. They either did not take scarlatina, being proof against it, or some *malaise*, and this sore throat were the only symptoms of it. One young girl, who had no eruption, did have sore throat and anasarca, (as occurred in the case of Graves), at the time when the others had the eruption, followed by the same phenomena. There are degrees in the symptoms. In a family having scarlatina, if the neck, the body, the arms, the fore-arm of children having the slightest *malaise* be examined, the disease will be found to manifest itself by fever, sore throat, and a little eruption upon the wrist and the neck; if the disease be still milder, you will find the sore throat only, which is the eruption in the mouth. These facts must be habitually kept in mind, otherwise errors in diagnosis will be committed.

Works have been written upon the angina maligna, and the angina diphtheretic of Bretonneau; many of them are allied to scarlatinous angina. There is no difficulty in diagnosticating those cases of angina maligna which occur in a community visited by scarlatina. But one not forewarned, might confound angina diphtheretic with that of scarlatina. The latter is characterized by its suddenness, by fever, also by its skinned tongue with its vivid red-

ness, (the other not so). Important characteristics are derived from the fact, that the germs of the one produce croup, of the other, scarlatina.

Two very considerable modifications may supervene in scarlet fever. First, many local affections coming on after, and second, dissolution of the blood, and hæmorrhage coming on in the course of and at the end of the disease. Let us first consider the second accident.

It happens in a scarlatina which has pursued its course sometimes sufficiently simply, that all at once, towards the ninth to the fifteenth day, when the physician flattered himself that all was doing well, the child is taken with nasal hæmorrhage which causes him but little inquietude. It is renewed, two, three and four times during the day. In the morning it suffices to make the child sit down, and to throw in his face a little fresh water. In the evening it is necessary to apply a *tampon*. The child becomes pale, the pulse has an extraordinary frequency which is no longer that of the scarlatina, but of the hæmorrhage, of anæmia. In the course of the night he passes blood from his bladder, and his night-clothes are bloody. On the next day the tongue is black, and fuliginous, and the pulse can no longer be counted. There are hæmorrhagic spots under the skin, and if a blister has been applied, it sweats blood. It is almost always, if not always a mortal form. The blood oozes out as in the "*maladie maculatoire de Werloff*," or as in scorbutus. I have had two such cases, and similar facts are indicated in all the authors.

Let us consider the first accident, the many local diseases which are always the result of diathesis. For when an individual has many local affections bearing a certain analogy to each other, the cause must be looked for in the diathesis. There are acute as well as chronic diatheses. The first are under the influence of a grave intoxication from morbid poison. When an individual has passed three weeks in the midst of the marshes of Africa, he will be found pallid, his spleen and liver swollen, and other accidents, appear, which indicate that he has a miasmatic diathesis, manifesting itself by a multitude of lesions. If the scarlatinous virus with its grave modifications be introduced into a person strongly predisposed to scarlatina, there will succeed the attack, deafness, necrosis of the nose, engorgement of the ganglions with suppuration, lachrynal fistula, etc. In such cases there is a modification of the blood, such that the mucous membranes take on the

disease, the one after the other, and should a phlegmasia supervene, it has a great intensity. We should be habituated to consider scarlatina as the cause of a multitude of affections of the mucous membrane of the nose, of the eye, of the throat, of the vagina, and of the impetiginous affections of the skin. Lesions, which besides the gravity attending the eczema, have also that gravity which appertains to the cause.

Scarlatinous rheumatism is a very common sequela. In some epidemics, one-fourth or one-fifth of those attacked with scarlet fever will have rheumatic affections. It would appear that this affection ought to be very severe in scarlatina, and tends to suppuration; theory would make us believe so; but it is far from being so, generally. It has, in fact, but little gravity and lasts but a short time. If it be monoärticular, it is dangerous, and leads to caries of the bone. This affection often lasts so short a time, that great attention is necessary to distinguish it. It is important to know that this rheumatism is common; for we have thus a key to facts advanced by old practitioners; as for instance, diseases of the heart (see the works of M. Bouillaud) occurring after scarlatina; and you will understand mothers when they tell you, "my daughter had scarlet fever about eleven years ago, and since then she has been subject to rheumatism," for scarlatinous rheumatism leaves a melancholy disposition to this disease.

Acute articular rheumatism is a disease which repeats itself in other forms. It is thus that the scarlatina in children is succeeded by Saint Vitus's dance, which generally follows the acute rheumatism, as demonstrated by M. Scée, whose exaggerations I do not accept. Boys rarely have chorea, especially at fifteen years of age. We have here a boy of fifteen who still has it, as the result of scarlatinous rheumatism. Succeeding this rheumatism, we have chorea, and diseases of the heart. Thus also, does Bright's disease succeed albuminuria, an affection which happens at the moment the anasarca begins, and when it has taken its development, convulsions, eclampsia come on.

Treatment of Scarlatina.—When the disease is simply sporadic, each one has a stereotyped treatment, according with his own medical education. But when the disease is epidemic, considerable difficulties arise. The scarlatina of one year is not that of another year, so far is this so, that the difference in some of the chief phenomena of the disease imposes upon the therapeutist considerable differences of treatment.

A few words of the medical constitution : What is it? As far as a certain point, we all have the same aptitudes, some things in common ; then there are idiosyncrasies which belong peculiarly to each individual. In fact, each one has his own temperament. Now all at once without any change of the wind, (for without explaining farther, physicians do believe in its influence which surely can effect something,) there occurs a modification of the conditions in which we are placed, in virtue of which different acts carried to a certain point bring on the cholera. On the 29th March, 1832, nine persons died with this disease; by the 30th, three hundred had died, and nineteen days afterwards two hundred died per diem. What is it that had occurred? A strange thing ; something has fallen upon England and France without touching anything upon its road. How is it we know nothing of it? Notwithstanding we do procure this important fact, that an individual who eight days before indulged in an excess without taking the cholera, eight days afterwards did take it, after a similar excess, and even after some customary indulgence, harmless heretofore. To cause these accidents there must have been something in existence more than ordinary. This something was a syncrasy, a general modality, a common manner of being, a medical constitution. In the month of February, the cold gives rise to pneumonia, acute catarrhs, etc. But at the period of the cholera we no longer had the ordinary diseases ; we had only the cholera. That is to say, all the causes of disease turned themselves into one powerful disease, and evidently from the common disposition to it.

We frequently see these modalites come on in Paris. We have an epidemic of grippe which has attacked more than one-third of the population. It has been attributed to the north wind, but the wind has changed, and the epidemic continued. The grippe has been taken by the fire place. It is caused by a particular disposition, in virtue of which one is subjected to an influence which he has not before undergone. In 1829, during fifteen days, all who partook of ice-creams believed that they were poisoned ; when the influence ceased, the accidents ceased.

To return ; if the scarlatinous virus infects you under conditions well determined and normal, the disease will run its course without serious accidents ; but if your condition be bad, terrible accidents will attend it. In grave epidemics there are some special conditions which render one peculiarly predisposed to receive their poison. Surgeons know this, for at some times nothing follows the cut of the bistoury; at others, erysipelas.

We have stated why the treatment should vary. Sometimes there should be no treatment. The physician is generally as impatient as the sick man ; he wishes to cure the disease at once. But it is not necessary to attack impossibilities; for thus is evidence given of ignorance and impotence. The evolution of diseases of a non-necessary form may be modified by therapeutic agents ; but those of a necessary form cannot be modified. We speak of morbid phenomena, and not of their accidents ; mark the distinction. If the contagious disease be a seed introduced into the human body, a sort of germination takes place. Such a disease will comport itself as far as a certain point, like a seed; it will increase, decrease, and die out. That is to say, there is a certain time for these diseases to last. If you accept these contagious diseases as having a necessary course to pursue, you will understand that you will be able to cause them neither to advance nor retreat a single step, and will be without action upon them. You will be able to exercise an action upon the economy, but none upon the morbid affection. You will have no control over the entity, rubeola, (if I may so speak) over the entity variola, etc., but upon the economy in contact with this entity. If the entity does not cause serious accidents, it is necessary not to interfere. The economy will be at first distressed, from which will result fever; then the economy will become accustomed to the poison, and at last will throw it off. It is then the part of the physician to contend against an inconvenient excess in the necessary diseases. He must yield to the necessity, and, so long as the thing is tolerable, the sore throat, the fever of the eruption, and the secondary fever, moderate, no delirium, nor serious accidents happening to the abdomen, nor the kidneys, do nothing. So much being considered let us pass on.

A singular fantasy has been entertained since 1816, that fever in man is the sign of an open or latent phlegmasia. We seek for it, and, when after searching well, we do not find it, we say that it is because we have badly searched. No; it is because the fever is not the sign of a phlegmasia. Have we not fever in a man who runs, acceleration of pulse, development of heat, perspiration? But there is here no phlegmasia. Then there is the fever of the man who digests, whose pulsations two hours after his repast are increased by twenty or thirty more beats ; this is the mode of action of the economy when opposing the chyliferous element, producing a modality which causes fever, even a chill, and perspiration. Again, the *fever of pleasure*, is it not a very active and veritable fever? Let us leave physiology.

Fontana caused a rabbit to be bitten by a viper; five minutes after, the movements of the heart were so accelerated that they could not be counted; rapid anpelation, vomiting, serous discharges followed. Was this a phlegmasia? No one will believe it, any more than that the enormous acceleration of the pulse succeeding the ingestion of opium, belladonna, or of hyoscyamus is the result of phlegmasia. Therefore, for like reasons is it, that when the scarlatinous virus is introduced into the economy, multiplying itself by a sort of fermentation, the fever of invasion comes on at the moment when this multiplication takes place. It is a very violent fever, but it is not produced by a phlegmasia. This leads us directly to the treatment; for with our rage for seeing phlegmasia wherever there is ardent fever, we resort to antiphlogistics, bleeding, saline purgatives, and we wish at every risk to extinguish this supposed phlegmasia. Although we find it not, we still attack it. Almost all who have observed epidemics of scarlatina, concur upon this point; when in the outset the pulse is extremely frequent, the fever is not modified by bleeding, which rather augments the accidents. Bleeding ought to be moderate, say those who employ it. There are some cases where it can be useful. But smallness and frequency of the pulse in the beginning contraïndicate bleeding; otherwise the nervous accidents will take precedence.

During the fever of invasion which is very short, it is necessary to wait; there is nothing to do. When from the first day, the frequency of the pulse is excessive, and at the same time delirium comes on and nervous phenomena, the prognosis is unfavorable. Administer the preparations of cinchona and aromatic infusions. Sustain the system by the chloro-hydrate, and above all, the acetate of ammonia. When the fever is not very high, with vomiting and diarrhœa, leave it alone. During the period of eruption which lasts from five to fifteen days, something must be done against the eruption in those cases about to be mentioned. You have heard of this folly; the greater the amount of, the severer the eruption in rubeola and scarlatina, the better it is. Just the same as if one were to say the more eruption in small-pox the better. It is a monstrosity, for scarlatina, rubeola and variola are far more benign in the discreet, than in the confluent form. The eruption ought not to be too great, unless there exists at the same time some antagonistic affection. For example, a child has rubeola, three days after he takes a pleuro-pneumonia. On the day for the appearance of the eruption, you will see only a few spots; this is not a discreet ru-

beola, but a rubeola which has attacked the lungs. So with scarlatina, if there be diarrhœa, violent vomiting, it is to be concluded that the mucous membrane of the intestines are attacked. Therefore, when the eruption upon the skin is benign, the disease is benign, the exceptions being omitted.

Swelling of the neck and face, tumefaction of the hands, so that the patient can only bend the hand, but not double the fist, is a vehement phlegmasia, or rather a particular form of phlegmasia which is manifested upon the skin. The eruption of scarlet fever does not retrocede oftener than that of variola, (rubeola sometimes retrocedes). A modification of the scarlatinous redness, indicating an internal antagonism, denotes great danger. The swelling of the hands disappears; the fist can be doubled; the color rapidly inclines to the violet red instead of slowly subsiding into it; in such cases it is necessary to act in order to reëstablish the eruption and to combat the antagonistic affection. The best means to be adopted for this are cold immersions or lotions. These means are especially useful when any cerebral accidents supervene. The accidents which accompany the eruption are (in the front line,) delirium, coma, insomnia, subsultus tendinum, tympanitis, retention of urine. In rather severe scarlatina, the delirium lasts from one to fifteen days; in more moderate cases, from two to eight days. Small doses of calomel should be given, castor oil, saline purgatives, very light diet, decoctions of arrow-root, of rice, bread, etc., in one word, restorative drinks. If the accidents augment, if the skin becomes very dry, the heat very great, if the delirium continues, then must be applied the medication capital, viz., cold affusions. It is by no means easy to overcome the prejudice that children who have an eruptive disease, should be kept as warmly covered as possible. When such a prejudice is strongly entertained, how can a physician who has grown old in practice, and with still greater reason, how can a young physician propose and enter upon this treatment? As for myself, I have administered these affusions for twenty years, and I loudly proclaim that it is the most powerful medication against those nervous accidents which are so terrible. To advise their application in the ordinary cases of scarlatina would be the height of absurdity. We counsel them when the nervous accidents are such that we suppose the patient will die the next day, even this day in such a condition that there are nine chances out of ten that he will die. However great the repugnance may be which is entertained against this medication, accustom yourselves to proceed straight before you

with it. Make the parents understand well that death is at hand, imminent, and when you will have overcome their resistance, you will proceed in this manner : Place the patient naked in a bath-tub : have two buckets of cold water at eighteen or twenty degrees cent. (=64°+ or 68° Fahr.) As the skin of the human body is forty or forty-one degrees cent. (=104° or 106° Fahr.,) and in the pyrexia of scarlatina sometimes even higher, there will still be a difference of temperature of twenty or thirty degrees cent. (=36° or 42° Fahr.); all of the water must be poured over the head, body, and breast, which should occupy half a minute. The patient should be covered and put in bed without being wiped. The results are felt immediately ; the pulse which was supposed to be at one hundred and sixty beats, falls to one hundred and forty, one hundred and thirty, one hundred and twenty pulsations. The pulse looses a quarter, a half, and even one-third of the pulsations which it had over the normal state, at the same time it increases in fulness. The skin which was dry and arid becomes in a quarter of an hour soft and moist; a moisture which is in part derived from the water imbibed by the skin, and in part from its function being renewed. At the same time there will be a diminution in the delirium, and in the nervous phenomena, and in less than ten minutes after the patient has been taken out of the bath, the eruption, which was livid, becomes rose-colored. Unfortunately, after five or six hours the nervous accidents reäppear, with a greater or less intensity. It is necessary then to repeat the affusion, and this time the parents will demand it of the physician. I have never found any repugnance to the subsequent affusions. It will be necessary to renew them two, three, and four times a day, and to make them very short ; at the same time give the patient the restorative drinks or very light aliments.

When there is no bath-tub, lotions must be resorted to, which better please the parents since they find the skin dry, they consent to the application of lotions of tepid water; sometimes they ask for them themselves. The child should be placed on a cot, and a sponge dipped in water twenty or twenty-one degrees, cent. (about sixty-five degrees Fahr). The sponge should be squeezed and passed rapidly over the body, and proceed as before. A little vinegar or lime-juice may be mixed with the lotions, for the parents are pleased with this, and will resort often to these lotions, which they can make themselves. Lotions are just as good as the affusion, and besides do not excite that repugnance which has been spoken of.

Such are the best means to contend against the nervous accidents, not forgetting to administer at the same time the restorative drinks, the preparations of cinchona, with a little sal ammoniac, etc. It is only a question of time, for if you contend successfully against the nervous accidents, the scarlatina will march alone. So much for the nervous form.

Let us proceed to the angina of scarlet fever. Generally, it is not very grave; it is the necessary appendage to scarlatina whose progress it follows. But in some cases the amygdalæ are covered with a white coat; there are excoriation and ulceration. Ordinarily they will be cured by local applications. When the angina assumes a certain gravity, take a solution of equal parts of honey, and pure chloro-hydric acid, touch the tonsils with a hair-pencil dipppd in it, avoiding the tongue and teeth. Repeat this two or three times, and when the angina has diminished, apply the acid more diluted, or indeed simply one part of calomel to thirty or forty parts of sugar. When the ganglions of the neck are involved in the disease, there is generally little or nothing to be done, either by anti-phlogistics, emollients, or irritants. Abscess, gangrene, etc., general accidents, may carry off the patient. It is necessary to guard against these engorgements, by treating early the pharyngeal angina.

For the anasarca of which we have spoken, I know nothing better than repose, emollient drinks, low diet, and light laxatives. It will, perhaps, be cured without these, and when no albumen is found in the urine, I make use of digitalis. When the disease becomes chronic, I know of nothing useful. I have seen two cured out of one hundred and fifty or two hundred cases which I have seen. It remains to speak of one accident of the anasarca, the eclampsia, which is sometimes frightful in scarlatinous anasarca; and which is fatal when the anasarca has come on in a dreadful manner. In the most simple cases, where children of from six to eight years have anasarca, there is nothing of great benefit to be done. In the eclampsia with a continued form, the head of the child may be elevated and the carotids compressed. (Resorted to from analogy; see the work of M. Dezeimeris upon compression of the carotids in facial neuralgia). This does not always succeed, but I have seen it of benefit in quite a number of cases. Concurrently with this, one or two centigrammes (one-fifth or two-fifths of a grain) of belladonna may be administered in an aromatic julep by small teaspoonfuls. There is nothing special to be said as to the treatment of the skin and the mucous membranes.

ART. III.—*History of the Diseases of Craven's Creek.* By JAMES PEEBLES, M. D., of Lowndes County, Mississippi. (*Concluded.*)

FROM September, 1853, until the following January, there was but very little of any kind of sickness; but in the latter month, (January, 1854,) there was a very tedious case of typhoid pneumonia, which resembled very closely the cases that occurred in the winter of 1853, and required stimulants from the very inception of the disease almost to its termination. In one respect this case differed from those of the previous winter, viz: there was more marked poverty of the blood, or at least a greater want of coagulability in it; about the fourth day of the disease it became necessary to apply cups to the patient's chest, and when they were removed it was with some difficulty that the flow of blood from the cuts of the scarificator could be checked; afterwards there was considerable oozing of blood for a week from a blistered surface. I have frequently met with this condition of the blood in what we term, in this country, typhoid diseases, and so far as my experience goes there is one remarkable symptom that invariably accompanies it, viz: a very irregular pulse as to its calibre, being sometimes very weak and thread-like, and perhaps in six hours becoming full and bounding, then in as short a time dropping down again, and so on, perhaps for weeks, or till boils, abscesses or carbuncles make their appearance.

In February there was a good deal of influenza, ordinary pneumonia, and one case of typhoid pneumonia. All the influenza cases were taken with, and suffered throughout the attack from, pain and a sensation of fullness in the epigastrium. These cases lasted ordinarily about five days, and invariably had a weak though not a rapid pulse and a cool skin. Opium, though highly useful in this disease, would not raise the pulse nor bring warmth to the surface, as it did in the typhoid pneumonia cases of 1852. As spring advanced, though the cases were much fewer, yet they became more grave, protracted, and allied to typhoid fever.

A. J. K. was taken May 4th with what he thought to be a common cold, but not getting any better of it, I was sent for on the 7th. He then had slight catarrhal symptoms, and complained of pain and fullness in his stomach. I thought he had the same sort of influenza that prevailed a short time previous, the only perceptible difference being that his pulse was of the proper calibre and his skin warm. After the action of an emetic I gave him an opiate and left

him on camphor and composition tea, with the intention not to return; but in three or four days I was again summoned; his stomach symptoms had then left him, but he still had light fever and complained of colicky pains. As others of the family were then suffering from chills I hoped that his case was in some way affected by malaria, and accordingly he was put on quinine, but without benefit to him. His fever continued and typhoid symptoms were soon developed. His fever lasted in all about seven weeks, and terminated by elimination in boils and abscesses.

R. B. E. was taken August 30th, as he thought, with chills, and after the action of a dose of blue mass, saturated himself with quinine; but his fever, though very light, continued and he soon begun to complain of fullness in his stomach, and his pulse became rather feeble; he also had slight catarrhal symptoms. Gave him an emetic of lobelia, which brought on a reäction in his pulse for a time, but did not entirely relieve his stomach; as he expressed it, "there was something deep down that the medicine had not reached, and he felt like he needed a thorough *cleaning out*," and accordingly the emetic was repeated with better success; but his distress returned and he was treated with morphia, and cupping over the epigastrium. His fever lasted five or six days, but it was three weeks before he was fully himself again.

There was a good deal of fatality this year among infants, during the spring and summer, from acute hydrocephalus supervening upon whooping-cough.

It is perhaps remarkable that there were four cases of pneumonia in July, acute and uncomplicated; they were not at all produced nor modified by malaria, nor did they exhibit the least typhoid symptom, but required and yielded to active antiphlogistic treatment. Three of the cases were in one family and originated within a very few days of each other.

I. N. B. was taken about noon, July 12th, with shivering, pain in his left side and fever; he soon became delirious. I was sent for early at night; he then had but little pain, though almost his entire left lung was inflamed; his fever high; his respiration embarrassed, and his cheeks had a well marked pneumonic flush. As he lived near the river, and it was at the season of the year when malaria is active, he was kept thoroughly under the influence of lobelia through the night, in hopes there would be a remission in the morning, and that something could then be done for him with quinine

and local depletion; but with morning the remission did not come; in fact his oppression had rather increased, and he was then bled from the arm until a decided impression was made, which very much relieved his respiration and abated his fever and delirium; but he still required to be kept on lobelia and to be extensively cupped twice a day for several days; then once a day, and, finally, to be blistered over the entire left lung.

The treatment in the above case is detailed, not that it is at all novel or original, but that it is a very rare thing to meet with a case in this country, and in the summer season, that requires and will bear such active depletion. In this instance the subject was young; he lived near the river, was very stout, had a sanguine, vital, real hog and hominy temperament, possessed a remarkably large living apparatus, a digestion like an ostrich, and a constitution like a sawmill, or a jackass at least.

A word relative to the use of lobelia. This herb possesses much greater medicinal virtue than a majority of the profession are either aware of or willing to grant it. It certainly has soporific and sedative properties, and just as certainly has a specific effect in controling inflammation of the lungs. As an emetic it is mild and possesses another advantage over all others, viz: it does not purge—Wood and Bache, and all other authorities, to the contrary, notwithstanding. That a great deal of the prejudice against this article both in and out of the profession, has grown out of its *empirical* use or abuse, there cannot be a doubt; but this is all wrong, and comes with rather a bad grace from that part of the profession who advocate the use of mercury, and who at the same time have the candor to acknowledge that the old Cookite practice was an abuse of a valuable drug, and a *scientific* warfare against life and health, which was in many instances as mischievous as the old Thompsonian heroic use of lobelia.

During the fall and winter there was pretty general good health. In the spring of 1855 there were a good many scattering cases of a singular form of head affection. A person would be taken with pain in the back of the head, sometimes extending down the nape of the neck, or along one of the sterno-mastoid muscles, which, in every instance that came under my observation, continued until it was relieved by free cupping, etc. How long this disease lasted when let alone I have no knowledge. I have known it to continue two weeks without evincing any disposition to terminate in health

naturally. In a few instances the fever was considerable, but generally it was very light.

About the first of June typhoid fever broke out and raged terrifically till December, within a radius of two miles from this point. During this time it fell to my lot to treat more than sixty cases, and out of that number seven died. Though the following is worse than the average of cases, yet it is a very fair type of the character of typhoid fever as it prevailed here during the summer and autumn of 1855; since that time it has undergone various modifying influences, and has finally worn out in this locality.

I. Z. W. was taken, July 1st, with pains in his limbs, slight fever, cough and a sensation of fullness in his stomach very much like that complained of in the influenza cases of 1854, but his dusky, confused countenance clearly indicated typhoid fever of a very grave character. Gave him an emetic and a dose of oil, put him on morphia and camphor, kept stimulating plasters to his chest, and "waited for the wagon;" but the "wagon" didn't happen to take the desired road, and the "ride" for about seven weeks was rather a rough one. As his cough continued an expectorant was substituted for the camphor; his fever continued regularly to increase, and by the end of the third week his delirium had become so inordinate, particularly at night, that it sometimes required more than one person to confine him in bed till he could be narcotised, to do which it usually required four grains of morphia, given in grain doses every hour or two. When in this extremely delirious condition he exactly resembled a person with delirium tremens, always fancied that some one was trying to kill him, and his violent efforts were made to get away from imaginary danger; he was also bathed in perspiration at such times; was very tremulous; his pulse very weak and extremely rapid, so that the morphia was always given in a pretty liberal allowance of brandy; in health he had made a full half hand at swiging the "critter," but at the time that he was taken sick he had not been on a "bust" for some time past, though he had very probably been imbibing to a small extent. He was very tympanitic. About the third week diarrhœa came on; several times there was slight hemorrhage from his bowels, and once it was alarmingly copious; he expectorated bloody matter and hawked up quantities of a fœtid bloody matter from his gums and fauces; his urine was bloody for a week. Several times his bladder became very much distended, and, not being able to empty it, the catheter was introduced, and a little urine and large quantities of gas escaped. About

the fifth week he seemed to be a living mass of boils, carbuncles and abscesses; while in this condition, and at his worst, his treatment was diet every four hours and a large tablespoonful of brandy every hour precisely, with twenty-five drops of laudanum in every fourth dose, except that from three to six o'clock every A. M. He was not disturbed, that being the time at which he was most disposed to rest. He finally recovered, though his convalescence was tedious, and it was six months before he was fully himself. This I believe is common in cases of recovery from typhoid fever; there is usually a kind of false plethora, but at the same time there is a great want of physical strength and energy and capability to endure fatigue, and there is also for six months sometimes an intellectual obtuseness. Every case that recovered this year did so by elimination in boils, etc., or by a critical rash. Of the seven fatal cases death took place in five from the fifth to the seventh days inclusively, and was by a mixed mode, but in every case it was preceded by opisthotonos; of the remaining two one died by coma during the fourth week, and the other by asthenia during the fifth.

What is the proper treatment of typhoid fever? Echo *ought* to answer—What? The course adopted by myself has already been incidentally mentioned, and opium and brandy are the helm and the sheet anchor. That the vaunted remedy—turpentine—may sometimes be useful is possible, but it or any other potent drug given indiscriminately is injurious. I have never known it to relieve even tympanites, and in my own hands it has never accomplished anything except mischief to the urinary organs. Those who advocate its use claim that it heals intestinal ulceration, and *therefore* cures typhoid fever; but really intestinal ulceration is but a fragment of typhoid fever, *as it existed in this locality from* 1851 *to* 1857. Where there is a tendency to asthenia opium is a capital stimulant, and where the pulse intermits it will probably sustain the action of the heart when nothing else will; also where there is flagging of the vital powers, and brandy is *relished*, it is *sure to do a great deal of good;* but in some very desperate cases where it was not relished, and for that and other reasons was not given, I have known the patients do firstrate without anything. The fact is, that in *acute* cases of the typhoid fever of this country a person may recover naturally from almost any condition short of death; yet much good may be done, besides the means already named, by keeping a strict lookout for local determinations and treating them promptly. The lungs particularly should be closely watched. The muscles of the arms and legs are

also liable to taken on inflammation, which if taken early is very easily checked by the application of strong rubefacients. I am in the habit of using the following: R. Tr. Caprie. ℥ x; Tr. Camphor ℥ iv; Liq. Ammon. Fort. ℥ ii. M.

Bed sores should also be closely watched for, as when once formed they are sure to have their own way and add very materially to the patient's sufferings, and, perhaps, something to his danger. By the use of a bed pan, in low cases, the patient saves his strength by not being compelled to leave the horizontal position, and in extremely low cases the head should be raised as little as possible in giving medicine or diet. In cases of painful micturiction relief may be given by an injection of morphia, and by warm applications, for which purpose an excellent thing is wheat bran, heated and moistened and put into a bag. Retention of urine may also be frequently relieved in the same manner, and the use of the catheter avoided. But where the leap is to the brain, and coma supervenes, it is well enough to "hang up the fiddle," for death will follow about as certain as two and two make four.

Do blisters encourage elimination in boils, etc.? I think they do, and I frequently apply them for that purpose when there is no other indication for their use.

What of quinine? Typhoid fever is sometimes periodical in its character, and this has induced myself, as well as others, to prescribe quinine, but I believe always without benefit. The fact is that periodicity in disease is not always so sure an indication for the use of quinine as many seem to think. Quinine will not cure otitis, yet the pain in that disease is frequently *perfectly* periodical in the sub-acute and chronic forms of the disease, and so it is in some nervous diseases that are incurable by quinine. That quinine will cure intermittent fever nobody doubts, yet *who ever saw perfect periodicity in an intermittent?* And by intermittent I mean the whole family of malarial fevers, from common chills up to the worst forms of the so-called bilious fever.

Typhoid fever, in a variety of shapes, continued to be the prevalent disease during 1856, and the last case that could be called by that name occurred in June, 1857, and during the summer months of this year there was a good deal of a violent form of dysentery, the average duration of which was about ten days, and in most instances there was the real narrow, red, typhoid fever-tongue; three of the cases died. During the same time there were, for the number of cases of intermittent fever, a very unusual amount of nervous

coma and spasms among children. In the fall and winter there was quite a number of cases of a mild form of pneumonia, and one very violent and fatal case.

In the foregoing articles I have endeavored to present a very general view of the most important diseases, or those that have been most prevalent at different times in this locality, and to give the origin and course of what has been termed, in this country, typhoid fever, typhoid pneumonia, typhoid influenza, etc. Many diseases that are common everywhere, and many that are incidental to all southern and particularly to malarious districts, and have not been more prevalent here than in many other parts of the country, have not been named at all, and some only incidentally, and among those diseases there is a class of much interest and importance to the country practitioner, for it is sure to be met with again and again, affords the very best proofs of the great medicinal virtue of certain drugs, and positively proves certain physiological laws. I allude to the complications and the secondary effects of intermittent fever.

Among the complications, or rather the symptoms of intermittent fever, one which frequently causes great and unnecessary alarm is spasms in children; it is certainly shocking to look upon, but is as certainly amenable to the proper treatment of intermittent fever, i. e. to huge doses of quinine and opium, and the *very worst cases may be relieved in an hour by a decided dose of the latter.* Try it, "ye of little faith," and do so without "fear and trembling," for you will surely be doing right. I have given to a child a year old as much as twenty drops of tr. opii at a single dose, and had the satisfaction to see the little sufferer *rouse up* in an hour or two entirely relieved of every spasmodic symptom and its fever nearly gone; but except in very violent cases from five to ten drops is sufficient for a child a year old, and, if necessary to be given that way, is nearly as prompt by enema. An adult takes fever, has severe head and back ache, becomes nervous and very restless, and nobody hesitates to prescribe an opiate. An infant takes fever and the *very same state of things amounts to spasms* in its case, and the first thing usually done is to souse it into a tub of hot water; then comes a purgative dose; the medicine fails to operate; worms or some intestinal obstruction are blamed for all the mischief; a stronger dose is given, but with no better success, (for *it is a very difficult matter to purge a child with spasms*, and not always an easy matter during a paroxysm of an intermittent when there is no spasm;) after a time there is an inter-

mission, the fever goes off and the spasms cease, the medicine purges it, and the little fellow then gets quinine and is soon well; but he is usually a little jaundiced and has to take calomel a day or two, and is very frequently compelled to swallow a vial or two of vermifuge. An excellent preventive of spasms is a large dose of quinine at the very inception of the fever; it very much abbreviates the paroxysm as well as allays nervous excitement; one advantage it has over opium as a preventive is that it does not interfere with the action of purgative medicine, but facilitates it, inasmuch as it abates and abbreviates the fever.

It is a fact that spasms are less frequently met with in the remittent forms of fever than in those cases where there is a decided intermission in chills, and it is also a fact that the worst complications and gravest symptoms of intermittent fever are most frequently met with in chills. It is in chills that we have effusion on the brain, and it is chills that lay the foundation of and develop a number of chronic organic diseases, enlargement of the spleen and liver, melæna, organic disease of the heart and lungs, anæmia, etc., etc., and it is chills that stunt the physical and intellectual growth and energy, and thus make the children of stout and intelligent parents stupid, dwarfish, pale, pot-gutted, splinter-legged, and of comical appearance, something like a jug stuck on stilts.

The most frequent chronic organic disease met with in this country, which is the invariable result of chills, and that follows to a greater or less degree every chronic case of chills, is hypertrophy of the spleen. So common is this disease in some parts of this country that I opine it would be a difficult matter to find a white person who has lived five years in the Bigbee valley, and within three miles of what is known as Lindsey's Ferry, who has not an enlarged spleen. I believe it is never met with in negroes. Little more than a year ago this fact was mentioned to me by Dr. J. C. Nott, and since that time I have examined every negro that I could get a chance at, without finding a single case of enlarged spleen, and many of those negroes would have had it if malaria could have produced it in the negro race; for they live, and some of them have been born and raised, where a white person *could not live* but a very few years without having a big spleen, viz: in the immediate vicinity of Lindsey's Ferry, which I will venture to assert is as unwholesome and malarious a locality as can be found within the state of Mississippi. I have been informed by old settlers that thirty-five years ago, when the country was but little cleared up, the Indians

could not or would not live there on account of sickness, and at that time the country was everywhere else comparatively healthy. About twenty years ago there was something of a town established at the Ferry, and in 1843 the inhabitants literally died out; others flocked in to fill their places, but in the next ten years disease and death greatly thinned their numbers, and during the fall and winter of 1856, pneumonia and typhoid fever pretty well wiped out the white population on the west side of the river. In 1843 they died of grippe, but intermittent fever was the ulterior cause of death; intermittent fever had worn out the constitution and but little else was sufficient to terminate life. Grippe prevailed elsewhere to an equal extent, but was only fatal around Lindsey's Ferry, and so it was with the typhoid fever of 1856; besides, during 1855, when there was so much typhoid fever, there was no intermittent except on the river; there malaria continued to be the stronger poison, especially around the Ferry, and yet negroes live there and do not have enlarged spleens; but they are perhaps as much addicted to intermittent fever as white people.

In connection with spasms should have been noticed nervous coma; this, too, is a symptom of intermittent fever that occurs in children, but usually in those eight or ten years old and upwards, whereas spasms mostly take place in those under five years of age. A child has an intermittent, is nervous for a time during the exacerbation of fever, then goes to sleep and so passes into a stupor, and with the stupor comes on profuse perspiration, cool skin, a weak pulse, and blueness of the lips and finger nails. Opium and quinine, of course, are the remedies for this state of things. Six or eight months ago I treated a case in a boy fifteen years old. He had fever two or three days and went into a semi-comatose state; had been in that condition about twelve hours when I first saw him; he could then be partially aroused, but not enough to swallow anything; there was slight opisthotonos, and profuse perspiration, but his skin was warm enough and his pulse of good calibre. Gave him by injection half a drachm of quinine and seventy-five drops of laudanum, and repeated the dose in four hours; in four hours more his fever was gone, and he had roused up quite rational; as he said that he did not feel the quinine, I gave him ten grains more at the other end, but it proved to be ten grains too much, for he did not get to sleep again in over forty-eight hours.

Lowndes County, Miss., March, 1858.

ART. IV.—*Cancer of the Cæcum.*

CAMDEN, Ouachita Co., Ark., Dec. 25, 1857.

GENTLEMEN: My young friend, Mr. Wm. Brown, now a resident student in the Medical Department of the University of Louisiana, will deliver to you, for inspection, a morbid specimen, which was taken from the body of Rev. Stephen McHugh, late Missionary Pastor of St. John's (Episcopal) Church of this city.

Mr. McHugh was a native of Ireland, but for the last twenty-five years a resident of the United States. He was of medium size, of sanguine temperament, and was fifty-four years of age. His circumstances were comfortable, and he was temperate in all his habits. With the exception of an occasional attack of remittent, or intermittent fever, he enjoyed a fair proportion of health.

The history of the disease which terminated this—"one of the noblest specimens of humanity"—a man who stood preëminently high in all the relations of life—may be summed up in a few words. The reverend gentleman dated the commencement of his disease two years previous to the 1st of May last, but thought so little of it that it caused him no concern or uneasiness. In his own words: "There was a very slight pain, with a little hardness, in the lower part, and in the right side of the abdomen." Under the use of mild aperients, the abdominal symptoms remained, apparently, stationary, until sometime in the month of January of the present year, when a severe shock, from a fall upon the ice, seemed to impart to the disease renewed activity.

Notwithstanding the disease had remained so long in a state of inactivity, there was, evidently, a slight, though well marked cachetic condition of the system. From this time till 1st of May, the disease made slow but steady progress, and was now a plainly visible tumor, which, from its locality, and partially mobile character, together with the effects it seemed to be producing upon the function of defecation, I had no hesitation in referring to the cæcum. There was a dull heavy constant pain, with occasional momentary keen lancinating pains darting through, and from the tumor in all directions. There were frequent bloody or blood and mucous discharges. The urine, so far as could be ascertained by simple inspection, was perfectly healthy, and remained so until within a few days of death.

The disease was now, for the first time, subjected to a course of direct medical treatment. The various preparations of iodine, chalybiates, hydro-chlorate of ammonia, with counter-irritants, etc., were

of no avail. The disease (tumor) with the increasing cachexia continuing to progress "*pari passu*," up to the fatal termination in October.

The examination (post mortem) took place a few hours only after death, in presence of Drs. Hobson and Bacon.

The body was extremely emaciated, giving to the tumor a great degree of prominence, and a plainly visible outline through the thinned abdominal parietes. There was no discoloration of the external surface ; but there was a slight adhesion between the integuments and anterior surface of the tumor. The tumor itself occupied the whole contour of the *caput-coli* and embraced portions of the ascending colon, and of the ileum.

Surrounding the tumor, and apparently fixing it in an immovable position, was an extensive morbid deposit, of a similar nature, in appearance, to the tumor itself. This morbid accumulation, however, was in no way directly connected with the principal diseased mass, but seemed rather to be the result of mesenteric infiltration, as it was embraced within the folds of this membrane. The whole mass, when first exposed to view, presented the appearance of a vast fatty deposit, having an irregular nodular outline or surface, and was resting directly upon and below the right kidney. This latter organ was greatly enlarged, without structural change, (simple hypertrophy). The tumor, on its anterior aspect, or that portion which was in direct contact with the abdominal parietes, was nearly smooth, and much discolored, particularly at its lower part, with bloody infiltration, (ecchymosis) which, I infer must, if not a post-mortem appearance, have occurred at a very late period in the course of the disease. The interior of the tumor, or, properly, the cavity of the cæcum, was much enlarged, and extensively ulcerated ; the surface being exceedingly rough, and covered with a dark, nearly black, tenacious substance, which appeared to be the debris from the disorganization which was in progress, tinged with some quality of the fecal matter, which was continually passing over it. This substance could not be entirely removed by means of simple ablution. In the lower anterior wall of the tumor the ulceration had well nigh effected an opening ; and it was here that nature had directed her most powerful efforts to prevent an extravasation of the intestinal contents, into the abdominal cavity. Adhesions seemed to be rapidly forming between this weakened portion of the tumor, on the abdominal parietes ; and had life been sufficiently prolonged, there is every probability that an external opening would have formed at this point.

In consistence, the walls of the tumor were about equal to those of the healthy human heart—the cut surface assuming a concave appearance.

Respectfully, your obedient servant,

J. J. McELRATH.

Messrs. Editors N. O. Med. & Sur. Jour., N. O., La.,

NEW ORLEANS, January 8, 1858.

My Dear Sir :—At the request of Dr. Chaillé I have examined the accompanying communication and pathological specimen. The latter seems to be a cancerous mass surrounding the cæcum and termination of the ileum. It is a frightful disease.

I am truly, yours, etc.,

J. C. NOTT.

To DR. B. DOWLER.

ART. V.—*Chloroform in Puerperal Convulsions:* Reported by DR. J. D. RICHARDSON.

AMELIA, (negress), aged fifteen years, complexion griffe, short in stature, well formed, eight and a half or nine months advanced in pregnancy, having complained of pain in the head for several days. On the morning of the 16th of February, 1854, she had pains of a bearing down character at seven o'clock, and at eight o'clock she was seized with convulsions. I was summoned, and arrived at twelve o'clock, noon, during the interval of a paroxysm. Pulse hard and bounding, skin hot, insensible, bleeding at the mouth from laceration of the tongue by the teeth.

Treatment.—Venesection was practised until the volume of the pulse was reduced; stimulating frictions to the extremities, and a cathartic enema, which latter produced a free evacuation from the bowels. At four o'clock, P. M., the convulsions still recurring at irregular intervals, repeated the venesection, cathartic enema, etc. which reduced the force and frequency of the pulse, and obtained a liquid evacuation from the bowels, interspersed with scybala. At seven o'clock, P. M., ascertained by vaginal examination that the head presented, and at eight o'clock she was delivered of a full grown child, during the existence of a most violent convulsion. In

consequence of the immediate and firm contraction of the uterus, the placenta was expelled in a short time ; considerable hæmorrhage ensued before its expulsion, so much that I was of the opinion that the loss of blood would moderate, if not entirely break up the convulsive action. In this I was disappointed, and I thought that the paroxysms occurred with more violence until eleven o'clock, when I most fortunately resorted to chloroform, which was administered by inhalation during the existence of a convulsion. It produced a most happy effect ; two or three inspirations were followed by perfect quietude and repose, which with another administration of the same remedy in an hour, resulted in complete recovery, with none of the untoward symptoms that usually accompany this affection.

I am satisfied that this case would have proved fatal had it not been for the administration of this potent remedial agent. I regret exceedingly my inability to procure the chloroform until the case had progressed so far. Had I a similar case, I would exhibit chloroform at once (premising venesection). I have never (up to the date of this case) heard of chloroform being used in eclampsia. The epileptic condition would seem to contraïndicate its use ; yet I resorted to it in this case because I thought it was a desperate one, and in my opinion no other article with which I was acquainted afforded a prospect of recovery. It is my impression that no quantity of blood abstracted (compatible with life) would have benefitted this girl, inasmuch as I bled her copiously twice, with manifest reduction and softness of the pulse, and a large quantity followed the birth of the child. The head of the latter had a sugar-loaf appearance. Its occipito-mental or oblique diameter measured seven and a half inches, the other diameters were normal.

I had another case of eclampsia a few days since, (primipara) feet presentation, which resulted favorably from similar treatment. In each the child was born dead.

Ashwood, La., Feb. 10, 1858.

Art. VI.—*Case of Lead Poisoning, Illustrating the Efficacy of Sulph. Acid as an Antidote.*

Sometime in the summer of 184–, I was called at eleven o'clock, at night, to visit one S. Barnes, a strong, robust man, of sanguine temperament, plethoric habit, very corpulent, about fifty-five years of

age, and much addicted to the use of ardent spirits, who, through some gross and strange mistake had completely swallowed half a quart of a strong solution of the *sup. acet. plumbi.* I arrived in half an hour afterward, and immediately recognized that terrible and alarming train of phenomena consequent upon the operation of such a poison upon the human organism—they were as follows: Great abdominal pain, frequent fits of syncope, radial pulse exhausted, surface bedewed with a cold, clammy moisture, hiccup, countenance expressive of great anxiety and distress, sense of weight and oppression about the præcordia, with a feeling of tightness or stricture across the chest, wrists completely paralyzed.

Prescription.—Sulph. magnus. ℥vi, to be dissolved in a pint of warm water; dose, one-fourth of this mixture every half hour. The patient had scarcely taken the second dose when all the painful and truly alarming symptoms were speedily arrested, as if by a charm. The only thing he complained of after having awoke from a long and refreshing sleep, was a degree of soreness and tenderness of the abdominal parietes, which yielded to a few soft pills of opium. Thus terminated this case. The life of the patient was saved by the sulph. acid. of the sulph. magnes. combining with the lead, forming thereby an insoluble compound, totally inert, and incapable of exerting any influence upon the animal economy.

G. E. ELMER, M. D.

MARKSVILLE, LOUISIANA, 30th January, 1858.

DR. BENNET DOWLER:

Dear Sir,—I send you a case of lead poisoning that occurred in my practice some years since, taken from my note-book. It may be fraught with some interest. Should you deem it worthy of a place in the *N. O. Med. and Surg. Journal*, you are at liberty to give it insertion. I was practising in what was then a wilderness, in fact in the very wilds of Louisiana.

Unimportant as it may appear, I think it establishes a very useful principle in therapeutics, which is, not to spend or throw away, in similar cases, valuable time in fruitless and unavailing attempts to dislodge the poison from the stomach, by an emetic or by a cathartic, but to have recourse immediately to the great, the proper antidote. I omitted to state that my patient had, before I was called, taken an active emetic and cathartic without relief, but much to the exasperation of his sufferings.

I am, with high respect yours truly,

G. E. ELMER, M. D.

ART. VII.—*Cerebro-Spinal Meningitis:* An Inaugural Dissertation presented to the Medical Faculty of the University of Louisiana (Session of 1857-8): By JESSE M. PACE, M. D.

THIS disease made its first appearance as an epidemic in the United States in the year 1840. It also prevailed in Europe about the same time, though probably long known in Europe previous to this, and described under various names, none of which suggested its true character. It is confined mostly, if not entirely to the Southern Hemisphere. We have no conclusive evidence that it ever exists in a colder climate than south of the thirty-ninth parallel of latitude, and especially in localities that are infested by autumnal fevers. It frequently follows relapsing intermittents, pneumonias and measles. It attacks persons of every age, but more particularly the younger portion of the population from five to fifteen years of age. It is not confined to either sex. In some countries it has been described as attacking boys exclusively. As it occurs in Arkansas, females are equally liable to its invasion. Blacks and whites are both subject to this disease.

It makes its appearance with pain in the head, of a continuous or remittent character, sometimes confined to the brow, temples or occiput; pains of the face, of the spine, of the muscles of the joints, of the extremities; occasionally vertigo, especially when the patient assumes the erect posture. Sometimes the disease is attended at the onset with impairment of strength, though generally the strength remains good until several days have elapsed. The appetite is generally good at the beginning of the disease, though sometimes affected or entirely lost; occasional nausea and vomiting. The attack frequently makes its appearance with a chill of varying duration and intensity, lasting from a few minutes to several hours, with a weak and vaccillating pulse, soft, scarcely ever tense, occasionally remittent and indistinct, sometimes accelerated but often very slow. In some cases there is more or less disturbance of the capillary circulation, indicated by cold surface and petechiæ, with low muttering delirium. This stage is soon followed by reäction, flushed face, accelerated pulse, hot and dry skin, and often with a wild and frantic expression of the countenance. The pupils are either dilated or contracted, sometimes one dilated and the other contracted; a violent pain in the head, generally over the brow, causing the patient to utter plaintive cries, at the same time carrying his hands to his head, and rolling the latter from side to side, which motion is almost perpetual. The respiration is sometimes

increased in frequency, sometimes natural, occasionally stertorous and slower than common. The tongue is frequently coated white or yellow, though sometimes pale and flabby, and its edges indented by the teeth; difficult articulation, indistinct and laborious. There is generally an acute morbid sensibility of the surface, the slightest touch causing the patient to utter cries or causing violent convulsions; the weight of the lightest counterpane is frequently intolerable. The muscles of the extremities and those of the trunk present pathognomonic symptoms of the disease; there is rigidity of the muscles of the head and neck; the head is drawn back upon the spine, the patient not being able to place his head in an erect position, neither can an assistant, without straining or rupturing the muscles of the neck, or causing violent pain to the patient. If the patient is called by name or disturbed by any unusual noise, he is frequently thrown into violent convulsions or tetanic movements of the muscles of the face, and also those of the upper extremities. It is frequently with the greatest difficulty that a patient can drink water from a glass without assistance, and very probably if offered to him he will seize it with a convulsive grasp not unlike a person suffering from delirium tremens, and in attempting to get the glass of water to his mouth will waste the greater portion of it. Occasionally there is more or less motion of the lower extremities, the patient throwing them from side to side, so that it is frequently almost impossible to keep the cover upon them.

Sometimes there is rigidity of the muscles of the back, causing the spine to be bent like a bow, constituting opisthotonos, and sometimes lateral curvature, though very rarely, constituting pleurosthotonos, and occasionally emprosthotonos or forward curvature. Paralysis exists in some few cases. The period at which the tetanic symptoms make their appearance is various, sometimes appearing very early, then again not until late in the disease. Delirium is a very common symptom of this disease, coming and going at short intervals from the beginning of the disease until it ends favorably or sinks into deep coma, the prognosis of which is doubtful. In some cases the patient sinks into a soporific state from which he scarcely can be aroused, with a diminution of tactile sensibility, which is also a very unfavorable symptom; such symptoms appearing, death is almost inevitable.

Sometimes there is tinnitus aurium, and occasionally inflammation of one or both eyes. There is generally more or less thirst, irritability of the stomach and tympanitic distention of the bowels

(which is frequently relieved by a cathartic). Constipation is generally present, though in the more malignant forms there is an incessant discharge from the bowels, which if long continued becomes thin and watery. Sometimes the disease makes its appearance with convulsions of a most frightful character, a symptom much to be dreaded, as it frequently requires personal restraint to protect the patient from injury. Occasionally the patient assumes a semi-comatose condition, moaning incessantly, grinding his teeth, and crying continuously. The pulse in this stage is generally accelerated, being from one hundred and forty to one hundred and fifty per minute, generally full, though occasionally weak, and unless soon destroyed by the severity of the attack, the patient sinks into deep coma, with a slow and weak pulse, difficult articulation, tongue coated brown, the teeth and lips encrusted, involuntary discharges from the bowels ; the bladder becomes distended with urine, and the patient having no control of the sphincter of the bladder, the urine is allowed to escape guttatim, and, at last, death closes the scene of the miserable sufferer.

The duration of this disease is variable, though the generality of those who die from it succumb about the fourth day, sometimes in twenty-four hours, and occasionally, though rarely, patients survive until the fiftieth day. This disease may run its entire course in a few days, or it may be more protracted and then terminate in convalescence, which, in this disease, is generally protracted and frequently the patient is cut off by some secondary disease, though some recover and again enjoy perfect health. The intellectual faculties, in some cases, are entirely destroyed, the intellect which is so characteristic in man, giving him precedence above the balance of God's creation, is abolished. The brain, the dome of thought and the palace of the soul, suffers intellectually as well as physically in this devastating disease, placing man upon an equality with the lower orders of animal creation ; he has no longer the pleasure of basking in the sunshine of progressive science ; he drags out a miserable existence and dies. In some cases the mental faculties remain intact.

There is a little girl in my immediate vicinity, about seven years of age, who recovered from an attack of this disease of about eight day's duration, and who now enjoys perfect health, though previous to an attack of this disease, she suffered from a frequent recurrence of intermittent fever, which, when checked upon her would return again in a few days; she also had an enlargement of the spleen,

which is commonly denominated an "ague cake," an affection of very frequent occurrence in miasmatic districts. Since she had an attack of meningitis, she has been entirely free from intermittent fever and the abnormal size of the spleen has disappeared. Her convalescence was slow and protracted.

The diagnosis of this disease is very difficult, on account of the variety of its symptoms, no one of which could be strictly considered pathognomonic. The prognosis is generally unfavorable as the disease has terminated fatally in the majority of cases. When the symptoms do not assume a formidable appearance at the beginning, such as prostration, a weak and vacillating pulse, petechiæ, delirium, convulsions and coma, the prognosis may be considered for the most part tolerably favorable, but if marked collapse ensue, and all the symptoms enumerated supervene, the prognosis is generally unfavorable.

The anatomical lesions produced by this malady are chiefly confined to the meninges of the brain and spinal marrow. The coagulum of blood is generally large, with an excess of fibrine. The pia mater of the brain is always deeply injected with blood, and the sinuses are generally full and turgid. The dura mater is sometimes apparently unchanged, occasionally dark spots may be seen upon it denoting capillary congestion. The arachnoid is generally dry in appearance with occasional thickening of the membrane, with also an effusion of a creamy looking fluid beneath, between that and the pia mater. The substance of the brain is sometimes of a pink color, with an effusion into its ventricles resembling pus with an admixture of lymph, and also found in great quantities at the base of the brain, and occasionally forming almost an organized membrane. Sometimes there is more or less effusion about the optic commissure. The effusion into the brain no doubt causes the coma which not unfrequently attends an attack of this disease.

The anatomical lesions of the spinal cord are about the same as those found in the brain, that is to say, congestion of the pia mater with more or less effusion between that tissue and the arachnoid with thickening of the latter, and sometimes with softening of the spinal cord from the occiput to the cauda equina. The lesions found in the alimentary canal after death, are occasionally slight; the stomach and bowels showing very little sign of disease; though very frequently the lesions are about the same as those found after death from typhoid fever, especially if the case assumed a typhoid type which it not unfrequently does. I believe it is generally an

established opinion that the disease of the alimentary canal is a secondary and not a primary disease, though it very frequently has a close connection with typhoid fever, and Professor Wood, of Philadelphia, says it frequently occurs in connection with the malignant forms of typhus fever. In some cases there is reddening and softening of the mucous coats of the stomach and bowels, and also thickening of the same. The glands of Peyer and Brunner are occasionally found to be diseased ; they become roughened, enlarged, and occasionally ulcerated. The mesenteric glands are sometimes implicated in this disease, especially those that correspond with the solitary and agminated glands of the lower portion of the ileum.

The causes of this disease are very obscure, and will be a topic for future inquiry. It frequently appears to be modified by, as well as complicated with other diseases. In every locality it shows a remittent character more or less apparent. I have seen the disease show a regular remittent type every morning with an exacerbation of fever in the evening ; occasionally the remittent character is so apparent and sensible to the patient that he flatters himself that he is getting well, but this state is soon followed by another exacerbation of fever, destroying the life of the patient. Whether the disease arises from the same cause as remittent or malarial fever is a question not yet decided. It also sometimes exists after an epidemic of erysipelas, and also in localities where traumatic and idiopathic tetanus is prevalent. We have well authenticated facts indicating its non-contagious character.

The modes of treatment in this disease have been various, none of which have been very satisfactory, the disease terminating fatally in the majority of cases. At the beginning of the disease, in the cold stage, mechanical means must be resorted to to restore the natural temperature of the body, more especially that of the lower extremities; warm bricks and bottles of warm water are to be applied to the patient, and if the temperature is not restored, more energetic means must be adopted—as friction, at the same time applying ammonia, turpentine, also mustard plasters to the ankles, calves of the legs, to the thighs, the epigastrium, until the warmth of the body is restored. After reäction and febrile symptoms arise, blood-letting is resorted to if not contraïndicated, either general or local, according to the age or state of the patient. Some prefer general blood-letting, extracting blood until the patient is on the verge of syncope; others prefer cupping applied to the temples, to the back of the neck, along the course of the spine, as long as

blood-letting is indicated ; it generally relieves the pain in the head, the patient falling into a sweet repose, though probably soon to be aroused by the returning pain. Mercury is a medicine very much to be relied upon, given in the mild forms of the disease so as to obtain its cathartic effects, but in the more malignant forms where there is a tendency to disorganization of the primary tissues and an organization of false membranes, mercury is given to obtain its constitutional effects in connection with mercurial inunction. Calomel is the preparation most generally used for internal administration. Tartar emetic has obtained considerable reputation as a therapeutic agent in this disease, appearing to have considerable control over inflammation of the various tissues. A recipe composed of the following ingredients alternating with mercury, appears to have a beneficial influence in this disease:* tartar emetic two grains, pulverized camphor one drachm, assafœtida two drachms, sweet spirits of nitre four drachms, and water or mucilage acaciæ six ounces, triturated together, and given in table-spoonful doses every two or three hours, probably oftener *pro re nata.* It checks the inflammation and also proves an excellent anti-spasmodic.

Blisters to the back of the neck and to the spine are of essential service; cold water to the head, keeping it up until the patient becomes chilly or otherwise disagreeable, seems to have an admirable effect in subduing the inflammatory action. Quinine and opium in combination is a practice that has of late attracted a good deal of attention, having at times apparently a specific influence over the disease, especially when administered early, being an admirable combination when the disease shows a remittent character; it brings down the pulse, relieves pain and promotes perspiration. Iodide of iron and iodide of potassium with the syrup of sarsaparilla, form an excellent preparation in convalescence from this disease, especially where there is inertia of the nervous system. I have seen an infusion of columbo combined with some aromatic, answer every purpose.

Convalescence from an attack of this disease, being very slow, patients, by error in diet, are liable to a return of the disease, and those who have had an attack should observe the strictest regimen for fear of the returning malady or some secondary disease which which would terminate sooner or later in death.

* Mercury must be partially or entirely suspended after obtaining its constitutional effects.

ART. VIII.—Reply of the President of the Board of Health to the "*Memorial to the Legislature*" published in the preceding Number of the *N. O. Med. and Surg. Journal.*

MESSRS. EDITORS:—I observe in the issue of your Journal for March, a Bill of Complaints filed against the Board of Health in the form of a *Memorial to the Legislature*, the whole *gravamen* of which may be reduced to the following heads: *First*, that the said Board in their Annual Report, treated with "disdainful silence" three cases of yellow fever, which occurred respectively in the months of June, July and August.

Secondly, That the officers of said Board had been invited to see the first case, which occurred in June, (Dominique Maillot, at the Circus Street Hospital), and that they refrained from so doing.

Thirdly, That the testimony upon which the Board bases its reasoning as to the origin of the fever of 1857, is untrue and falsely reported.

Fourthly, That the Board affirms the infectiousness of yellow fever and consequently denies its domestic origin.

I purpose as briefly as possible to examine into the character of these charges, and to see whether, in preferring them, the writer has not sought to indulge an unenviable rancour, which I shall not stoop to imitate.

As to the first accusation, that the Report of the Board treated the cases to which reference has already been made, with "disdainful silence;" I submit that there are distinct references both in the context and the mortuary records of the Report of these cases. In the former they are alluded to as cases of fever, while in the latter they are reported yellow fever. If the Board had reasons for believing the first case (that of Dominique Maillot) to be spurious, it was very proper, in endeavoring to unravel the intricate and uncertain problem of the origin of the fever, that it should avoid any allusion to it except as a doubtful case. Had it been more specifically referred to, it would have been necessary to state the grounds for the doubts, which influenced the mind of the writer of the Report. To have done this gratuitously, would have provoked the very controversy, and probably with increased petulancy, which has now arisen, and which it was desirable to have avoided. As this controversy, however, has been precipitated upon the Board, I will state the grounds upon which the Report reserved its opinions, as to the genuineness of Maillot's case. It must be remarked *in limine*, that although doubt was entertained as to the true character of this

case, still the benefit of the doubt was given to it, and it stands on record as a case of yellow fever. This has been the invariable practice of the Board, and if any departure from it have occurred, they have been in instances when no yellow fever being known to exist, certificates have borne the signatures of Deputy Coroners of death from this cause, unattested by a physician. In these latter instances, the Board has very properly suppressed the publication on such inadequate authority. To do otherwise would be needlessly experimenting with the fears and anxieties of the community at a season and in a subject, which every true-hearted and zealous citizen views with the utmost jealousy. For, however debauched may be the temper which views yellow fever in the aspect of a positive blessing, protecting us against the inroads of foreign or Northern hordes, to those who look to the growth of this Southern metropolis in wealth, power and commercial greatness, it is justly regarded as the direst calamity that wastes and consumes its hopes and its fortunes. There may be those who desire to deal in it as one of our summer staples, and who wish for its perpetuation as an established and traditional institution necessary to preserve our individuality of character; but the more patriotic and humane of our citizens can only see in its annual visitations a mortal blight to all those lofty aspirations which delight to scan the future, when freed from its baneful influences, we shall rise to the due proportions of our destiny as the first commercial city on this broad continent. Under a proper sense then, of the nature of its public functions, the Board has exercised a sound discretion, descriminating and suppressing any unprofessional report,* while it has endeavored, in giving every case returned by physicians, to give them, sanctioned by the name of the reporter. I need hardly remind you, Messrs. Editors, of the many grave difficulties that surround the diagnosis of an early case of yellow fever. We are all so accustomed to see in the recurrence of our hot seasons cases of fever occupying so debatable a position between the malignant forms of remittent and yellow fever, that no one, accustomed to the differences of opinion they occasion, ever thinks it a warrantable ground to hold his neighbor amenable to public censure because he happens to differ in judgment on the merits of the case. We have agreed to differ, simply

* A Dr. Goodall reported a case of fever subsequent to that of Dominique Maillot, and it was reported pernicious or some other fever by the Secretary, who alone has charge of the mortuary records. The rule sanctioned by the Board requires that the name of the reporting physician be attached to every early case of yellow fever, so that the community may attach the importance to it that it deserves. Why it was not done in this case, the Secretary can explain, if he thinks the "tempest in the tea-pot" is not already high enough.

because there is no common standard to which an appeal may be referred, which would be universally true. I do not know that perfect accuracy of diagnosis is physically or pathognomonically possible in an early case. Hence it is that cautious physicians, men who think before they speak, ordinarily reserve their judgment before openly pronouncing the existence of yellow fever.

The medical annals of yellow fever not only here, but wherever it has prevailed and found an historian, abound with evidence going to show the difficulties and contrariety of opinion attendant on an early and correct recognition of the genuine type of the fever. To this doubtless, are due the many and angry controversies that compose a part of its medical history. But without reference to these, the pages of your own Journal have put on imperishable record the truth of the observations I have just made, and what is all the more note-worthy and remarkable, in connection with the name of the writer of the present memorial. I find, on reference to page 449, vol. 3, of your Journal, a case in many respects the counterpart of Dominique Maillot. For example, it was the first case of yellow fever announced in the summer of 1846; the dead body was seen by the then President of the Board of Health, as was that of Dominique Maillot by myself; a difference of opinion arose between the reporter of the case and the President of the Board of Health; a publication in one of the city papers of the character of said case followed, and a denial of the correctness of that publication. The language is so noticeable, as being a part of the history of the times, as chronicled by the medical Pepys of our day, if I may be permitted the term, in alluding to the only true chronicler yellow fever has found—one who thinks nothing too trivial for him to record, nor too insignificant for him to notice, that can be made to reflect any light on the absorbing, difficult and interesting feature in its history, that I cannot forego the occasion of reproducing it. This patient, pains-taking and indefatigable chronicler writes: "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. 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." He then gives

the history and symptoms of the first case, and, true to his vocation of chronicler, adds: "A day or two afterwards one of the city newspapers 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. Luzenberg and 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 anything resembling yellow fever." But aside from this historical evidence, very well calculated to inspire me with distrust in information coming from that source; aside from the fact that I had seen the corpse of Dominique Maillot, and could trace nothing in its external revelations, that looked to my eye, like an unmistakable example of death from yellow fever, Dr. Chaillé, the associate proprietor in the Circus Street Hospital, with the writer of the *Memorial*, who saw the patient during his illness, and accompanied me to the dead house to inspect his body, in conversing upon the case and its relations to true yellow fever, observed to me at that time, to others and to myself as recently as the 25th February, 1858, "That he did not consider it a case of genuine yellow fever, but that if it were to occur during an epidemic, it would unhesitatingly be called a case." I thought the observation very just as we looked on the corpse; I thought so when the Report of the Board of Health was written, and I still think so. Entertaining these opinions, would it not have been the most flimsy toadyism to the writer of the *Memorial*, had I surrendered them from an amiable weakness to acquiesce in his? It will be observed then, that my doubts were not single and unsustained; that they did not entirely arise from the recorded antecedents of the Memorialist, but that they derived confirmation from one who saw the sick man in his mortal illness, and whose eye could not discern its likeness to genuine yellow fever, either when living or when dead. These were then good and valid reasons for withholding assent to the authenticity of the diagnosis in Maillot's case; yet it went forth uncontradicted as one of indigenous yellow fever. Could a fastidious and priggish taste exact more at the hands of the Board of Health? Could it, with any show of reason, demand that in its reasonings the Board should do more than award it the only notice it was entitled to? Of the other two cases, the Board knew nothing, until they were met with on the sexton's returns as yellow fever. From Dr. Albers, who reported the case in July, the Board

received only a verbal statement of its history, symptoms and autopsic appearance, and that a week after his death; so also from Dr. Lewis, the note which bears his signature was received only after Geohagan was dead and buried. In placing, then, these two latter cases in the same category with Maillot, the Board saw nothing which looked indisputably like yellow fever. Cases in every particular analogous to these are met with almost every summer, which are called alternately, yellow fever, congestive remittent fever, and even pernicious fever, according to the doctrinal notions of the observer. I have repeatedly met, during my residence here, with subjects presenting just such an assemblage of symptoms, with just such terminations, in what I am accustomed to call the malignant forms of congestive remittent fever. And what is equally true, analogous phenomena are occasionally seen over the whole of our Southern States, or at least, when paludal fevers are common, and exhibit a high grade of febrile excitement.

Is suppression of urine a characteristic and invariable symptom? Yet it was not present in Dr. Albers' case. Is the vomiting up of black vomit a characteristic symptom? Yet I have met with it in cholera, in parturition, in delirium tremens, in dry belly-ache, or what has been popularly called for a few years back, lead colic, and in measles. What, then, is the significance of all this? Simply, that there is no certain, invariable and indisputable standard to which reference can be made which will be universally applicable to every case. Beside the entire grouping of all the symptoms, the *tout ensemble* of the case, the only one known to me, is the fact that genuine yellow fever in this locality, and under the usual circumstances of our summer and autumnal weather, is never sporadic, resultless and without other cases following in quick succession. These may be few or many, according to circumstances not entirely known, if known at all. That a case can occur and die, without similar consequences of a limited or general character, is what I have never witnessed during my residence here; and what did not take place last summer when true yellow fever began to prevail. How different the results in September from those in June, July and August! Let us contrast them to show how unlike were the phenomena. On the 26th June Maillot dies, and there is a pause of nineteen days before another death is reported, viz., Dr. Albers' case on the 16th July. From this date to the 26th of August, there is another pause of forty days, when Dr. Lewis' case is reported. During this long interval of sixty days from the death of Maillot

to that of Geohagan, there is but one other case reported. So far, then as these are concerned, and as they had any appreciable influence over the health of the city, they are as if they had not happened. But how altered becomes the record when passing another interval of twenty-four days, viz., from Geohagan's death to that of Christian Miller's, on the 20th September, when, without controversy or dispute, the genuine type of yellow fever is everywhere recognized. On the week ending September 20th, there is reported one death; on that ending the 27th, there are returned seven deaths; that ending on the 4th October, there are thirteen deaths; that ending on the 11th October, there are twelve deaths; that ending on the 18th, there are thirty-seven deaths, etc. The difference is at once well marked, striking and extraordinary. When we have incontestible and genuine yellow fever, it never ceases with a solitary case; but owing to its inherent properties of reduplication under propitious circumstances, it grows and diffuses itself by virtue of some inscrutable law, shared in common by it with typhus fever, pestilential dysenteries, some forms of erysipelas, and even by smallpox and scarlatina, when they prevail as epidemics. Can this be said with the same show of reason, or the same abundance of proof, of any of the other types of fever met with in this region? Of that form of remittent fever which is the most frequently confounded with it? Certainly no parallel can be drawn, which will meet all the exigences of the yellow fever type. Closely resembling each other as these two types do,

"Forever separate, yet forever near,"

it is questionable, at least I know no example, when in the endemic prevalence of remittent fever, you can trace out in such quick succession, its rise, progress and decline, as usually marks the course of yellow fever. And in this, quite as much as in the grouping into one whole of its several symptoms, am I inclined to look for the true pathognomonic characteristics of yellow fever. At least such has been the uniform and almost invariable results as derived from experience of its origin and spread in divers places and under divers circumstances since 1853. There the facts are, and they speak for themselves, despite the mystifications in which they have been sought to be involved.

I have but one more observation to make before I pass to the second accusation. The Memorialist berates the Board for venturing to cast a doubt on his case, and indulges in a vast deal of vir-

tuous indignation at this disrespectful license; yet, almost in the very breath which expresses his displeasure, he assumes the same liberty, and "gently as a cooing dove" reprimands the resident surgeon of the Charity Hospital for signing a certificate, typhoid fever, which he would have signed yellow fever. Hear his language: "However high may be the opinion we entertain of the young physician who signed the ticket, we cannot see here a case of typhoid fever." Verily! consistency thou art a jewel! Is it then an atrocious offence for the Board to differ from the writer of the *Memorial*, but a venial one for the Memorialist, whenever to suit his views, he chooses to dissent from that of one of the surgeons of the Charity Hospital? How stands the case? Here is a gentleman chosen for his qualifications to fill the responsible position of a medical officer to the first and noblest Institution of the State, where, as a general rule, yellow fever is earliest seen, who attends on one of its subjects to the period of her death, examines the body, and from the concurrent testimony of the symptoms during life and the morbid anatomical changes after death, has good cause for believing the case not to be one of yellow fever, and signs it according to his convictions. But in the ardent zeal of the Memorialist to produce evidence that there were other cases of yellow fever beside his own, without having seen either the patient or the dead body, and looking through the jaundiced eye of one whose mental idiosyncrasy sees in every patient with a yellow skin, in the dog-days, pathological affinities to yellow fever, a doubt is indulged and expressed of the correctness of the gentleman's opinion, and the case is dogmatically pronounced one of yellow fever. With what show of reason under these circumstances, can the Memorialist claim indulgence, and evince all that wounded sensibility because the Board extended to him the rule, which he has so inconsiderately applied to another?

But I pass to the consideration of the second accusation, to-wit: that the officers of the Board had been invited to see the case of Maillot, but that they refrained from so doing. It is true, that this accusation is not made in these set terms, but worse than this, it is artfully insinuated. Too cautious to commit himself to a categorical affirmation, it is asked with much parade of indignant feeling, at page 245, of your last number, "Is it myself," etc.? I will relieve the writer of his disturbed fancies by submitting the following affidavits:

AFFIDAVIT OF DR. CHAILLÉ.

I hereby certify that Dr. Axson did call at the Circust Street Hospital, the day on which the *post mortem* examination was held on the body of Dominique Maillot, (a circumstance which had escaped my memory, until recalled to it by Dr. A.,); that Dr. Mercier was absent, and Dr. A. was informed that the *post mortem* would not take place until after his arrival, which was usually at 12 o'clock, M.; that I gave orders to the nurse to save a portion of the black vomit, if any were found in the stomach, who, on the subsequent day, failing to furnish the black vomit, stated that there was some black fluid or matter in the stomach, but either not sufficient or not in such a condition as to be enclosed in a bottle.

STANFORD CHAILLÉ, M. D.

Sworn to and subscribed before me, February 25th, 1858.

C. M. BRADFORD, J. P.

AFFIDAVIT OF DR. BALDWIN, SECRETARY BOARD OF HEALTH.

I hereby certify that a letter was left at the office of the Board of Health, directed to Dr. Axson, which I opened and found to be from Dr. Mercier, to this effect: "I have a patient at the Circus Street Hospital, who, if he has not yellow fever, has something much like it. Please come this afternoon and see the case." I transmitted the note to Dr. Axson. The next morning Dr. A. came to the office about 10 o'clock, A. M., and informed me that he had been to see Mercier's case of yellow fever; that the patient was dead, and that he had been informed at the Hospital that Dr. Mercier would make a *post mortem* at 12 o'clock, M.; that he (Dr. A.) could not be present, and requested me to go and witness it. I went to the Circus Street Hospital, where I arrived five minutes before 12 o'clock, M., and I remained until half past one o'clock, P. M. Dr. Mercier did not come, nor was the *post mortem* made, while I remained. The apothecary and his student kept repeating that Dr. Mercier's hour for visiting the Hospital was 12 o'clock, M., and that something must have detained him. After waiting until, as stated before, viz., 1½ o'clock, P. M., I left, thinking Dr. Mercier would not come. Further, some weeks after this, I met Dr. Mercier in the postoffice, and conversation occurring relating to this case, Dr. Mercier stated that he *now* had a case of yellow fever, about which there could be no doubt, thus implying doubt to himself of the case mentioned above.

H. D. BALDWIN, M. D.

Sworn to and subscribed before me, February 25th, 1858.

C. M. BRADFORD, J. P.

From these statements, it will be seen that the writer of the Memorial might have been spared much of his querulous humor had he have been as zealous to know what the Board did, as what they were supposed to have left undone. I dismiss this topic with the single remark, that whatever were the reasons which prevented the Memorialist from visiting the Hospital at the customary hour, it would have certainly been more courteous on his part to have explained, than to have indulged the disingenuous insinuation that the officers of the Board, and myself in particular, might have seen the case, living or dead, if I had so chosen to do.

The third accusation is by far the most important, and it acquires this distinction altogether from the ill-concealed venom it seeks to hide. It is sought to be impressed on the reader in the form of a *postscript* to the *Memorial*, that the testimony upon which the reasoning of the Board turned as to the mode of the origin of the fever, is not only untrue in itself, but untruly reported, and he submits notes *without comment*, from certain parties to prove this.

The discomfiture of the Board was to be so complete by this publication, that the Memorialist, forsooth, could generously refrain from comment; could magnanimously spare a prostrate foe! One can almost hear the complacent cachinations of the writer, as he penned these portentous words, which were, like Jove's thunderbolts, to blast the Board for all time to come. Lucky thought this! just in the nick of time, and it suits the purpose, and does the job so well, we can imagine him to say, that I can afford to be silent;

> "Can assume the God,
> Affect to nod,"

and shake not the spheres, but that villainous abomination, the Board of Health. Really, one might imagine on perusing the very solemn and measured phraseology of the postscript, that the Board had committed some awful crime, which merited indictment by the Grand Jury, too fearful to be mentioned! too serious for comment! And what does it all amount to? Why, simply, that the Board reported the testimony of Mrs. Rose as she gave it, and that Mrs. Rose, after being informed what she did say, declares that she speaks English so badly, she must have been misunderstood. I submit, then, Capt. Ivy's affidavit to prove that what she was understood to say by myself, she was understood also to say by Capt. Ivy:

AFFIDAVIT OF CAPT. IVY.

This is to certify that I was present with the President of the

Board of Health, when the testimony of Mrs. Rose was given as reported in the Appendix to the Report; that the published statement is correct in fact and details, as narrated by her.

THOS. J. IVY.

Sworn to and subscribed before me, February 25th, 1858.

C. M. BRADFORD, J. P.

Thus far then, the correctness of the report of the testimony stands unimpeached and unimpeachable. Its truth and value as testimony is quite another matter altogether. Nowhere does the Report affirm its truth or endorse it. It speaks of it in guarded and circumspect language, as very disputable evidence, and it expresses regret that nothing more reliable, nothing more direct and to the point, and nothing more pertinent could be had. Moreover, the only use made of it was simply to show, on the hypothesis of its truth, that it might be made to explain the origin of the fever, at the moment of time when every doubt was dispelled as to its real character. It went to show that the history of yellow fever as transmitted to us by eminent observers in the early part of this nineteenth century, furnished points of analogy and comparison, similar in principle and fact, to what the Board sought to establish on the hypothesis of the truth of Mrs. Rose's testimony. And yet so simple and plain a proposition is twisted and contorted, like a ligature in the writer's hand, to suppress the truth and to staunch conjectures by which a reasonable solution of the undetected source of the fever might be discovered.

But I have already wasted more time on an issue which was entirely unnecessary when respect is had to the obvious meaning of language. This is so plain, direct and unambiguous, that one would be tempted to wonder how it could be made to mean anything else, were not the perversions of the Report to transparent and flimsy to be misunderstood. The unskilful hand betrays too plainly the meditated purpose,

"To hint a fault and hesitate dislike,"

and shows the real aims of the Memorialist. It is sufficient that they are understood. I therefore proceed to examine into the last accusation brought by this bill of complaints against the Board of Health, which is, that the Board affirms the infectiousness of yellow fever, and consequently denies its domestic origin. Wherein lies the incompatibility between the infectiousness of yellow fever and its domestic origin, it is difficult to discover. Because a disease originates

here, is it therefore necessarily non-infectious; or conversely because it is imported, is it more so in the nature of things? But unfortunately for the assertion of the Memorialist, the Board nowhere denies that yellow fever is indigenous, and it challenges the proof of a single line or paragraph in the Reports of 1856 or 1857, which will admit of such an interpretation. The whole drift of the Reports of each year aims to show that the phenomenon of the spread of yellow fever is as rationally explained on the hypotheses of its infectiousness, as on any other of the many hypotheses which have been advanced, or on all of them put together. It hinted nothing more, nor attempted anything further. If, then, the writer of the *Memorial* chose to rival the renowned Knight of La Mancha, and fight phantoms of his own creation, he may do so till "the crack of doom," if it affords him any intellectual pastime. But unless he desires his exploits to point a moral or adorn a tale, he must be heedful how he attempts to place behind his wind-mills the Board of Health, and to represent it as sustaining such unreal and fancied positions.

If the object of the Memorialist be to render odious in the public judgment the policy of the State in the establishment of quarantine, he has sadly erred in making his argument to hinge so entirely on personalities and innuendoes. The people of the State and their representatives in the Legislature, cannot fail to see the utter impotency of a cause that limps along on the feeble crutches of insinuation and misrepresentation. Quarantine may fail to prevent the introduction of yellow fever, but its failure will nowise affect the question of the infectiouness of the disease as long as the vast array of facts, which have accumulated since 1853, leave their impress on the public mind. It is confessedly a difficult task to close against its introduction all the avenues by which the fever may be introduced, but because the difficulty is great and acknowledged, is it a sufficient reason why efforts should not be made to accomplish it?

But, Messrs. Editors, I have already occupied more of your valuable space than this whole matter merits. If the Memorialist had confined himself strictly to argument, I should not have bothered myself with a reply. He is entitled to his opinions, and the reasons upon which those opinions rest. They are his own, and he has the unquestioned right to entertain them, be they what they may. But when, to give point to those opinions, he seeks to place the Board of Health in the attitude of a foil to them; when, to give interest to his querulousness, he represents it as doing and maintaining

what it did not, and as leaving undone what it should do, the whole bearing of the question is changed, and an appeal is made to prejudice and passions, which can neither subserve the cause of truth, nor fail to defeat it by mixing up with it matters altogether foreign and repugnant to it.

Having thus shown the utter groundlessness of the charges against the Board, I take my leave of this subject with the single observation, that I have no complaint to make, if there be some in and out of the profession who may differ from the views put forth in the Report, and may contest them. Discussion, perhaps, even in so trite a subject, may elicit something more new or more true, but I submit that it must be a fair and frank discussion, exempt from mischievous innuendoes and flagrant misconceptions.

A. F. AXSON,
President Board of Health.

[NOTE.—The Editors of this Journal having admitted into its columns an extended article animadverting upon the proceedings of the Board of Health, and also, the above reply to the same, will be compelled to exclude any further controversy, should any be attempted upon this subject. The weight of character and great ability of the gentlemen engaged in this controversy, would give more or less interest to their writings, even on medical polemics, yet a sense of justice towards the many and increasing patrons of this Journal, calls for renewed efforts on the part of its conductors to make it as far as they can, an useful scientific one, and withal *impersonal*, *peaceful*, and *fraternal*.

The history of Journalism teaches that when certain disputable questions concerning local medical police and polity, contagion and quarantine, become blended with personal interests and recriminations, they are apt to give rise to animosities which the all-atoning grave alone can silence, being not only fruitless and inconclusive for science, but unacceptable to a great majority of the patrons of a Journal, and incompatible with the standards by which the Editors of the *New Orleans Medical and Surgical Journal* wish it be judged, namely: TO DO NO HARM: TO DO ALL POSSIBLE GOOD: TO BE INDEPENDENT: TO BE CATHOLIC: TO BE PERMANENT—OF

"Dying, leave no line they wish to blot."

B. D.]

ART. IX.—*An Essay on the Cause, Treatment and Cure of Asiatic Epidemic Cholera:* By T. B. PINCKARD, M. D.

[B. DOWLER, M. D., *Managing Editor of the N. O. Med. and Surgical Journal:* Dear Sir,—Having only a few weeks since read in the N. O. Med. and Surg. Journal, published in November, 1857, your article on "*Researches into the Natural History of Cholera,*" in which you give, "the remedial measures in cholera," and say that, "*water,* perhaps, should be reckoned as an important, if not a principal remedy in the treatment of cholera, although it is often interdicted by the medical attendant, under the apprehension that it increases the tendency to vomit." You farther say, "*theoretically* considered, it is the remedy best adapted to supply the loss of the watery parts of the blood; for water, which is, perhaps, more readily absorbed than anything else, is adapted to supply the great loss of the watery parts of the blood; the patient has an irrepressible desire for it, and this fact alone affords a presumption favorable to its employment."

The reading of your article, which contains in substance what I practised and wrote in 1833, has induced me to send a copy of the manuscript which I wrote in September, 1833, to you for publication, if you consider it of any value. The old manuscript was prepared at the request of the late Professor Daniel Drake, of Cincinnati, who had heard of my treatment of cholera and its success, in the epidemic of 1833, in Lexington, Kentucky.

Dr. Drake, who was editor of a Cincinnati Medical Journal in 1833, published a few extracts from the manuscript I sent him, and returned it, merely mentioning cold water as one of the remedies used, but did not attach that importance to its use in the stage of collapse which I did; nor did he give my opinion of the *pathology* of the disease which clearly showed that cold water or some fluid was absolutely necessary to cure cholera when in the state of collapse, and that no other remedy will ever be discovered or needed if water is given freely, or some other fluid suited to the appetite or desires of the patient.

Dr. Drake was entirely excusable in attaching but little importance to my mode of treatment; for, at that time, *calomel* was considered a *specific* if given in doses sufficiently large, and persevered in, by nearly the whole medical profession.

Professor Cooke's cases of cholera came daily under my notice,

and it is true that some recovered after taking many ounces of calomel which passed through the bowels often in a dry mass, surrounded by a thick mucous covering ; he having prescribed it to be taken from a tea or table spoon in its natural dry state. These recoveries could be generally traced to the free use of cold water after they had become collapsed, for Dr. C. was less rigid with his patients than the other practitioners, as he considered if they swallowed enough calomel they would recover, even if they transgressed in minor indulgences ; his collapsed patients did indulge freely in drinking cold water, for there was an instinctive craving for it, and so soon as the irrepressible thirst was satisfied some of the patients recovered rapidly. Dr. C. probably knew nothing about his patients allaying their thirst, and if he did, he attached no importance to it as a curative agent, but would have attributed the severe salivation which generally appeared after so much calomel as caused by its free uses, and that his large doses cured them.

Professor J. R. Mitchell, then of the Transylvania Medical School, and now of the Jefferson Medical School, Philadelphia, knew of the success I had in prescribing cold and iced water to collapsed patients. Some years since, in looking over the article *water*, in his work on Materia Medica and Therapeutics, I observed that he slightly noticed the success attending the free drinking of cold water, as prescribed by *a physician* in Lexington, in cases of cholera.

Four years since, while living at Pass Christian, in the winter, and having but little professional business, I read in a newspaper that the legacy left by Mons. Breant of France, giving one hundred thousand francs to the person who should discover the *cause and cure* of cholera, and that the Academy of Medicine, in Paris, should decide who was entitled to the prize, and that thousands of essays had been presented to the Academy but no one had been successful, I concluded that I should lose nothing by copying my manuscript of 1833, and to make an addition to it by my experience in malignant and epidemic cholera, in Versailles, Ky., in 1835, and also in the epidemic in New Orleans, in 1848, and, to add to its importance, to give in detail a short history of some few of the cases in Lexington which were considered hopeless by the physicians who had left them to die. The old manuscript of 1833 was copied, and the additions made, and the manuscript was enveloped and directed to the *Académie de Médecine*, Paris, the postage paid to the French Con-

sul at New Orleans, and his seal affixed. It is now four years since it was mailed, and no doubt my thirty odd pages of foolscap have long since been sold at the rate of "two cents a pound to some grocer to fold up coffee and sugar," which I learn from your Journal of March, is occasionally the way of disposing of communications, etc.; we have this statement in a letter to you, from one of the most illustrious physiologists living.

Accompanying this letter will be the manuscript on epidemic cholera.

I remain, with great respect, yours, etc.,

T. B. Pinckard.]

An Essay on the Cause, Treatment and Cure of Asiatic Epidemic Cholera.—In 1832, the cholera made its first appearance in the United States, and in June, 1833, epidemic cholera suddenly attacked in a most malignant and fatal form, some of the most wealthy and temperate persons in Lexington, Kentucky, an inland town, remarkable for its healthy location and great cleanliness. The first cases proved fatal in less than twenty-four hours. This produced a general panic among the whole population, numbering something less than ten thousand. A great number fled from the town in great terror, in a short time after the terrible disease appeared, many of whom were attacked, and some died after leaving.

We had removed to Lexington, and opened an office in May, one month before the cholera *broke out*, and saw none of the first cases. We had, from the first appearance of cholera in Europe, and especially after its existence in the United States, read everything that we could obtain on that *mysterious* disease, and had decided on the remedies we should use, if we had to treat it, until experience taught us differently. The first cases that came under our care were considered hopeless, and had been so pronounced by the physicians who had left them to attend to those that had recently been attacked and who might be relieved.

The relations and friends of those forlorn patients, very naturally called us, who was unemployed, to see them, hoping something might be done to alleviate their sufferings, produced from cramps. With hopeful feelings of being useful, we commenced our labors by rubbing unremittingly with our hands the parts affected, using a stimulating liniment of equal parts of tinct. of camphor, spirits tur-

pentine, hartshorn (aqua ammonia) and olive oil or hog's lard; the latter may be increased in quantity; we put the feet and legs in a hot bath with salt and mustard ; we permitted them to take anything in a fluid state they desired, and with a cheerful countenance and encouraging words, inspired them with hopes of being cured; this encouragement, with our own personal labors, appeared to inspire them with new hopes of life, and indulging their excessive thirst with cold water, made life more desirable and calmed their minds. We gave no medicines in those cases, as they had, in every instance, been freely dosed with calomel, opium or its preparations, and in some cases with astringents. Those cases that had been narcotised, generally by the attendants exceeding the dose prescribed of the narcotic, frequently terminated in death ; some of the others recovered rapidly, which astonished us and all who had witnessed their apparently hopeless condition less than twenty-four hours previously. We were not aware then of the wonderful effects of fluids, either in the form of cold water, brandy and water, or very weak salt and water ; for we used the latter to satisfy friends that were afraid of the cold water causing the calomel to salivate ; the salt was disguised by being rubbed with gum arabic, and colored so as to be a mystery to those who mixed it in the water ; (for nothing delights mankind so much as mystery.)

We are candid to confess that we did not give our first patients water as a remedy, but merely from feelings of humanity and a firm belief that the natural instincts of nature seldom err, but are beneficial if indulged in a judicious manner. While a student of medicine, we had determined never to deny our patients cold water, except directly after an emetic, if they earnestly desired it. We had seen fever patients rendered frantic from withholding cold water by the wise M. Ds. that attended them, and had known recoveries take place most unexpectedly when cold water had been secretly obtained ; the why and wherefore we did not comprehend. At all risks we had invariably permitted our patients for the seven years we had practised to drink cold water prudently, and had every reason to know it did much good, and no harm in a single case.

In a few days after treating those cases which were *collapsed* and considered hopeless, we were called to see many cases in the first or *premonitory stage;* and in the second stage, or what we called *established cholera.*

We have from that time till the present, a period of nearly twenty-five years, observed *three well defined stages* of cholera, unless in a few

cases where *fear* and the *cause* producing cholera were so overpowering to the nervous system, as to destroy life almost as soon as a "stroke of lightning," or some virulent poison, the patient dying in a few hours, it being impossible to produce reäction by stimulants internal and external, or even by the mustard and salt emetic.

We have also noticed and are satisfied from what we have read and our own experience in three epidemics, viz., in Lexington, Kentucky in 1833; in Versailles, Kentucky, in 1835, and in New Orleans, 1848-9, that cholera from its first appearance in Europe and America, and the same might be said of Asia, is identically the same disease, passing through the three different stages, if not arrested, except the anomalous cases mentioned; and the same causes produce it; and furthermore that the same means and medicines prevent or cure it as certainly now as formerly. Some epidemics are milder and less fatal than others, and different individuals are attacked with more or less violence, but it is the same disease, requiring precisely the same remedies only modified to suit the case.

The three stages of Asiatic Epidemic Cholera: First stage, Premonitory — Is known generally by slight purging, (diarrhœa) with but little uneasiness, and frequently none at all. This purging soon empties the fæcal contents of the bowels, and if this mild stage is neglected or not properly treated, it runs rapidly into the *second stage, True Cholera,* and still more rapidly into the third stage—*Collapse—the death stage.*

Second stage.— True Cholera, appears at different intervals of time, depending on the violence of the attack, after the existence of the first stage, or premonitory symptoms; it is known from that stage by the most casual observer from the watery discharges from the bowels, resembling rice-water, and very appropriately called "RICE-WATER DISCHARGES;" also, from the rapid sinking of the pulse, the shrinking of the muscles, the peculiar expression of the countenance, general exhaustion of body, with watery exhalations from the skin, and above all, by the sick stomach, vomiting and the most severe, painful and excruciating cramps, producing spasms of the muscles so as distort the body and face in a frightful manner, producing horror and dismay on the feelings of those who witness this sad but true picture of "*real cholera,*" just previous to the existence of the *third, and last stage—Collapse.* This stage, if the two former are not arrested, appear sooner or later, according to the violence of the attack, or the treatment; it is known from the second by the greater

wasting and shrinking of the body and strength, and sharpness and death-like appearance of the face, the entire failure of the pulse, and consequent coldness of the extremities, the excessive thirst, and craving for cold water, which appears to supercede all other sufferings, and to make it most painful to witness, except by scientific, stoical and philosophical M. D's., who positively forbid water, and especially cold water, for fear of vomiting and throwing up of the calomel, opium, tannin, sugar-of-lead, phosphorus, etc., etc., which they have prescribed to be taken at regular intervals, some of which are valuable in proper doses in the first and second stages, but never to be used in the third. Cholera patients have been tantalized of late years with the liberty of cooling the mouth and tongue with small pieces of ice.

Having given the symptoms and distinguishing characteristics of the three stages of epidemic cholera, which we have invariably observed to be most prominent in three epidemics, we proceed to give the treatment we pursued with but little variation in all cases, from 1833 till '48—a period of more than fifteen years.

Mental treatment of the First, or Premonitory stage.— When called professionally to visit those in the first stage, we invariably made it a rule to appear quiet and composed, and avoided all conversation respecting recent deaths; but spoke of recoveries even in the worst forms of cholera. (All this was accomplished in a few minutes.) We then inquired of the patient in what way he was complaining, and having fully ascertained the state of his or her bowels, and if possible, examining the discharges, we spoke lightly of the attack, but in a decided manner explained to him or her the necessity of remaining in the bedchamber quietly, and by all means in a recumbent position, having previously a hot foot-bath, and if not inclined to sleep, to have one or two of the inmates of the house in the room to converse cheerfully, having been forbidden to speak of cholera. We were careful to call the threatened attack *diarrhœa*—as there is much in a name. When cheerful company could not be obtained, we recommended some interesting book to be read. All company was positively forbidden unless some prudent friend was desired; the door bell was to be muffled, if in hearing of the bedchamber. A quiet mind and body often prevents and checks cholera.

Medicinal Treatment of the First stage.— As has been stated, we had decided on the medicines we should use before we had seen any case of cholera. This decision we had made in consequence of all

we had read, as we found no uniformity of treatment, though the largest number in the West and South considered calomel in large, even in enormous doses, the specific remedy in all stages of the disease. We had no faith in those large doses as being curative, and, therefore, decided on a prescription which we thought from the medicines composing it, better adapted from the therapeutic effects of the different ingredients composing it, to meet the indications, of arresting and curing cholera, than any other; we made the addition of one article to the compound prescription, after a few days' experience, viz., quinine. *Prescription for the first and second stages of cholera :*

R. Calomel, ʒ ss.;
Pulv. Gum Camphor, (very fine) xx grs.;
Cayenne Pepper Opt., (best) xx grs.;
Opium Opt., (best) pulv., x grs.;
Ginger, Jamaica best, x grs.;
Quinine, x grs.;

rubbed together most intimately in a mortar, and divided into ten equal powders. When called to a patient in the first stage, one of these nerve-composing, tonic, aromatic and liver-secreting powders were given in a syrup of loaf-sugar and brandy, washed down with brandy and water, or some fluid prepared, the patient reclining on a sofa, lounge or bed; if there was the least nausea, a moderately strong mustard poultice was applied over the stomach and bowels. In this stage we gave but little drink, and that little brandy and water, ginger tea, toast water, gum arabic, or slippery elm and wild cherry bark, or peach leaves steeped in cold water, and if any evidence of acidity existed, weak lime water. But if a full meal had been eaten and not digested, we promptly gave a salt, mustard and warm water emetic and completely emptied the stomach, at the same time causing perfect reäction, not only in the vascular but nervous system. So soon as the stomach was composed, a powder was given.

It is to Prof. B. W. Dudley, the skilful and successful surgeon of the Valley of the Mississippi, that we are indebted for a knowledge of the great value of this *special emetic,* in numerous cases of cholera in the first and second stages, and also for his mode of administering it, which is, to give it rapidly to the patient, at the same time filling the stomach with tepid salt and water, so as to produce emesis in a few minutes. We found it in many cases a most invaluable remedy, when given thus promptly.

We also acknowledge our indebtedness for suggesting the addition of quinine to our cholera powder, by a talented and practical young physician, Dr. Joseph Challen, who fell a victim in the second week of that terrible epidemic in Lexington.

Our directions for using the powders, after giving the first, was to give one after every operation from the bowels; by this method giving entirely for effect, and not according to intervals of time. This we have always considered the only correct mode of giving medicines in certain diseases. Sometimes we increased the dose, and always in delicate females lessened it. We gave it to children in portions of one-eighth to one-fourth of a powder, from two to twelve years of age. Quiet sleep was most important, and one of the most favorable indications of the disease being arrested. When sleep could not be readily obtained, friction along the spine and other parts of the body, in young persons, was most useful.

This constituted our entire treatment during the premonitory stage; the object was to quiet the nerves, equalize the circulation, to check and prevent the frequent action on the bowels, to arrest nausea, and in proper time to estalish healthy secretions from the liver; this, when effected, was *health*, and required no further professional interference, only admonishing to great caution in diet, and avoiding much exercise, especially in the sun.

When, from not feeling that any real disease existed, as the discharges from the bowels were unattended with pain, or from using improper nostrums that were said to be infallible in preventing and curing cholera, and sometimes from injurious professional prescriptions, the first stage progressed to "rice water" discharges, (for that was the first unmistakable evidence of the second stage,) the disease then, to those acquainted with epidemic cholera, had reached a point calculated to alarm the most experienced physician, and if the patient became alarmed, he was in the most imminent danger.

Treatment of the Second stage of Cholera. — If we had been called early in the first stage, (which was rarely the case) and had treated it as has been directed, without giving relief, or if the second stage existed when we saw the patient, we pursued the same course of treatment as in the first, except in giving a double dose of the powders at the commencement, then returning to a single powder, and using injections of a strong solution of sugar-of-lead, laudanum and starch; tannin, or a decoction of nut-galls or oak bark. We also, in a few cases, bled generally or locally, in addition, when vomiting, purging and other alarming symptoms continued after a fre-

quent repetition of the powders; we were induced to give calomel in doses of 20 grs., and to repeat till three doses were taken. It was in this stage, and in cases of vomiting and purging, increasing the sufferings of the despairing patient, that those spasmodic cramps existed, causing affright and horror to those who witnessed them; it was now that the services of those present were constantly needed, in using friction over the parts cramped, saturating their hands with the liniment, but avoiding blistering the skin, first washing off the copious watery fluid (for it was not perspiration,) from the whole body with a strong solution of alcohol and alum. The friction we directed to be used downwards in every case, not up and down, or irregularly. It was in these distressing cases, where cramps, vomiting, etc., existed, that we brought to our aid that powerful remedy — that *lever* to produce reäction in the human system—that power in mechanics with which Archimedes proclaimed he could move the globe, if he had a fulcrum to support it, namely, the *mustard and salt and water emetic*, which certainly in some of the worst cases, when given promptly and boldly, achieved the most wonderful results, if it did not pass from the stomach through the bowels; promptness in giving it was the only preventive of its having this effect. The mustard emetic, when it acted properly, not only produced reäction, but set up a new and more healthy action in the stomach; it promoted healthy action of the whole absorbent system, (this is well known in the treatment of dropsy, etc.,) and we know that the absorbents and exhalants are antagonizing systems; if the absorbents act, the exhalants are passive to a great degree, and epidemic cholera may properly be called a general disease of the exhalants, for if the exhalants are restored to healthy action, the absorbents act, and all the secretions will be healthy, and this is health.

We invariably noticed after the proper action of a mustard emetic that the skin and extremities became warm and dry, the stomach quieted, and those horrid cramps were not felt. It was in the latter part of the second stage, after a large portion of the serum (water) of the blood had been drained off through the exhalants of the bowels, skin, etc., that the thirst for cold drinks appeared so distressing. As the stage of collapse approached, the thirst was, if possible, more distressing and painful than the cramps. Many would have given all they possessed for free indulgence in cold water; but still it was forbidden by the whole mass of *scientific physicians;* it was the worst form of cruelty—as bad as the tortures of the inquisition! Yet it was the only remedy now calculated to prevent collapse and death.

Treatment of the Third Stage — Collapse in Cholera. — As we have stated in our treatment of the first cases of collapsed cholera that we saw in Lexington, we gave no medicines in this stage. We encouraged our patients, and attended most perseveringly to all his sufferings and desires, doing everything to alleviate them. So soon as the thirst had appeared in the second stage, we had indulged it by giving cold water frequently, but not large draughts each time, for fear of increasing the vomiting and throwing up medicines we were then giving; but we gave sufficient to prevent our patient's suffering from thirst, and sufficient to keep the absorbents acting and employed in carrying it into the blood to supply the loss of serum from the exhalants. But in the third stage, we relied on water or fluids the most agreeable to our patient exclusively. We gave water *ad libitum*; for their cry was, water—cold water! We did not limit them; they drank it from a tumbler, cup, pitcher, calabash, or even bucket, as some of the slaves preferred. If they threw off a part by vomiting, it only made the absorbents more active to carry the balance into the circulation, (for it is a medical axiom that *vomiting promotes absorption.*)

It was in this last and hopeless stage, (it was hopeless without the free use of the fluids desired by the patients,) that we were annoyed in many cases, by relations and friends, before there was time for the patients to be relieved by the water, to give more calomel, or some nostrum, which was infallible; for some were not satisfied with the *mysterious* powder we added to the water, but required regular doses to be given; from their urgency, we had sometimes to resort to a *ruse*, and gave white or red chalk, sweet spirits of nitre and comp. tincture of cardamon mixed, a tea-spoonful for a dose, etc., at regular intervals.

We avoided giving all powerful medicines in this stage, as we found, as has been stated, that there was less hope of recovery by the use of cold water, in those cases that had been over-dosed in the first and second stages. Calomel appeared to form an exception, for we had heard of a number of cases that had taken ounces of calomel before they became collapsed, and then recovered by using cold water. This fact of recovery after using large doses of calomel, though they had no effect to check the disease and prevent collapse, and the patients were left to die by the advocates of the infallibility of large doses of calomel, yet when they heard of these recoveries and claimed the credit of curing them. As for water—plain well or cistern water, ice water, the physicians were too

learned and wise, or too prejudiced to believe in any thing which to them seemed so absurd, so simple, so unphilosophical, so unscientific! Did the learned astronomers of the colleges of Italy believe in the doctrines of Galileo? or the professors of Salamanca in the demonstrations of Christopher Columbus? Did such believe in vaccination, by Jenner, or in the circulation as announced by Hervey? No! Those called learned and wise often will not compromise their dignity by believing the simple truth, though God in his perfect wisdom effects the greatest objects by the most simple means.

That fluids are absolutely necessary to cure cholera after collapse exists, there can not be a single doubt by any unprejudiced person who has a knowledge of the anatomy and the physiology of the circulation, the different offices of the absorbents and exhalants, and the unerring instincts of nature in requiring that which is imperatively necessary for the healthy and natural wants of the different parts of the body in performing its healthy functions.

We know of no disease which exemplifies so clearly and beautifully the necessity of understanding anatomy, physiology and pathology as epidemic cholera, and the use of water for the cure of collapse; no problem in mathematics, natural philosophy, or chemistry is plainer.

It is true, as has been stated, that it was by experience we discovered its curative powers, and we should, after all the experience we had of its restorative effects, no doubt have neglected its regular use, but from the irrepressible desire of our patients to allay their thirst, and our gratifying them from feelings of humanity. It is from ignorance of the true pathology of diseases that the most valuable medicines, specifics, which have for ages stood the test of experience, have been discarded from general use, and some newfangled article without any real value, substituted by some flimsy, plausible writer, whose main object is to obtain notoriety. A knowledge of pathology prevents these *unscientific* impositions.

It was after treating many cases of collapse in cholera with cold or ice water, that we had a very interesting consultation case with two physicians, who ruled the treatment, but left the patient to die after collapse had existed for some hours. The physicians having objected to the use of cold water entirely to this time, after they left we directed the husband to send for ice and to give her as much ice water as she desired, for which she had been entreating her physician through the whole night and morning to give her, (one of the

physicians having remained with her during the night). After prescribing ice water in this case, we went many miles in the country to see cholera patients, and while riding slowly on horseback, we commenced reflecting on the almost miraculous cures we had seen daily, from the free use of cold water, and the certain death after collapse, when it was cruelly withheld. We asked ourselves in what way cold water cured? and also asked what is epidemic cholera? The natural answer to the second question that cholera is the separating and passing off from the coagulable or firm part of the blood, the serum or watery part. *Question*—How does it pass off? And in what way is it conducted into the bowels and to the surface of the body? *Answer*—Through the exhalants, which are in a relaxed and diseased state. We now knew the nature of cholera and collapse. What if the blood is deprived of its serum? Can the blood continue to circulate? *Ans.*—No. Impossible. The crassamentum or coagulable firm part of the blood which alone remains, can only circulate imperfectly in the large vessels. The circulation ceases in the extremities and surface of the body. Congestions form in the lungs, etc., and death is inevitable, unless the loss sustained is supplied by drinking water. But how can water be taken into the circulation? *Answer*—By drinking it, and the absorbents being thirsty, convey it rapidly into the circulation to mix with the solid part of the blood, and this causes in a few hours a healthy circulation. In this off-hand manner we reasoned, and at any rate demonstrated to our own satisfaction that we had discovered the reason why water taken into the system, through the stomach, skin and bowels, cured *curable* cases of collapse.

The great mathematician in his hot garret in a nude state after demonstrating the 48th proposition of Euclid in geometry, ran through the streets, proclaiming "*he had found it*," — "*I have found it*," could not have been more delighted than we were, and to render our pleasure complete, on our return to the city, the same evening, and visiting our dying patient, we found her entirely relieved, and pressing our hand, said, in a feeble voice, that the ice-water had cured her. She is still living in Lexington. After this, who could doubt its powers of curing curable cases? We did not, for we had that day discovered the *true pathology* of epidemic cholera, and why water cured it.

We will now give an account of a few cases of collapse cured in Lexington, by using cold water without any limits.

We had been giving cold water freely to all our patients for a

week without any restraint — some recovering and others dying — but had attached but little importance to it as a remedial agent, only observing that some of the worst cases recovered very unexpectedly to ourselves and others. But the cause we did not understand; when one morning in passing a hotel, the family in which we knew intimately, we were called by two of the daughters of the proprietor, in great distress, to come in and see several members of the family as a friend, as they were dying of cholera. On entering the hall, we saw two of the nieces, about nine and elveven years old, lying on a pallets, and a servant girl who had been their nurse, also on pallet, in the same hall, all placed there to have the benefit of the circulation of air, for it was excessively hot. We were informed that an eminent professor of Transylvania Medical School was in the house examining the mother of the children, their sister, and that their mother had died that morning, and that the professor had examined the children and their nurse, and said he could do nothing for them, and that they would die before night. At this time the Dr. came into the hall, and I explained to him that I had been merely called in when passing, by the ladies, as a friend. He requested me to examine the three dying patients, which I did. The skin of the extremities and face was blue, cold and clammy. They were all entirely pulseless. They were quiet and free from pain, sickness or purging. He observed that they would all die before night. He had given them small doses of calomel and, we suppose, the mustard emetic. The Dr. used, in no case, more than ten grs. of calomel at a dose, and in no instance used opiates or astringents, but forbade the use of cold water and other fluids, except in small quantities. He invited me to the bedchamber of the mother of the children, who was in the second story, suffering the violent spasmodic cramps, when I entered the room. We soon left, and the Dr. observed he had some hope of her recovery. He passed out, and we remained at the request of the ladies. One of them soon asked us if they could all drink cold water, as they had craved it above all things through the night. We replied, certainly; send to the ice-house in the yard, and give them freely of ice-water. It was soon prepared, and we enjoyed the pleasure of giving them all the first drink; but no sooner had they drank it than they both exclaimed, it will salivate ! We replied, not if they die before night; do let them enjoy cold water the few hours they have to live. But fearing it would be withheld, we called several times before our dinner, having to pass the door in visiting other patients, and gave

them as much ice-water as they would drink. In making our last call before dinner, we could observe that the circulation was returning rapidly to the extremities; that they had slept refreshingly, and had a free, warm perspiration, and bilious discharges from the bowels. Late in the evening, when we returned, we found the mother dead and the children and servant out of all danger. All thirst had ceased so soon as the circulation was restored and bilious secretion was established; in fact, all the secretions of the skin and kidneys acted properly when the circulation was restored. We next morning visited our patients. We found the two little girls sitting on their pallets playing with their dolls.

The rapid recovery of these three collapsed patients, filled our minds with wonder. It appeared almost miraculous, and we now felt fully impressed that water, simple ice-water, was the curative agent; we could not, nor did we, doubt it. From this time until some days after, when we discovered the true cause, as we have stated, of its healing properties, we were constantly trying to discover in what way it cured. We have always felt satisfied that those cases had been treated in the best manner to render cold water efficient in a prompt manner. They had taken small doses of calomel, but previously had been freely vomited with the mustard emetic, which had cleaned the stomach of all vitiated accumulations; it had aroused the healthy action of the absorbents, which now only needed a full supply of agreeable fluid to be taken by them into the circulation. The doses of calomel were of the right size to act gently on the liver.

We were daily seeing cases and recoveries which we will not report. But one occurred about this time which is too remarkable to pass unreported. On returning home one evening at twilight, (one of the most fatal days of the epidemic,) we saw one of our nearest neighbors walking the pavement, and so soon as we dismounted, he requested us to go and see his hired slave woman, who was dead, and had been so for several hours, as stated to him by two young doctors, the office pupils of Prof. Dudley, for some years; that he had been waiting for some time expecting some of the factory hands to pass by, so that she might be placed in her coffin which was in her room; that having himself just recovered from a most violent attack of cholera, (his wife having been saved by the skill of Dr. Dudley,) he had not felt able to go into her room during her illness, or since her death, for fear of a relapse; that while waiting to have her placed in her coffin, he recollected of having read

and heard of cases of persons being buried, thought to be dead, but who were alive, and that he wished me to examine her carefully.

When I entered her little close room, I observed her coffin on two chairs by the side of the bed, she lying on her back, with her eyes staring open and her head thrown far back, her limbs drawn up and her hands tightly closed, and her clothes and bedclothes in great disorder, showing evidently, as I thought, she had died in a spasm. After admitting the fresh air in the filthy room, I drew forward her head and placed the pillow under it, straightened out her limbs, and adjusted her clothes and the bedclothes, and tried to ascertain if life existed. I thought life extinct; but went to the pump on the pavement fronting the house and procured water, (there was no servant, and the family were all sick, or so alarmed that they did not appear.) I filled a spoon and inserted it between her teeth and poured the water into her mouth repeatedly, and dashed cold water into her face. I then discovered some appearance of life, and persevered with the cold well-water, till she drank freely. Her husband came in at this time, and we directed him to give her freely of cold water. Some hours later, before retiring for the night, we called to see her and found her much improved, and disposed to rest. Before leaving we sent her husband to the pump for a bucket of water, and directed him to give it to her freely, whenever she desired it through the night. Next morning, when light, we hastened to see her; she was sitting up in the bed, with her tangled hair in knots, and her face presenting a scurfy appearance, (she was a mulatto,) her coffin still remaining on the chairs near the bedside. She looked as if she had been dead and buried, and just taken out of her coffin. In three days she was doing the cooking for a family of six or seven, as there was no one able to do it. She had not been over-dosed, and the absorbents and powers of nature soon restored her to health.

We will only detail one more extraordinary case and recovery from the use of cold water in Lexington.

Our brother-in-law, L. M. Vance, now of Indianapolis, at that time a youth of 17, was driving us rapidly down Main street, on the lower part, when he observed a large negro man lying coiled up on the side of the street, in the hot sun, near where the gig had to pass; he stopped, and we both got out to examine him; we thought he was nearly dead, but concluded from humanity, we would remove him to a factory not far distant across the street, and try the powers of cold water, which both of us just at that time consid-

ered almost an infallible remedy. With some difficulty we dragged him to the large eating room of the factory. We found the old cook woman alone, as all the slave operatives had left from alarm some time before. She permitted us very kindly to deposit him in the room, placing something for bedding under him. Young Vance then procured a bucket and filled it from the pump with water; we dashed it from a tin cup on his face neck and breast, and then used a spoon so as to wet his mouth. In a short time he revived, and was able to drink freely from the cup. We left the bucket full of water and the cup by his side, and requested the old woman to let him drink freely. Three days after, we were both passing, and had a curiosity to call to see our patient. We still found the old woman alone, and on inquiring for the man found he had gone home the day before to his master in the country. We never knew who was his master, or anything more about him. On inquiry of the the old woman, we found he had drank immoderately of the water the evening and night we left him — a whole two buckets full, she said—enough to kill a horse. She thought "he would *bust* himself; I never see any human drink so much water, and he wanted it cold from the well. I tell him our factory doctor told us, all niggers, that cold water, when they had collery, would kill every nigger that drink it—I hearn Ben, Jim, Tom, and Dick die, *case maby*, they got cold water when no one no it; anyhow, this nigger got well, and drink two bucket full."

L. M. Vance, Esq., of Indianapolis, would now probably recollect a great number of cases that he saw with us, and many he himself, relieved, when we could not attend. He knew one regular M. D., who pursued it with great enthusiasm.

(To be Continued.)

ART. X.—*Radical Cure of Inguinal Hernia, by the use of the Truss:* By W. T. LEONARD, M. D., formerly Professor of Anatomy in the Washington University of Baltimore.

THE highest aim of surgery is to avoid the use of the knife, whenever it can be done with safety to the patient; and that surgeon is most truly great, who makes it his last, as it ever must be, his most terrible remedy.

It is in view of the magnitude of this truth that I propose a few

thoughts and illustrations of the nature and treatment of inguinal hernia, and on its curability by the use of the truss.

It is true the truss has been applied, in one form or other, to hernia, from time immemorial; or, if the instrument has not been used, the principle upon which it is applied has been a necessity growing out of the nature of the accident. This is self-evident.

It is not necessary to the purpose to be accomplished in this article, to enter into an elaborate description of the anatomy of the parts concerned in hernia; embracing the coverings, the situation of the epigastric artery and other vessels, and all the vexed questions which have been prolific themes for the ingenuity of anatomists and the casuistry of surgeons. As its purpose is, if practicable, to avoid the use of the knife altogether, it is not a matter of interest, at this moment, whether the hernial sac has but one homogeneous covering from the skin to the *fascia propria*, or as many as the scalpel of the most ingenious and astute dissector can make for it. Nor does it more concern our present purpose, whether it is necessary, in order to avoid wounding the epigastric vessels, in the operation for strangulated hernia, to cut "upwards and inwards," "upwards and outwards," or directly upwards," or whether safety to the patient indicates the propriety of "not making a decided incision in any direction, but just to nick the stricture all around."

If the purpose here suggested be at all times practicable, strangulated hernia need never occur, and the operation for its relief would be a matter of surgical history, to be pointed out as a course pursued by our fathers in the days when surgery was a terror.

It of course is not claimed as a new idea that inflammation excited along the inguinal canal, and terminating in the throwing out of coagulable lymph, which agglutinates the walls of the canal around the cord, or the round ligament, as the case may be, and to each other, will effect a radical cure of hernia.

The only thought for which any consideration is claimed, or to which attention is invited, is,—that it is practicable in all cases to *excite* that adhesive inflammation, and *produce* the effect which prevents the return of the hernia.

An inquiry into the anatomy of the groin intended to convey a clear idea of the formation and relations of the parts, having in view this leading idea will be serviceable.

Inguinal hernia is the descent of some portion of the abdominal viscera along the line of the spermatic cord, to a greater or less extent to the scrotum.

It is known as oblique inguinal hernia when, entering the internal abdominal ring, it traverses the inguinal canal, emerges at the external abdominal ring and descends to the scrotum in close association with the spermatic cord; as direct inguinal hernia when, breaking through the internal abdominal ring it descends to the scrotum, but not in the direct and close association with the spermatic cord as in the other case.

Oblique inguinal hernia is by far the most frequent occurrence; so much so, indeed, as to render the idea of a direct inguinal hernia an exceedingly hypothetical affair in the minds of some very good surgeons; and when we reflect upon the nature of the structures which must be broken up to produce a direct hernia, and the comparatively easy egress of the viscus by the oblique route, we are almost led to the belief that a direct hernia exists rather in the fancy of the surgeon than in the phenomena of surgical accidents.

I know I shall be met with profound arguments about the difference in the coverings, in a direct and an oblique hernia. The absence of the cremaster in the direct, and other arguments equally as conclusive to the mind of the surgeon who has been *taught* that such is the fact, and that such injuries are common. But let reason supply the place of such exceedingly *apocryphal* facts; and let your own experience that the place of the time-honored dogmas of the preceptor, and the idea that there *is no such thing* as a direct inguinal hernia existing as a primary condition is not so desperately absurd.

Mark! A direct inguinal hernia is, when a viscus breaks through the walls of the abdomen at the internal ring. What is the internal ring? Is it an *opening* that *weakens* the walls of the abdomen at that point? No! The internal ring is formed by the testicle, which has left its bed beside the kidney, pushing a *fold* of peritoneum before it, in its descent through the channel that nature had provided for its pathway to the scrotum, and a viscus seeking escape from the cavity of the abdomen, will scarcely form a new route when one has already been opened for it. Hence, oblique inguinal hernia is by far the most frequent, if not the only form of inguinal hernia.

A moment's consideration of the course traveled by an oblique inguinal hernia, will serve, not only to make this idea apparent, but to elucidate the plan of treatment, having in view its radical cure by the use of the truss.

We have seen the hernia enter the internal abdominal ring — we

have learned how that ring is formed. Next it traverses the inguinal canal. How is that canal formed, and what are its boundaries?

The floor and external wall of the inguinal canal are formed by the tendon of the external oblique muscle — that tendon being reflected on itself so as to form a semi-circular groove, in which the spermatic cord is received; the upper and inner walls of the canal are formed by the parietes of the abdomen.

This canal is about 1½ inches long, and terminates in the *external abdominal ring*. This ring, or rather this triangular opening, is formed by the tendon of the external oblique muscle, as it is separated into two distinct pillars or parts, one of which passes on to be inserted into the symphysis pubis; the other, into the spine of the pubis. Escaped from this, the descent of the Hernia is direct along the line of the spermatic cord, to the scrotum.

Thus we have seen exactly what an oblique inguinal hernia is; learned its course from the abdomen to the scrotum, and also the very important fact that the inguinal canal has a lining of serous membrane, the prolongation of peritoneum which the testicle had pushed before it from the margin of the internal ring.

It is an established fact in pathology, that serous membranes excited to acute inflammation will throw out a coagulable lymph, which, coming between two serous surfaces thus excited, will cause them to agglutinate. Here, then, is the whole philosophy of cure by the use of the truss. The hernia is completely reduced, and a truss is applied while the patient is yet in a recumbent posture. If, then, the truss is so adjusted that it maintains its position over not only the line of the external ring, but along the line of the canal, irritation, and consequent inflammation, of the lining membrane of the canal must be the consequence, and adhesion of the walls of the canal to the cord, or the round ligament, as the case may be, and to each other, will follow as a natural termination of this form of inflammation. This done, the hernial opening is hermetically sealed, so that the accident is not likely to recur. To accomplish this end has been the aim of several truss inventors.

I remarked in the beginning of this article, that the truss, in one form or other, had been used almost from time immemorial; but it must be remembered that it was used as a means of relief, not of cure; for until a comparatively recent period, it had been thought impossible to effect a radical cure of hernia by any remedial means, and the patient once subject to a rupture was deemed a doomed man, so far as that accident disabled him, and lived in continued

dread lest he should become a sufferer from that terrible concomitant, or rather consequence of the injury — strangulation of the hernia.

The idea of exciting adhesive inflammation and consequent cure by the application of external pressure, is comparatively of modern origin. But the idea once started, ingenuity was taxed to its utmost extent to accomplish the purpose.

Soon it was found that the old convex pad which had been used as a means of retaining the viscera in place, was not equal to the new duty imposed upon it; and with the facility with which extremes meet, the concave pad became fashionable, and in the earlier portion of my own surgical career, Hull's Truss, with concave pad and rotatory axis, was all the rage.

It may be well in this place to note the theory on which it was formed, for more recent aspirants for inventive honors have adopted the same idea, and are claiming credit for the discovery.

It had been found that the old convex pad, from being easily displaced, or partially displaced, and thus allowing the viscus to escape beside it, was not only inefficient in keeping the hernia reduced, but absolutely increased the trouble, by enlarging the opening at the external ring, by its continued and unequal pressure on its periphery. Meanwhile, some accidental cures had been effected by this very pressure. The attention of those interested in this department of surgery was at once directed to the fact, and, reasoning, that if pressure on the periphery of the ring would excite enough inflammation to *obliterate* the ring the pressure should, in all cases, be applied directly to the periphery; and it was conceived that a concave pad, made so as to cover a larger surface than the ring itself, would have the double effect of pressure at the proper point, and of drawing the ring into a smaller compass, and thus facilitate a cure. That it would, in fact, act as a species of external sphincter in contracting the size of the ring. Hence, the invention of Hull's Patent Truss, as before spoken of, and which, for a while was so great a pet of the profession.

Hull's truss, however, had a very short reign, for it was found to be obnoxious to all the inconveniences of displacement—*the great evil* to which the convex pad had been liable.

Next followed Semple's truss, with its conical pad of metallic lead. This was, by immediate and severe pressure upon the opening, to excite the peritoneal inflammation at once, and the salts of lead, formed by the action of the secretions of the skin on the

metallic lead were to act as refrigerant lotions to allay the external irritation induced by the pressure, and thus hernia was to be disarmed of all its terrors and receive a quietus *extempore.*

This notable invention soon found its grave beside the remains of its predecessors, and other and later contrivances assumed the sceptre of fashionable surgery in its stead ; each and every one of them, however, based upon the same idea, and, therefore, failing to achieve the desiderata which had been attempted by every previous invention. And at this day, the fashionable trusses are liable to the self same objection as those already enumerated—the constant tendency to displacement, and whether we look to Stegnal, Chase, Marsh, or any of the less pretending trusses of the present day, all are subject to the same objection, except one, as far as my knowledge extends.

Of Marsh's Truss it would be well to speak more at large, for as great efforts are being made to attract the attention of the profession to *it* at this time, it is well to canvass all its properties, and in a spirit of humanity point out its faults. First, as to its general formation; it is known results flow from too great an amount of pressure. The spring of this truss is very powerful, and presses with equal force upon the rupture and upon the spine. Next, the peculiarity of the pad is, that it is a combination of Hull's concave pad, and a modification of Semple's cone. Verily extremes have met in the pad of this truss.

The base of the pad is a flattened ring, made of pliant material and covered with buckskin ; to this another buckskin ring is added, when it is deemed necessary to apply an additional compress. This is Hull's concave pad in principle. Within the circle formed by this ring there is placed an exceedingly convex or conical pad, if a lump of ivory about the size of a hickory-nut, may be called a pad. This protrudes so far beyond the rest of the apparatus that the whole force of the spring will not give any pressure to the concave pad, while the continued application of the ivory must be productive of insupportable agony. The pad is fixed immovably to the mainspring, and, therefore, must press continually in the same spot.

There is no doubt but, if the pressure could be borne, that adhesive, or even depraved, inflammation would be excited; but if the abdomen did not revolt against such inhuman pressure, the spine would, and the instrument would of necessity be laid aside.

But is it not strange, that all attention has been directed to the *adjustment* of the rupture pad, and apparently without one thought

as to the collateral forces which operate upon it? For no care seems to have been taken, either in formation or adjustment, of the main-spring, or of its mechanical power in effecting a displacement of the rupture pad—the very evil most to be apprehended.

In every truss I have hitherto named, the rupture pad is placed at one end of a spring long enough to reach more than half way round the body, and a back pad is generally fixed at the other end; the two ends are then connected by a flexible strap of leather which completes the circuit of the body and is fastened to the rupture pad either by buckle or strap. Is it not at once manifest that the back pad is the fulcrum of a lever, on the long arm of which the rupture pad is placed, so that, with such immense mechanical disadvantage, no possible contrivance could keep the rupture pad in place?

One truss only, of which I have any knowledge, is not obnoxious to this objection, and, as that one has about it so much to commend it to the profession, I may be allowed to give a more minute description of it. I allude to Sherman's Truss. This instrument was invented by Dr. J. A. Sherman, of this city, and may now be obtained of him.

The pads, both for the rupture and the back, are fixed to a silver spring which nearly encircles the body, so that, when the instrument is adjusted, the back pads are applied on each side of the spinal column, but there is no pressure whatever on the column itself; from this point, the spring passes around the body, having impressed upon it just vertical curve enough to lift it above the crest of the ilium, the course of which it traverses until it reaches a point—until it reaches the anterior, superior spinous process of that bone. Here a new curve is impressed upon it which takes it directly over the course of Poupart's ligament; it passes along the line of that ligament until it reaches a point over the external ring; from this point it rises gently over the symphysis pubis, and with similar curves to those already described passes over the opposite side of the body to its place along side of the spine on the opposite side from that from which it started. Thus we see, that the point directly over the external ring, where the rupture pad is, of course, placed, is the stationary point of the apparatus—the *fulcrum of the lever*—and not the long arm as in the other cases. Here is the recognizance of a principle never before contemplated, by which an incalculable advantage is gained over any other truss in use.

The rupture pad is oblong, or ovoid, slightly convex, and ample in size. The greater diameter being placed on the line of the

inguinal canal, the larger end being applied to the external ring, so that the greater amount of pressure is applied there, which pressure gradually is diminished, until it passes over the internal ring.

This pad is fixed to the mainspring by an ingenious contrivance, which enables the operator to apply the pressure, however, in any required direction, and to maintain it there; or at will, let it be diffused over the whole surface of the canal and rings, and yield to the motions of the body; in which case the greater pressure is always on the most dependent portion of the hernia.

Thus we have a truss which answers every indication that can be desired by the surgeon. First and most important is the ease with which it is applied and its perfect freedom from the danger of displacement. Next, the manner in which the rupture pad is fastened to the mainspring as already explained. Again, the fact that the pressure being made on the rupture pad at every mechanical advantage, need not be so great, and therefore can, with more certainty, be borne by the patient.

Another and a very important consideration is, that the material of which it is composed is silver, and therefore not liable to oxidation by contact with the secretions of the body, so it does not, as in most other cases, form a nidus for the accumulation of filth.

This truss is equally applicable to single or double ruptures, as pads are fixed with equal facility on both sides; and the moment that the posterior indentation is made at the point over the external ring, each side is converted into a fulcrum of a double lever which is the spring.

I have spoken of pressure as being necessary to excite inflammation, but the principle here involved, seems rather to supply the requisite power to the walls of the abdomen, at this point, to enable them to resist the force of the abdominal muscles; and the mutual resistance thus established excites the irritation requisite to effect a cure.

The following are a few cases selected from a great number which might be adduced in proof of the efficacy of this instrument.

Case 1.—Mr. D. W. C. Campbell, a gentleman who resides in New Orleans, and who, I believe, is known to the Managing Editor of this Journal, was ruptured when quite a small boy (he is now thirty-three years of age). For some years he wore a common truss which had been applied by the physician to his father's family; but finding that it encumbered him and increased the trouble of his

hernia, he threw it away, and for some years was without any support, the hernia being always down except when he was in the recumbent position.

In 1853, he decided to try Sherman's Truss. He called on Dr. Sherman in December of that year, and had one applied. He wore this truss about twenty-two months, since which time he has had no symptoms of a return of the hernia, being capable of taking and does take athletic exercise without any unpleasant sensation. I have examined this gentleman and there is now no sign of hernia about him.

CASE 2.—Henry Tebbe, aged 18 years; lives at the corner of Thalia and New Levee streets, in this city. Has suffered from an oblique inguinal hernia since September, 1857; first used a truss which was applied by B. Pradel, of this city; after wearing it about four months, he found his hernia increasing in size, and it became very troublesome to him.

He then procured a truss of Dr. Sherman's manufacture. He has worn that truss for about two months. I examined this case on the 27th day of March, 1858, after this period of two months' wear of the truss. The examination was made while the patient was in the erect position; there was considerable external irritation, but the hernia did not protrude at all. I caused him to cough and exercise the abdominal muscles, but it still did not produce a protrusion of the hernia. He yet wears the truss, however, as a precautionary measure, but feels that he is relieved of the effects of the accident.

CASE 3.—Mr. L. W. Dodge, the road-master of the Opelousas Railroad, now living in Algiers, aged about 40 years, had an oblique inguinal hernia, produced by very great physical exertion in 1853. He applied to Dr. Sherman, and obtained from him a truss which he has worn regularly, except one or two intermissions of a month or two at a time. There has not been any appearance of the hernia for about three years; nor has he suffered any inconvenience when dispensing with the use of the truss; but as his occupation requires sometimes violent exertion, such as jumping on or off a train when in rapid motion, and as the truss is so perfectly comfortable, he wears it as a habit.

Other cases might be added did space permit their insertion.

ART. XI.—*Researches into the Natural History of Cholera. Fatal Cases:* By BENNET DOWLER, M. D. (Continued from Vol. XIV.)

In order to avoid as much as possible, tedious and unimportant minutiæ, and to save the reader's time without exhausting his patience, a brief, elliptical method not fully warranted by strict grammatical construction, will be adopted, in giving the history of cases and autopsies, whereby pages may be sometimes reduced to paragraphs. Tabulation is generally impracticable or unsatisfactory in symptomatic and post mortem descriptions, chiefly because differentiation, parallelism, identity, or analogy either in the cases themselves, or in their essential conditions, points of contact, and divergence must necessarily be omitted, and individuation must be either lost or replaced by an arithmetical skeleton, as repulsive in appearance, as it is limited in its analytic expositions, results and uses.

The series of post mortem researches which follow, and of which the present paper is a part, will be contributed without selection, omission, arrangement or classification. The reader will, therefore, form his own opinion concerning the pathological import of the anatomical lesions enumerated, and if possible determine the morbid anatomy of cholera. In this, if in any disease, purely functional disorder plays an important part, being an element of morbidity the inherent structural alterations of which are little manifest to the observer.

The great value of anatomical research into this as well as into other maladies, cannot be questioned. Sir Astley Cooper very justly says, that "great advantages may be derived from the examination of morbid structures, and from a comparison of their external character and symptoms with their internal appearances. Such a comparison facilitates the treatment and cure of diseases. The difference between the experienced and scientific, and the ignorant and unobserving member of the profession, is manifested, in the former readily discovering the distinctive character of disease as soon as it is presented to his attention; while the other guesses at its nature, and if right is right only by accident. It must likewise be admitted, that though no specific remedy may have yet been discovered for the cure of some diseases, it is still a great advantage to be able to discriminate curable from incurable cases; the dangerous from the slight; those which require surgical operations from those which do not demand them; and such as admit of a trifling operation, from those which call for one of great severity." (*Dis. of the Breast.*)

It is not accurate to affirm, as many do, that pathological anatomy

wholly fails to throw light upon the pathology of cholera, because its lesions are neither uniform nor always strongly developed. But as already indicated, the formal examination of this branch of study does not enter into the plan of this paper.

I. May 14th, 1851, half-past eleven, A. M.; C. B., born in Germany, aged forty-seven, resident twenty-three years, single; had, yesterday, vomiting, purging, and cramps; these continue; lips, hands and nails of a dark blue, arms and face bluish in a less degree; skin cool and clammy; no petechiæ; eyes injected, not sunken; voice a faint, low whisper; tongue swollen and not easily protruded; its sides deeply indented by the teeth; a red line runs along the dental border of the gums; thirst; the pulse occasionally perceptible in the wrists; intelligence normal; is apparently free from pain and restlessness, the body being torpid like a log. Axilla, ninety-six degrees; bend of the arm, ninety-one and a half degrees; hands, eighty-four and a half degrees.

Took yesterday cal., camph., capsici; aa 3i, in six powders—one every half hour; had brandy freely; had sinapisms applied; took also carb. ammon. at intervals.

May 14. Blister to the epigast.; sinapisms to the extremities; quinia grains thirty; also tinct. opii for an enema; brandy. Evening: tinct. opii 200 gtt. for an enema.

May 15. Died quietly, without delirium, at nine o'clock, P. M.

Post mortem examination thirteen hours after death. Body nearly as warm as in health; rigid; the blueness of his hands and lips had faded. The gums pale, with a blue line along the dental margins; abdomen without convexity; muscles and fat natural. Some old cicatrices on the shins which are blue and congested; cadaveric injection slight, if any.

Head. Brain natural; pia mater quite vascular, and with the arachnoid was increased in tenacity and opacity. Intra-arachnoidal and ventricular serosity about five ounces.

Tongue swollen, reddish; fur dark, saturated with blood; papillæ hypertrophied. (Glossitis.)

Pharynx red. Gullet contained a thick milky liquid; its lower third had sub-mucous injection.

Abdomen. The stomach contained a greenish water; its greater curvature infiltrated with sub-mucous blood-dottings, also arborizations with softening two or three inches in length and half as wide; below and around the cardiac orifice the mucous tissue was thinned,

but little remaining over an extent equal to one-fourth of the internal surface.

The intestines were in some places contracted and dyed in a greenish bile, with considerable injection in two places; slight incipient development of the Peyerian glands in plates. The large intestine healthy, but contained rice-colored liquid.

The bladder contained an ounce or two of rice-water liquid without any tinge of urine. The kidneys were flaccid. The spleen enlarged three times and firm; its envelope thick, tough, almost cartilaginous (supposed to have originated from intermittents). Pancreatic, mesenteric, lymphatic, and salivary glands natural. Pleurae and peritoneum dryish or sticky.

Chest. Trachea and lungs natural. The whole thyroid gland is more or less infiltrated with bony matter; the heart, the systemic, pulmonic, and abdominal arteries enormously enlarged, or rather dilated. The auricles and ventricles dilated in an equal degree. The right auricle and ventricle contained a large mass of a pale, dirty yellow, fibrinous concretion. The blood which abounded in the large veins had a black hue and an adhesive consistence.

The liver large and congested; its inferior or concave sub-serous tissue, with a portion of the duodenal and sub-peritoneal tissues are for two or three inches in several directions of a black greenish color. Bile had transuded from the capacious gall-bladder which still contained two or three ounces.

At the origin of the aorta, and in the semilunar valves, scales of bone had formed in the sub-serous tissue, some being near the size of the finger nail. The aorta at its transit through the crura of the diaphragm becomes bony for two or three inches, though the bony degeneration does not occupy the entire circle of the tube.

The blueness of the skin, and the failure of the pulse, though, to considerable extent common in cholera, were probably aggravated in this by functional impediments from the dilatations and osseous transformations of the heart and great arteries. But judging from the physical appearances of the body, these changes of structure, doubtlessly of long standing, had not impaired the nutritive actions of the general economy.

II. 1848. Dec. 17th, half-past one o'clock, P. M.; L. A., German, aged thirty-six years, resident eighteen months; had cramps and vomitings yesterday; is now collapsed; voice low and hoarse; skin variegated with blue and a dusky red; hands wrinkled; external veins comparatively empty; eyes injected.

A physician who relied on the cold bath as the most certain remedy

for curing cholera, by, as he affirmed, bringing on subsequent reäction, obtained permission to try his method on this patient, whom he immersed in hydrant-water in a bathing-tub ; the water which was at fifty degrees, was also poured on his head freely. The patient, in the mean time, complained much of suffering from the cold. One arm was raised out of the water, at the bend of which, the temperature was seventy-nine degrees. Under the tongue it was eighty-three degrees. The experiments lasted five minutes, the patient having become nearly pulseless during the experiment.

The patient was removed from the bath to the bed. Bottles of warm water were applied to his feet and his body covered with blankets. In an hour after, his pulse was perceptible though feeble, thready and unsteady; some small veins on the back of the hands were distended ; intelligence natural ; complains of coldness of the feet. Tongue, eighty-eight degrees; bend of the arm, ninety-one degrees; axilla, ninety-three and a half degrees.

The regular physician of this patient had been administering to him free doses of carb. ammon., capsicum, and brandy; blisters also were applied.

Died during the night. Learn that after coming out of the bath, water for drink was interdicted, and that the patient had in consequence secretly drank all of the warm water out of the bottles which had been applied to his extremities.

Autopsy fourteen hours after death. *Body* muscular ; adipose tissue moderately developed ; rigid, except the neck ; elevated as well as dependent parts of the surface marbled with blue discolorations; abdomen free from convexity ; eyes injected ; corneæ dull or glassy; muscles natural, inclining to paleness.

Head. Venous vascularity; brain firm, less moist than natural.

Chest. Lungs natural ; about three pounds of black, thick, adhesive, (as if greasy) blood flowed into the cavity on cutting the large vessels; the pleuræ adhesive or sticky; liquor pericardii only a few drops; right side of the heart greatly distended with black blood like the above, partly coagulated ; left ventricle and auricle filled with a dense, tenacious, yellow, bloodless, strongly adherent polypous or fibrinous concretion, estimated at from one and a half to two ounces ; air passages pale and bloodless.

Abdomen. Tongue, fauces, and salivary glands natural though comparatively pale; the mucous tissue of the œsophagus discolored with a yellow stripe three inches long and the fourth of an inch in width ; stomach contracted, the larger or cardiac portion but three

fingers' width; the pyloric half still smaller; it contained a little yellow mucus as if tinged with bile.

Almost the entire intestinal canal was distended with the opalescent choleraïc liquid like milk and water or milk and gruel, though somewhat tinged with yellow; in the large intestine the liquid became turbid or muddy.

The mucous tissue of the stomach was corrugated, of a leaden hue, uninjected; the ileum had some red elliptical patches of Peyer an inch long, and quarter of an inch wide, but of different sizes, with also a complete eruption of white, round, salient, rough, firm, isolated granular masses or points nearly as large as barley grains (the solitary glands hypertrophied); softening of the mucous tissue in several places; the large intestine was dotted over with many enlarged solitary glands; the mucous tissue, particularly in the rectum, had many ulcers without raised edges or redness and of many different shapes, round, angular, oblong, complete excavations extending to the muscular coat, generally about the third of an inch long and half as wide; kidneys, pancreas and spleen small, the latter flaccid; mesenteric glands slightly reddened. Liver flaccid, pale, otherwise of good consistence; gall-bladder distended (℥iis) with bile as thick as molasses, of a dark color, but on spreading it out or diluting it with water it was found to be of a yellow, natural color. Bladder contracted, pale, totally empty. All the serous tissues adhesive to the touch and comparatively dry.

III. 1848. December 18th, ten o'clock, A. M.; dead house seventy-four degrees; dissection by several doctors. A young unknown man was admitted into the hospital an hour since, dying of algid cholera. Was examined half an hour after death. His temperature forty minutes after death was in the rectum one hundred and three degrees, at sixty minutes one hundred and two degrees.

Body very muscular, large, and free from emaciation; muscles and adipose tissue natural; eyes glassy; no cadaveric injection.

Head. Pia mater vascular; serosity about one and a half ounces in the arachnoidal sack.

Chest, natural. The heart after removal from the body and the opening of the ventricles became rigid in half an hour in an open, gaping manner; blood thick, black, adhesive, scanty, clotted slightly.

Abdomen. Stomach, contracted, nearly empty, contained a little yellow liquid and several portions of apple; mucous tissue corrugated and mammillated; had several longitudinal red striæ of

the mucous tissue in a dotted form. The external appearance of the intestines, mesentery, omentæ, etc., livid, reddish, marbled. All the serous membranes of the cavities felt greasy and adhesive and diminished in moisture.

IV. December 19th, 1848, half-past one o'clock, P. M.; air about eighty degrees. D. M., aged twenty-five years, steamboatman, admitted into the hospital yesterday at half-past seven o'clock, P. M., with vomiting, purging, and severe cramps; skin cold, corrugated, blue, clammy; thirst; eyes injected. On rising to stool, fainted and fell several times; retained his senses until about two hours before death.

Dissection one hour after death: neck, jaws, fingers, and recti muscles rigid; semiflexion of the forearms on percussing the biceps; face blue, marbled and congested, as were some other portions of the skin; much of the skin was smooth, shining, having innumerable small veins finely injected on the trunk. This injection increased. The veins of the arm filled. The jugulars were distended; one was opened and about six ounces of blood flowed in four or five minutes, being very black and of a glucy or gummy consistence, clotting but slightly. The subclavian was opened. The blood flowed rapidly for several minutes, perhaps, twice as fast as an ordinary venesection. Other vessels were divided, and as the lungs were collapsed to the size of the feet, the blood accumulated in the chest; a medical gentleman present estimated the amount at four pounds, in which estimate I concurred. Nearly all of this blood accumulated in half an hour; it was black, adhesive like honey, or mucilage, clotted slowly and not firmly. The muscles were large and less pale than healthy muscles. The adipose tissue somewhat dusky in color and sticky or gummy to the touch; eyes injected, having ecchymosed spots; cornea glassy and dry. Rectum, two minutes, ninety-nine degrees—five minutes, one hundred and one and a half degrees; axilla, ninety-four degrees.

Brain. Venous vascularity; subarachnoidal and ventricular serosity amounting to about four ounces; brain firm.

Chest, natural except the absence of the liquor pericardii; right side of the heart distended excessively; lungs extremely collapsed.

Neck, trachea, gullet, salivary glands, tongue, fauces, pale and bloodless.

Abdomen. Omentæ and mesentery loaded with fat; these tissues, also the cardiac end of the stomach, part of the pancreas, part of

the abdominal peritoneum, much of the left kidney, adhered more or less to the spleen by strong, almost cartilaginous bands, free of blood. The serous coat of the spleen white, being thickened perhaps, twenty times, cartilaginous ; its parenchyma firm, tough, bloodless, being enlarged three times, doubtlessly an old lesion.

The peritoneal cavity dryish, or rather coated with a gluey adhesive exudation, which on separating the fingers that had been exposed to it, drew out into fine threads like spiders' web.

Bladder totally empty; much of the colon contracted ; kidneys natural ; stomach contracted, being about three times less than usual, pyloric half rugous, cardiac portion thin, its mucous coat soft ; it contained yellow serosity and two pieces of liver, (probably beef's) as large as a hazel nut each, and of angular shapes as cut with a knife.

The small intestines contained serosity of whitish color, tinged with yellow. The large intestine, where not contracted, contained an opaque milky or rice water liquid ; twelve inches of the lower part of the ileum intensely red from injection of the sub-mucous coat; slight development of the solitary glands chiefly in the ileum and colon. Liver enlarged one-third, cork-colored, of good consistence; gall-bladder distended with dark bile as thick as molasses, ropy, becoming yellow on dilution. Mesenteric and lymphatic glands natural.

The blood did not, in two hours, separate into serum and clot as usual. In twenty-four hours after death, and twenty-three hours after the blood had been discharged from the jugular, it continued clotted, adhesive, not very firm, with but very little serosity upon the outward margins of the crassamentum.

The bowels throughout their whole course contained either serosity or dense white opaque concretions ; the latter had the appearance of semi-coagulated albumen or chyle combined with mucus. These masses and the serosity became turbid in the lower portion of the ileum and in the large intestine.

The solitary glands were more or less hypertrophied both in the large intestine and in the ileum; in the latter about one dozen of the elliptical patches of Peyer were developed in a raised, not ulcerated form, having a dark red color.

Urinary bladder empty and contracted. The gall-bladder contained about an ounce of normal bile; the common duct pervious.

V. May 15, ten o'clock, A. M.; M. S., female, born in Ireland, aged eighteen years, single; of medium size; resident six months; was admitted, May 5, for dysentery, for which she was treated about one week; her disease assumed the form of acute diarrhœa, and this was followed by the cholera. The vomiting and purging were severe and prolonged until she died.

The treatment: opiates, camphor, etc.

Dissection four hours after death. *Body* warm, rigid. *Abdomen* free from convexity; no marked external indication of cadaveric injection.

Head. The brain was taken out but not divided; its exterior healthy. Adipose and muscular tissue natural. The left tonsil contained a drachm or more of pus. The lymphatic and salivary glands natural. The thyroid hypertrophied two or three times, and degenerated into granular and striated scirrhus.

Chest, healthy; lungs collapsed; membranes sticky; both sides of the heart had yellowish fibrinous concretions. Lower third of the gullet injected.

Abdomen. Greenish liquid in the stomach and upper portion of the small intestines. Punctiform injection with softening of the mucous tissue along the greater curvature of the stomach; a considerable space at and around the cardiac appeared to have little mucous tissue remaining, the muscular coat appearing of a silvery white. Stomach large. The ileum had a considerable portion invaginated, but the tissues were not altered in a marked degree. The submucous tissue of the small intestines injected were in some places contracted; both bile and small portions of fæces were found. The large intestine was much thickened, its calibre diminished: the mucous tissue contained innumerable isolated ulcers, as large as half a pea, with many elevated points like scabs. The intestines contained a little gruel-like, milky liquid. The pelvic basin contained in the peritoneum about one or two ounces of liquid like urine; the bladder empty. The uterus and ovaries engorged, as was the liver. The gall-bladder contained yellow greenish bile; kidneys contained a few milky drops. Mesenteric glands, spleen, pancreas, cavas, and right side of the heart loaded with black, thick, and uncoagulated blood.

PROGRESS OF MEDICINE.

ART. I.—*Experimental Researches on Normal Albuminuria in Man and Animals:* By DR. CLAUDE GIGON, Physician to the Hospitals and Prisons of the City of Angoulême. Translated for this Journal, from *L'Union Médicale*, by M. MORTON DOWLER, M. D. (Continued from the March No.)

LET us now turn our attention to the phenomena observable in the urine as disclosed by experiments. If we take the urine of any individual in health, whether it be of man, woman or child, and subject it to the process of boiling, or, indeed, drop into it a few drops of nitric acid, no turbidity is seen, nor any precipitate. If a few drops of the rectified alcohol of pharmacy be used in a test tube, a light annular flocculency will make its appearance a little below the top of the fluid, this flocculency being whitish, and very nearly identical with what is observed in feeble dilutions of the white of egg, or of normal albumen.

If we pour into an experiment tube a few grammes of urine, and drop into it a few drops of creosote, these oily drops do not mix with the fluid, nor do they produce any change ; but if the fluid be shaken the liquid becomes whitish, and if then left to settle, it will throw to the bottom an abundant precipitate. If, moreover, we treat the urine with a strong solution of tannin, a precipitate equally well pronounced will be produced. Finally, if we treat in the same manner, the same quantity of urine with from ten to twenty drops of chloroform, we shall see this liquid descend by its greater specific gravity to the bottom, and disperse by its transparency, the yellow color of the urine at the points of contact. There is at first no change from the action of the chloroform, and no precipitate ; but if the liquid be shaken it becomes turbid, and presently there is deposited at the bottom of the tube, an abundant white precipitate of a more than oleaginous consistence. This precipitate acts in the presence of nitric acid and in that of potassa, in the same manner as do the precipitates of the highly diluted solutions of the white of eggs, and if the liquid be decanted the precipitate remains dissolved at the bottom, in a large excess of nitric acid and in a supersaturat-

ed solution of potassa, in which latter case the chloroform is revived.

As to the reäctions produced on urine by the agency of the metallic salts, the nitrate of silver, etc., I only mention them here to recall the fact that though these reäctions under these circumstances are distinctly recognizable, it is not necessary at present to take them into strict account, since it will be found that these same salts produce reäctions very nearly similar to those produced by salts normally contained in the urine.

If after having, by the aid of a few drops of chloroform, obtained a precipitate in an experiment tube containing urine, we decant the supernatent fluid, so as to leave nothing excepting the deposit, and treat the latter with a few grammes of chloroform in excess, with strong agitation, there here occurs what we have shown to take place in experiments made with white of eggs, that is to say, the molecules of the urinary deposit, at first mixed with the liquid, are rapidly separated and rise above the excess of chloroform, where they form a *cylindrical coagulum*. The minute quantity of urine present is separated, and being lighter than the coagalum, takes its place above it, the latter being seen between the colorless chloroform below, and the minute quantity of urine—known by its yellow color—above.

I have repeated these experiments a great number of times, with the same result, on the urine of animals, and it appears to me that the coagulum, thus obtained, examined comparatively with the same quantity of albuminous liquid of which we have spoken, exhibits a very great approximation on the part of the quantity of albumen contained in normal urine, to the solutions of the white of eggs, at the seventh or eighth hundredth dilution

And here, before proceeding further, we may add that we have obtained the same reäctions in the treatment of vegetable albumen by the whole series of reägents. Thus, after having pressed the juice of scraped turnips through linen, and filtered on washed animal charcoal, the liquid was obtained perfectly limpid. This treated successively with heat, nitric acid, tannin, creosote, alcohol, and the metallic salts, it has exhibited reäctions quite identical with those of the white of eggs treated by chloroform according to the method which we have already indicated, forming an abundant white precipitate, which in the presence of nitric acid, potassa, and chloroform in excess, acted in every respect like the solution of the white of eggs, and the albumen of urine. The juices of carrots, cabbage,

potatoes have been in like manner, experimented upon, and with the same results.

A further experiment was made by us which appears still more conclusive. "Among the properties *of albumen* we must especially note the reäction which it manifests in its relations to the oxide of copper and potassa, in which it forms a true double salt of a beautiful violet color. By mixing the hydrated oxide of copper with the albuminous fluid, and adding potassa, the violet solution is produced." (*Dumas. Loc. Citat.* p. 343.) We have performed this experiment with the pure white of eggs, and with solutions of the same, augmenting the dilution, as also with the serum of the blood, and the same beautiful violet blue coloration—becoming paler according to the degree of dilution—has always appeared. Before the fluid is carried to the 1–1000 dilution, the oxide of copper and the potassa ceases to display this coloration.

If the fresh juice of plants, containing vegetable albumen, be treated with the hydrated oxide of copper and a concentrated solution of potassa, the same phenomenon is observed, that is to say, the liquor becomes blue in a very characteristic manner; but another singular fact discloses itself, namely, that while the blue color is indefinitely maintained and continued when animal albumen is employed, the color disappears in a few hours when the juices of plants which contain albumen are submitted to the experiment. Thus with the juice of carrots and turnips, the hydrated deutoxide of copper, the pale blue changed to orange yellow in the same manner as the protoxide of copper resulting from the reduction of the liquor of Barreswil by glycose. The juice of cabbage, on the contrary, also at first blue gives place to a brown deposit, which is itself probably nothing other than a deutoxide of copper.

If the albumino-chloroformic precipitate, either animal or vegetable, be treated in the same manner the deposit is dissolved, the chloroform is revived, and the line of separation between the chloroform and the albuminous liquid is indicated by a ring of metallic copper on the tube.

If human urine be treated with the hydrated oxide of copper, and by potassa, the same blue colorization will be seen that is seen to occur with animal and vegetable albumen, excepting that the intensity of color, which attend the concentrated solutions of the white of eggs, is wanting. But the color is identical with highly attenuated solutions of the latter, being blue by a reflected, but of a slightly greenish tint by a refracted light. This coloration in animal albumen is preserved for an indefinite period.

The article of M. Claude Gigon, a portion of which we have already translated and published in this Journal, has been spun out to so great a length, that we have declined to follow the author further with translation. He goes largely into the experimental examination of the urine of animals, from the frog up to the ox. Let it suffice to say, that after pronouncing the existence of such a body as *albumen*, as announced by M. Mailhe, as little else than a hypothesis, he closes with the following general conclusions:

1. That chloroform is of all known reägents the most sensible to the presence of albumen; seeing that the latter is thereby revealed in the most evident manner, when diluted with twenty thousand parts of water. 2. That man and all carniverous animals have, in the normal condition, constantly a certain quantity of albumen in their urine. 3. That while all herbiverous animals in which the pulsations of the heart number less than sixty in a minute, have no albumen in their urine; the urine of all those in which the pulsations exceed seventy, exhibit albumen in the most evident manner. 4. That animals having a slow cardiac pulsation, as the ox, the horse, the ass, in the urine of which albumen is not ordinarily found, have that secretion rendered albuminous when, by any accidental cause, their circulation becomes highly accelerated, a quality which after some days of repose disappears.

Such are the fundamental conclusions of M. Gigon. *M. le Docteur Becquerel, Professeur Agrégé à la Faculté de Médecine, Médecin de l'Hôpital de la Pitié, in L'Union Médicale*, for December 1, 1857, publishes a long experimental paper in which he attacks with no sparing hand the whole of the fundamental conclusions of M. Gigon. We shall proceed no further with the paper of M. Becquerel than to give the general conclusions which he draws from his experiments which are as follows:—1. That normal urine to which chloroform has been added with agitation, gives a precipitate which is nothing else than a simple emulsion formed by the chloroform on the one hand, and on the other by the mucous and organic matter contained in the product of the urinary secretion. 2. That normal urine contains no trace of albumen. 3. That chloroform is a most unreliable reägent in the detection of albumen precipitating only a part of the albumen, and leaving intact and in solution, in the upper part of the fluid most of the albumen which it may be found to contain.

Results so radically different obtained at the hands of two such able observers, are an instructive comment on the value of a great mass of the experimental physiology of the day.

ART. II.—I. *Albumen, Starch, and Gum, as Diet.* II. *Physiological relations of Albumen.*

I. *Albumen, Starch, and Gum, as Diet.*

WILLIAM A. HAMMOND, M. D., Surgeon U. S. A., to whom was awarded a Prize by the *American Med. Assoc.*, (1857) for an Essay entitled, *Experimental Researches relative to the Nutritive value of Albumen, Starch and Gum, when singly and exclusively used as Food*, concludes his elaborate Memoir occupying seventy-seven pages of the *Transactions*, with the following *Résumé*:

From the preceding investigations, I think the following conclusions (several of which, however, are already well established) fairly deducible, and applicable to the human subject:—

1. That albumen may be assimilated into the system in such quantity as to furnish a sufficiency of both nitrogen and carbon to the organism.

2. That under the use of an exclusively albuminous diet the nitrogenous constituents of the urine are increased over the ordinary average amounts, though not in proportion to the quantity of albumen absorbed into the circulation.

3. That either some other means than the urine exist for the elimination of nitrogen from the system, or the excess (over two-thirds) is retained in the organism, even when the body is rapidly decreasing in weight.

4. That the continued use of albumen as an article of food increases the proportion of this substance (and of fibrin) in the blood, and in a short time causes it to appear in the urine.

5. That whilst pure albumen cannot be regarded as of itself adequate to supply the several wants of the system, there is no reason why, when associated with suitable inorganic matters, it should not support both life and health.

6. That starch can be assimilated by the absorbents in more than sufficient quantity to sustain the respiratory function.

7. That under its use the nitrogenous constituents of the urine are very much reduced in amount, even below what would probably occur during inanition, and, that although starch is not capable of nourishing the tissues, it is yet serviceable, aside from its heat producing power, in retarding their destructive metamorphosis.

8. That the continued use of highly amylaceous food causes the appearance of sugar in the urine.

9. That under the use of such aliments the nitrogenous constituents of the blood are diminished, and the carbonaceous increased.

10. That gum is altogether incapable of assimilation, and therefore possesses no calorifacient or nutritive power whatever, but is, on the contrary, a source of irritation to the digestive organs.

11. That in consequence of the above fact, the solids of the urine during the immediately preceding researches, were entirely derived

from the waste of the tissues of the body, and the carbon exhaled by the lungs from the consumption of its fat.

12. That gum, when exclusively used as food, from the irritation it causes in the intestinal canal, and the fact of its non-assimilation, induces more constitutional disturbance than either starch or albumen, and that under a similar condition starch is more productive of ill consequences than albumen.

II. *On the Physiological Relations of Albumen.*—By DR. HAYDEN, Professor of anatomy and Physiology. Read before the British Association.—The inquiry of which this paper contains the first part, was undertaken with the view of determining the relations which subsist between the albumen and other organic constituents of the blood. It was suggested by the apparently anomalous fact, that in various conditions of the animal body—sometimes of confirmed disease, occasionally of temporary derangement—one of the staminal principles of the blood is eliminated in large quantity by the excreting organs; and regarding these, as I conceive we must, not only as depurants of the system, but likewise as adjuvants of nutrition to the extent of removing from the body superfluous alimentary matter, from whatever cause present, we are warranted in concluding that the state of the economy in which this occurs is one of repletion, or oversupply as regards the principle eliminated and the actual requirements of the body for that particular principle. It is well known that the "elements of respiration" may be stored up in the body by a process of deposition in the form of fat, to meet the urgent demand involving a large expenditure of these elements, to which variations of external temperature and other circumstances occasionally expose the animal. Not so, however, the plastic element of nutrition; these are appropriated only as required for immediate use in the renovation of the tissues, and if from any cause one happens to be in excess in the blood, whether absolutely, as the immediate result of ingestion, or relatively by loss of some of the allied constituents, then a process which may be conveniently designated *elemental adjustment*, is set up, by which the principle in excess continues to be discharged from the system, till it attain the normal proportion relatively to the other staminal elements.

Many facts may be adduced in proof of this self-adjusting property in the blood. Kaupp asserts that the quantity of chloride of sodium excreted by the kidneys is usually in proportion to that ingested, but when, after prolonged abstinence from this salt, a large quantity of it is taken, then the quantity excreted in the urine is less than that ingested. Andral says that the first effect of hæmorrhage on the constitution of the blood is manifested in a decrease of the corpuscles only, but if it be prolonged or repeated, the albumen and fibrin are found to have undergone a corresponding diminution, whilst, according to Becquerel and Rodier, the equilibrium is not restored through the blood drawn, in which the corpuscles and albumen observe a regular and equal ratio of decrease with each subsequent bleeding. Andral further states that some females suffer

during pregnancy a loss of corpuscles to the amount of twenty-seven parts in a thousand of the blood, and become anæmic; we know that it is in such cases more especially albumen is found in the urine of pregnancy. An absolute or relative decrease of the fibrin, as in scurvy and plethora, will give rise to hæmorrhage, by which, in the words of Andral, "the equilibrium is spontaneously established between the fibrin and globules." Becquerel and Rodier analyzed the blood in fifteen cases of Bright's disease, and found a mean of 117.28 parts of corpuscles and 60.58 of albumen in a thousand; the decrease of albumen was greater in proportion to the length of time that had elapsed from the commencement of the disease. This has an obvious relation to the progressive diminution in the quantity of blood corpuscles, as evinced by the increasing pallor of the patient. In thirteen cases of chronic Bright's disease, in which the blood was analyzed by the same pathologists, it yielded a mean of 108.8 parts of globules and 55.93 of albumen per thousand, whilst the fibrin increased to a mean of 4.37. The blood occasionally found in the urine in these advanced cases exhibits a marked alteration in the physical character of its corpuscles, which are serrated or broken up into a *detritus*.

In order to determine the value to be attached to the views here enunciated, I performed some experiments, the result of which I beg to offer to the Association. The experimental portion of the inquiry had a threefold object: 1. To determine the effect produced on the composition of the urine by inducing a relative or absolute increase of albumen in the blood. 2. The action of urea on the blood-corpuscles as exhibited by the microscope; and 3, the proportion of albumen contained in the serous effusion of renal and cardiac dropsy respectively.

The first mentioned object I thought would be best accomplished by abstracting blood from living animals, the quantity to be such as to make a decided impression on the system, and determined by the absolute weight of the animal's body. It is obvious that the effect of a first bleeding of this kind on the constitution of the blood remaining in the body would be a decrease in the proportion of corpuscles, and a relative increase of the albumen. An absolute increase of albumen might be induced by bleeding an animal to a small amount, and then injecting into the vein an equal quantity of a solution of albumen, having the temperature and specific gravity of the serum of the blood. The contrivance adopted for collecting the urine was a wire-crib, placed on a concave zinc table, having an aperture conducting into a receiver fixed beneath; in this the animal was imprisoned till a sufficient quantity of urine was obtained for analysis, when it was liberated in order to take exercise till the next experiment. A rabbit, weighing three and a half pounds, was fed on cabbage, milk, and water; the urine passed next day was feebly alkaline, specific gravity 1.020, and free of albumen. The animal was then bled to six drachms; and fed on fresh grass and warm milk. The urine examined the following day presented the same reäction and specific gravity as at first, but became distinctly

opalescent on the application of heat and nitric acid. A dog of eighteen pounds' weight was fed on milk and oatmeal stirabout; urine neutral; specific gravity 1.020; contained no albumen. The animal was bled to eight ounces on the 18th August; on the 19th the urine collected during the previous night was examined and found neutral, specific gravity 1.030, contained traces of albumen. Examined again on the 20th, the urine was found alkaline—its specific gravity had fallen to 1.020, but still a trace of albumen. August 21st, specific gravity 1.022; albumen as on yesterday. The quantity of blood taken in these two experiments was determined by the estimate of Welker, according to which the total quantity in the body of an animal is equal to one-thirteenth of its weight.

With reference to the detection of albumen in fluids containing only a minute trace of this principle, the aid afforded by the microscope deserves to be noticed: if a drop of the suspected liquid, the opalescence of which is barely discernible under the ordinary tests, be placed on a slip of glass under the microscope, and a drop of nitric acid added, a cloud of minute vesicles will be observed to pass slowly over the field of view, and if the line of advance of this cloud be accurately marked, the suddenness with which the constituent vesicles start into view from an apparently structureless fluid, cannot fail to strike the observer. These minute bodies present a highly refractive margin with a light centre, and an average diameter of 1-1000th part of an inch.

A young dog, weighing twelve pounds seven ounces was next subjected to experiment; the urine collected before operating was free of albumen and alkaline; specific gravity 1.005; blood was now drawn from the jugular vein, to the amount of five ounces, and into the aperture in the vessel was injected half an ounce of fresh dilute ov-albumen having the temperature and density of blood serum; bread and warm milk were given as food and ravenously eaten. The following day it was found that no urine had been passed in the interim. August 27th, four ounces of urine were collected, neutral in reäction; of specific gravity 1.030, and highly albuminous, being almost gelatinized by heat and nitric acid. The albumen was coagulated and collected by filtration, next dried, pulverised, and deprived of impurities, by ether and boiling water, subsequently dried and incinerated; the total quantity of pure albumen thus obtained was nine grains. In order to determine what proportion of this was due to the ov-albumen injected, and what, if any, to the ser-albumen discharged in consequence of loss of blood sustained by the animal, I endeavored to ascertain the amount of pure dried albumen yielded by half an ounce of the white of egg, and found it to be seventy-two and a half grains. I had expected, and probably would have found, had the operative part of the experiment been in all respects successful, a balance in favor of the albumen execreted with the urine, as compared with the quantity contained in half an ounce of the white of egg; but unfortunately at the moment when the last portion of albumen was injected, and before a ligature could be applied to the open vessel, the dog

struggled violently and caused a fresh loss of blood, with probably more than a proportionate loss of the albumen injected, as the bleeding occurred chiefly by regurgitation from the heart. Five ounces of urine were obtained from this dog on the 28th August ; specific gravity 1.010; feebly acid, but no trace of albumen.

With reference to the next subject of inquiry indicated at the outset, namely, the visible effect of a strong solution of urea on the blood corpuscles, the result of my experiments was not as decisive as I had expected, yet it was sufficient to show that when exposed for a few hours to the influence of this agent in a concentrated form, the corpuscles undergo a considerable modification. They are much diminished in number, and those which have not disappeared are become tumid and spherical ; the action, however, is by no means energetic.

The proportion of albumen contained in the different dropsical effusions next engaged my attention. I had reasoned thus, *a priori*. If albuminuria be the result of an effort of the blood to restore the equilibrium between its corpuscles and albumen, by ridding itself of a portion of the latter ; if anasarca be a concomitant symptom of the same disease, and produced by the same cause, then we may not unreasonably expect to find an excess of albumen in the serum effused. In order to determine this point, it became necessary to institute a comparison between the fluids of cardiac and renal dropsy. I have been so fortunate as to obtain a sufficient quantity for examination from two patients affected with chronic Bright's disease, but failed in the short time allowed me to procure any from a case of cardiac dropsy.

This is the less to be regretted, since Andral's data are amply sufficient for our own purpose with reference to the latter disease, although as regards the former he has not supplied us with any information bearing on the point now under discussion. Andral analysed the serous effusion in sixteen cases of cardiac dropsy, and found a proportion of albumen ranging between four and forty-eight parts in a thousand. The facts elicited by myself from the two analyses I have made, do not, I am bound to say, support the hypothesis with which I started, namely, that the percentage of albumen would be found higher in the fluid of renal than in that of cardiac dropsy. In the first case examined the proportion was twelve, and in the second twenty-four parts in a thousand. It will be observed that the higher of these two numbers stands midway between the minimum and maximum of Andral.

I now venture to submit in the form of propositions, the inferences which I think may be drawn with at least the force of probability from the preceding data. The inherent property of *quantitative adjustment* in the blood, probably has reference mainly to nutrition, which would appear to require as an essential condition for its healthy exercise, certain fixed mutual proportions between the constituent elements of the blood, but in part also to the maintenance of the normal relative density between its serum and corpuscles. The appearance of albumen in the urine is either transitory or per-

sistent ; when transitory it is produced either by an error of excess in the use of proteïn substances of which the blood seeks to relieve itself through the kidneys, or by a state of congestion of these organs in which the blood serum transudes through the walls of the renal capillaries and is discharged by the urine. When persistent it is probably always the result of loss or solution of the blood corpuscles, and produced by an inherent self-regulating property in the blood, by which the normal proportion between its constituent elements is sought to be reëstablished. In acute renal dropsy, the *point de départ* in the blood changes would appear to be loss of albumen ; but in the chronic form of the disease, attended with uræmia, the starting point probably is solution of the corpuscles. The loss of albumen experienced by the blood in Bright's disease, would appear to be inversely proportioned to the degree in which it appears in the urine, and it may be in the dropsical effusion likewise. The quantity of fibrin in the blood is regulated in great part by that of the corpuscles, not by adjustment, but in virtue of the causal relation subsisting between the disintegration of the one and the formation of the other. Diminution, therefore of the quantity of globules in the blood will not necessarily cause elimination of the fibrin, because it involves diminished production of the latter; but the converse of the proposition will not hold, as diminished proportion of fibrin, by whatever cause produced, may give rise to elimination of the blood corpuscles in the form of hæmorrhage. *Dublin Hospital Gazette, October* 15, 1857.

ART. III.—*On Icterus Typhoides:* By DR. LEBERT, M. D., of Zurich. Translated from the German of R. VIRCHOW's *Archiv für Pathologische Anatomie und Physiologie,* (*Vol. VII, p.* 343): By M. SCHUPPERT, M. D., for the *N. O. Medical and Surgical Journal,* by request of B. Dowler, M. D.

IDIOPATHIC jaundice is generally considered a mild and an almost insignificant disease; nevertheless, every experienced physician will probably have seen cases which, though from the beginning of a mild character, and without being in any way complicated with a severe affection of the liver, take a bad turn, and sometimes even resulting fatally. That cases of this kind are not of rare occurrence, will be proven by the fact that I have been able to collect no less than seventy-two of this character. Yet all classical text-books are either silent upon this malignant species of jaundice, or mention it in a very unsatisfactory manner. The few monographical essays which we possess on the subject are either not sufficiently known, or they are defective in themselves.

The study of the more dangerous form of jaundice offers a manifold interest; it affords an accurate description of a not much known and dangerous disease, an estimation of its nature and of the therapeutical resources we possess against it, showing finally, its relation to other similar diseases, and particularly to the group of typhoid diseases—the extent and importance of which (group) increases daily.

I will, first, give a short historical survey of the essays on this subject and explain afterwards, according to an analysis of documents, the pathological anatomy, the general and special symptomotology, the duration, course, prognosis, and the treatment of the same. In enumerating a series of cases, I will be enabled to express at the end of this treatise, my views of the essential nature of this disease.

1. *History and Literature.*—The first observation of this disease we know of, is made by Franciscus Rubens. A man of twenty-two years of age, laboring under a common icterus, who was attacked on the fourth day of his illness by violent cerebral symptoms, which first showed the character of an intensive delirium, but then passed over into coma, whereupon death ensued on the fifth day. I have not been able to read this factum in the original; I cite it from Oza-nam. Bonnet mentions in his *Sepulchretum* several cases, one of which is that of a young man of fourteen years, who, according to an observatiun of Ballonius, was attacked on the fifteenth day with also a simple icterus, having had cerebral symptoms, which proved fatal in a short time; though at the post mortem examination, the brain did not show any abnormal conditions. The liver, however, was peculiarly discolored. Another case of which he reports, according to *Guarinonius*, that of the cardinal Sforza, who died on the 16th day after icterus had appeared, with violent convulsions, from which he had suffered for the last three days. The liver was yellow, the blood throughout the body black and fluid. We find further in the *Sepulchretum*, the remarkable observation made by Kerkring, who reports that a woman in the eighth month of pregnancy, was delivered of a dead fœtus, totally icteric, and whose blood and bones even had become of a yellow color. Morgagni reports also several very interesting cases: one of a child five months old which died of jaundice accompanied with convulsions; another of a priest, who after a violent emotion turned icteric, and died on the fourth day after intensive cerebral symptoms, delirium, convulsions and coma had set in. His liver was flabby and pale;

and the stomach contained a blackish fluid. This and the following case are taken from *Valsalva*. This latter case refers to a young man of Letters, who, in consequence of a violent emotion, brought on by having been attacked by robbers, was affected with jaundice; he suffered afterwards by violent delirium and convulsions, and died twenty-four hours after the beginning of the delirium. VAN SWIETEN, in his Commentaries on *Boërhave's* Aphorisms (vide Villeneuve Dictionaire des Sciences Médicales) mentions also two cases of icterus, which were caused by violent emotions, and proved quickly fatal under cerebral symptoms.

Towards the end of the preceding and at the beginning of the present century, several authors, among whom we cite Bruning, Kerksig, Lange, and Mende, have described epidemics of jaundice, of which some malignant and fatal cases are mentioned more particularly.

Of a disastrous character appears to have been the epidemic which reigned in the year 1772, in the town of Essen, and of which *Peter Frank* says, that it proved in many cases fatal to children. Of the epidemic of 1794, described by *Kerksig*, our attention has been directed to the dangerous manner in which the jaundice affected the women already far gone in pregnancy. In those cases the life of both the mother and fœtus had always been much endangered, abortions having frequently occurred.

In the second decennium of the present century we find several very interesting observations, made especially by English physicians; by O'Brien and by Cheyne; and the contributions from Alison and Bright, of a still later period. We are made acquainted with another peculiarity of this disease by Griffin, Graves, and lately by Budd, showing the fact that several members of one and the same family were affected with icterus running a dangerous course, and proving sometimes fatal. These cases go to prove a local malignant cause of the disease.

Up to the latest times, several, and mostly very good observers, have compared the cases of malignant and typhoid icterus with yellow fever. Already in the year 1826, Damiron communicated such a case to the Medical Academy of Paris. A commission of three, Dalmas, Pariset, and Orfila were appointed, and the reporter, Dalmas, stated that sporadic cases of yellow fever did appear in Paris, and in France in general; and a few years later the same was shown by Paradis, in regard to the southern part of of France, and by Louis, in regard to Gibraltar, where even several years before

the famous epidemic of 1828, some sporadic cases of this kind appeared. Cases of similar kind have been described within the last years by Bondon, Dalamir, Carsenac, Siphnaios and Monneret; and, according to a report of Garnier-Leteurrie, it seems such cases have been epidemical among the French army in Rome, in the year 1849. Although according to our opinion this analogy has been much exaggerated, yet in some of the cases a certain similarity cannot be denied to exist between the two diseases. The series of diseases belonging to this category is rendered more complete by Griesinger's beautiful investigations of the nature of the bilious typhoid of Egypt. In the important progress of our knowledge of the severer forms of icterus, we number the treatise of Horaczek on yellow liver-atrophy, which appeared first in the year 1843. If we dissallow the important connection of this kind of liver-atrophy with icterus typhoides, still we are not inclined to recognize the yellow liver-atrophy as the cause of the disease, as this has been done by Horaczek, whose opinion many German physicians have adopted. In the monograph of *Budd* which appeared first in 1845, and in a second edition in 1852, very interesting observations have been collected. The author does not explain his doctrine about the form of icterus, but mentions among other interesting points, specially the modification and partial destruction of the liver-cells; yet even these alterations do not appear to us to be the *ultimate cause* of the disease. Finally, we mention an inaugural dissertation on the malignant forms of icterus, by Ozanam, (Paris, 1849) who made use not only of the existing material, but offers besides this several interesting new observations, yet this dissertation, though written on good and general principles, can not be looked upon but as a contribution to the history of icterus typhoides.

(*To be Continued.*)

Art. IV.—*Pharmacy:*

On a new mode of preparing Mercurial Ointment. By M. Coldefier, Chemist at Geneva.—The process I am about to describe is the result of a discovery due solely to chance. In the course of one of my investigations on ozone, on approaching my apparatus with a candle in my hand, in order the better to observe the progress of the experiment, some drops of tallow fell into a capsule containing mercury, heated by the current of a voltaic pile. I was annoyed at my awkwardness, but what was my astonishment, when I saw my globule of tallow assume a gyratory move-

ment, and becoming grey colored; the rapidity of the motion increased with the coloration. I intercepted the current and the globule stopped; I took it up cautiously with a little analysis spoon, and satisfied myself, after it cooled, that I had a small quantity of perfectly prepared mercurial ointment, in which it was impossible to perceive, even with the aid of a double achromatic lens, the most minute metallic globule.

It naturally occurred to me that this phenomenon could be produced only by the presence of the ozone, by which the mercury was surrounded. In fact, the tallow having become ozonised in the atmosphere of this gas, absorbed the mercury by means of a purely mechanical action, that is to say, by the friction of the adipose spheroid upon the metal.

In a word, omitting all description of my numerous experiments on the subject, I may state that I eventually devised the following formulæ, the result of which is as certain as the process is easy of execution :

Put into a large porcelain capsule sixteen ounces of lard perforated with holes, so as to increase the extant of surface, and place half an ounce of phosphorus in a vessel suspended on a thread above the lard; cover the whole with a glass receiver, and at the end of a fortnight ozonization is complete. This lard, so prepared, is introduced into a wide-mouthed bottle, and melted on a sand-bath, at a temperature of 194° F. Four ounces of mercury are now gently heated, and rapidly poured into the lard; the vessel is then briskly agitated for some minutes, and the operation is terminated by quickly plunging the bottle into a vessel of cold water.

Codliver Oil and Carb. Sod. as a local application in Eczema of the Face in Children.—Dr. Behrend, in a note on the treatment of eczema, recommends the employment of the following combination as a remedy for the numerous scales which frequently cover the face of children: Cod liver oil, half an ounce; carbonate of soda, half a drachm—mix.

Adulteration of Sulphate of Quina with Sulphate of Aricine.—M. Ascoop has just met a specimen of sulphate of quina adulterated with sulphate of aricine. This fraud is the more difficult to detect (several pharmaciens had passed the specimen alluded to as good), inasmuch as when examined by Liebig's process, recommended in the *Nouvelle Pharmacopée*, the sulphate in question presents the character of a genuine product, aricine being soluble in sulphuric ether as well as quina. It is only by evaporating the ethereal solution, and heating the dry residue with *concentrated* nitric acid, that the falsification can be discovered.—*Dublin Hosp. Gaz., of Dec.*, 1857. *From Journal de Méd. de Bruxelles.*

Valerianate of Ammonia.—Pierlot's formula for the original solution of this salt, prescribed by Dèclat, is distilled water f. ℥iv, valerianic acid a drachm, carb. ammonia q. s., neutralize and add ℥ij, alcoholic extract of valerian.—*Amer. Jour. Pharm.*, July, 1857.

Ter-iodide of Bismuth.—Schneider gives a process for making this compound in the dry way by heating 1 eq. ter-sulphuret of bismuth and 3 eqs. of iodine intimately mixed, in a loosely covered flask. The iodide of bismuth sublimes and the sulphur passes off chiefly as sulphurous acid. —*Chem. Gaz.*, March 16, 1857, p. 109.

Iodide of Antimony.—Mr. Copney has recently published a formula for this iodide (*Pharm. Jour.*, Feb., 1857, and *Amer. Jour. Pharm.*, May, 1857,) which is made by fusing 1 eq. metallic antimony and 3 eq. of iodide, intimately mixed, as in making iodide of arsenic.

Artificial Glycerin.—M. A. Wurtz has succeeded in the synthesis of glycerin by operating on triacetine with baryta. The resulting substance has the composition and properties of glycerin, and, like that body, yields iodized propylene when treated with iodide of phosphorus.—*Amer. Jour. Pharm.*, Sept., 1857, and *Comp. Rendus*, April, 1857.

Mannite and Glycerin convertible into Sugar.—M. Berthelot has succeeded in converting these bodies into fermentable sugar by acting on a solution of them with certain animal tissues.—*American Jour. Pharm.*, September, 1857.

Amygdalin.—Messrs. Kolliker and Müller have proved that amygdalin and emulsin meet in the circulation, when amygdalin is taken by the stomach, and emulsin injected into a vein, producing prussic acid, and consequent poisoning; but when the emulsin is ingested and the amygdalin injected, no reäction follows, as the former undergoes digestion and does not reach the circulation.—*Amer. Jour. Pharm.*, September, 1857, and *Brit. and For. Rev.*

Iodized Oil.—M. Hugounenq proposes a formula for making a solution of iodine in oil of almonds as a substitute for cod-liver oil.—*Amer. Jour. Pharm.*

Ursone.—A proximate principle obtained from uva ursi by Trommsdorf has been ascertained by Hlasiwetz to have the composition C^{20} H^{17} O^2.—*Chemist*, 149, October, 1856.

Alcoholic Fermentation.—M. Berthelot has presented some interesting views in regard to this process, and the substances capable of modification by it, to the French Academy. He groups all such substances together; which, commencing at glucose, becomes less and less readily fermentible till glycerin is reached.—*Chemist*, July, 1857, from *Comptes Rendus*, April 6, 1857.

The Active Principle in Indian Hemp Resin.—The Society of Pharmacy of Paris having for two years offered a prize of 1,000 francs for the best analysis of this substance, the prize was last winter adjudged to M. Personne, of Paris, whose essay was reported on by M. Robiquet, in the Journal de Pharmacie, Jan. 1857. Abandoning the idea heretofore adopted, that the active principle was a resin, as pointed out by T. & H. Smith, of Edinburgh, (*cannabin, haschishin,*) from the fact that when smoked, its peculiar effects were strongly manifested, he sought for a volatile principle, and by distilling the dried Indian hemp with water, obtained a volatile oil with an alkaline distilled water. The alkali proved to be ammonia. The volatile oil was lighter than water, had an amber color and the characteristic odor of the hemp. Exposed to cold it crystalized, and by fractional distillation yielded two distinct hydrocarbons; one, *cannabein* liquid, is C^{36} H^{20}; the other, in crystalline scales, is

$C^{12}\ H^{11}$. *Cannabein* exhibits the physiological effects of hemp resin, but its effects are less lasting.—M. Robiquet in *Jour. de Pharm.* Jan. 1857.

Tinctura Ferri Chloridi.—Dr. E. R. Squibb, U. S. N., has proposed a new formula for this preparation, which obviates all difficulty in reference to its strength and preservation. Protochloride of iron is formed, with two-thirds of the prescribed quantity of muriatic acid, and q. s. NO^5 to peroxidize the protoxide. This solution does not change or deposit.—*Amer. Jour. Pharm.*, July, 1857.

Liquor Ferri Nitratis; a New Process.—W. Procter, jr., has proposed a new process for this officinal solution, which renders it as permanent as the tincture of the chloride. It consists in making a proto-nitrate by saturating diluted muriatic acid with iron filings or tacks, (in large excess,) filtering and adding the requisite proportion of NO^5 to convert the proto-salt into sesquinitrate. The solution has a light wine color, and has no tendency to change by keeping.—*Ibid.*

Action of Alkaline Chlorides on Calomel.—Dr. Andrew Fleming, of Pittsburgh, Pennsylvania, has carefully investigated this subject, and corroborates the views of Mialhe in reference to it, viz: that they do form corrosive sublimate by contact in presence of water; and he has ascertained that the amount of bichloride of mercury found, is proportional to that of the alkaline chloride present.

Iodized Glycerin.—Dr. Richter, of Vienna, proposes a solution of one part of iodide of potassium in two parts of glycerin, and then dissolving in this one part of iodine, as an external iodine caustic.—*Ibid.*

Proto-Sulphuret of Carbon.—M. E. Baudrimont has discovered this hitherto unknown compound of sulphur and carbon. It is a gaseous body, colorless, odor resembling CS^2, and is powerfully anæsthetic. Its gaseous condition is the cause of its having escaped detection before. It is easily prepared, and is an interesting substance to the chemist.—*Chemist and Am. Jour. of Pharm.*

Prescriptions.—The following resolution, offered by Prof. Procter, of Philadelphia, was adopted by the *American* Pharmaceutical Association:

Whereas, the dispensing of medicines on the extemporaneous prescription of physicians is the most important of the duties of the pharmaceutist, involving a large amount of ready knowledge and skill, and as the demand for this knowledge is very frequently required at a moment's notice, when delay might seriously retard the recovery of the patient and impair the usefulness of the physician, and as the feeling of responsibility, at all times great, becomes often oppressively burdensome through the careless manner in which prescriptions are frequently written, and their quantities symbolized, it is hereby

Resolved, That this Association, speaking for the pharmaceutical profession of the United States, do most respectfully and earnestly desire that the grave importance of this deficiency, and the many evils which arise out of it in the dispensing of medicines, will claim anew the action of the Medical Societies of the United States, with a view to influencing those of their members to whom the charge applies; and while fully

aware of the numerous imperfections which appertain to our own practices and practitioners, and which we are striving to overcome, we believe that the cooperation of physicians in the manner suggested will greatly aid our endeavors.

James' Powder; its Composition, etc.: By E. S. Wayne, Cincinnati, Ohio.—At one time, the preparation known as pulvis antimonialis, (the Pharmacopœia substitute for James' powder, a patent nostrum of great celebrity nearly a century ago) was used to a considerable extent, but at present is but little used from the fact that it has proven to be so very uncertain in its action.

The patent article is yet made by the heirs or descendants of Dr. James, in London, and is I believe, much used by English physicians, and considered by them to be a much more reliable remedy than its substitute.

Recently a quantity of the English made James' powder has been brought to this city, and has been used by a number of our physicians, who speak highly of its merits. Their statement of its superiority, has induced me to make some examination into the composition of James' powder, and what the difference, if any, between it and the pulvis antimonialis of the Pharmacopœia. I am aware that James' powder has been examined by chemists, and no two of those who have exaamined it, have agreed as to the true composition of it; they all agree thus far, that it is an oxide of antimony with phosphate of lime, but what particular oxide, they have left us in my opinion quite in the dark. That there exists in the analyses made by Drs. Pearson, Brandt, Berzelius, and Chenevix of this powder, discrepancies, is not surprising, as no compounds present so much difficulty to the chemist in their analysis, as the complex oxide of antimony, and it is only within a very recent date, that the true composition of the various oxides of antimony, acid and basic, have been made known.

The preparation of the pharmacopœia, is based upon the supposition, that the antimony in James' powder, is in the condition of antimonious acid, Sb. O4, (modern chemistry repudiates the existence of an oxide of this composition, it should be an antimoniate of the teroxide Sb.O3 + Sb. O5), this difference in chemical fact, however, is not material, as far as the point in question is concerned.

I think that I can without the trouble of an analysis of this James' powder, and with our present knowledge alone of antimony and its oxides, prove that the preparation of the pharmacopœia has little or no resemblance to it in its composition.

Dr. James, in his record filed in chancery, says, "that he takes antimony, and calcines it with a protracted heat in a flat earthen vessel, and adds to it from time to time, animal oil and salt, then boils it in melted nitre for a considerable time, and afterwards separates the powder from the nitre, by dissolving it in water." Now from this description, it is evident that in the first process, the antimony becomes oxidized at the expense of the air, and the oxide thus formed combines with the chlorine of the salt more or less, forming

probably a portion of ter chloride Sb. Cl3, which volatalizes by the heat, and an oxichloride Sb. Cl3 + 2 Sb. O3. This oxichloride and oxide, upon being boiled with an excess of nitre is decomposed, forming a neutral antimoniate of potassa, which is left as an insoluble powder, upon the fused mass being washed with water.

Neutral antimoniate of potass, can only be formed by the process he describes, and is undoubtedly the base of the so-called James' powder. It can, however, be made with much less trouble, than by the process he describes. The phosphate of lime in the powder is added after the preparation of the antimoniate. The addition of the phosphate of lime is not specified in the original recipe by Dr. James; it thus appears that he only gave a part of his compound, or the method of making it. Nor is this preparation original with him. I have an old Pharmacopœia printed nearly a century before he filed his recipe, that has in it precisely the same formula, and for it is claimed the same virtues—which preparation, in the lapse of time, seems to have been overlooked, and revised again by Dr. James, who was shrewd enough to involve the preparation in mystery, to his individual gain. Whether this preparation has any merit over the other oxides of antimony, or the pulvis antimonialis, I am not able to say. Pereira states that the teroxide of antimony is the only reliable one, and recommends the use of it, (as tartar emetic,) wherever antimony is indicated. But I have no doubt, many who have been in the habit of using James' powder, would not admit that they could derive the same effect from the administration of tartar emetic. Such is the opinion of those who have used the article I have alluded to in this city.

I have, based upon the conclusions given above, made a preparation of compound of antimoniates of potassa, with phosphate of lime in equal proportions, which has been placed in the hands of physicians for trial, and hope at an early date to give their report of its merits.—*Cincinnati Lancet and Observer*, April, 1858.

ART. V.—*Vaginal Thrombi.*

CASE 1.—I was called fifteen miles in the country to see Mrs. B., who, since her delivery, two weeks before, had suffered from a tumor in the right labium pudendum. Her labor had been quick and easy. Immediately after the birth of the child, she "felt a stinging pain," which was followed by a tumor in the part as large as a hen's egg; this subsequently became still larger. It caused much pain, and, also, difficulty in urinating.

Having ascertained from a careful examination, that the tumor was not owing to vaginal hernia, (several examples of which, have occurred in my practice,) I made a free incision from which several ounces of semi-coagulated blood were discharged, with some apparent force. The remaining clots were removed and the sack completely emptied.

A poultice made with the crumb of bread and a concentrated ooze of oak-bark, supported by a T bandage, was directed. The cure was rapid. B. Dowler.

Case 2.—At a meeting of the St. Louis Medical Society, Dr. Boisliniere reported a case of *thrombus of the vagina:* The patient, a primipara, of leuco-phlegmatic habits, with a normal pelvis, after an ordinary labor, ending with rather severe expulsive pains in the birth of a living child, was left comfortable, when, an hour and a half after, he was called to see her in haste. He found her in the greatest suffering — crying loudly, and assuring those around her that this was ten times more severe than her labor pains. She complained then of a burning sensation in the vulva, and a severe bearing down in the rectum, which gave her a pressing inclination to go to stool. She suggested that it was piles. This pain was so agonizing that she had no words to express it. In fact, she could hardly utter two words in succession. This appeared to him the more strange from the great fortitude she had shown during her labor. Dr. Boisliniere confessed that he fell here into the same error committed by Dr. McBride in a similar case, and that he took this at first to be one of those violent tenesmic pains that are experienced sometimes after labor, when the child's head has quickly and violently swept over the vagino-rectal septum—a sort of severe spasm — and he ordered opium suppositories, and hot fomentations to the parts; the latter, as proved on trial, only aggravated the evil; the pain continued to increase instead of abating. He made then a vaginal examination, which revealed to him the true nature of the complaint—it was a very large thrombus, beginning on the left side, ascending along side the pelvis as high up as the finger could reach, filling the whole of the vagina, so that the finger could with difficulty only be introduced, and descending as low down as the left labium, where it was characterized by great hardness and elasticity. This tumor was rapidly enlarging—he could ascertain this by keeping the end of his finger on its surface—the patient was in the greatest agony, her pulse was getting quick, her countenance anxious and distressed. There was only one course to follow—Dr. Boisliniere, under these circumstances, availing himself of the great experience of Dr. M. M. Pallen in matters of this sort, sent for him, and it was decided to open the tumor largely at once. Dr. Pallen then incised the tumor, which was projecting and bluish —his incision extended to three inches in length, and reached the cavity of this thrombus, after which he emptied it of the coagula it

contained, except the most adherent at the bottom for fear of inducing hemorrhage, and a sponge was introduced into the vagina to act as a tampon. The patient was immediately relieved, and recovered well. The amount of clotted blood extracted must have been several ounces.

Dr. Pallen said that the case related by Dr. Boisliniere was a very interesting one; interesting on account of its rareness, and on account of the fatality often attending them. He believed that the first published case in British midwifery, was that of Dr. McBride, which occurred in 1776. It had, however, been observed before. Since then, it has been noticed and described by various writers. Dr. Denman saw three cases, and all recovering, he was induced to think that the disease was devoid of danger; subsequent experience has, however, shown that he was mistaken — death has frequently occurred.

This disease, which consists of an effusion of blood into the areolar tissue, may be in one or both labia, may extend into the pelvis and into the perineum. It doubtless sometimes occurs in the unimpregnated female; but it is usually an accompaniment of labor, appearing either during its progress, or soon after. The swelling of the labia, and the feeling of weight are succeeded by great pain. If the distension be very great, the pain is agonizing—fever of an active kind ensues, and even delirium. The patient can only lie on her back, with her thighs widely separated. The tumor is of a livid color, almost black, and is extremely tender.

Dr. Pallen added, that there could be no doubt that the effusion arose from the rupture of some vessel. In a state of pregnancy, the vessels are in a varicose condition, and their rupture easily produces a large discharge of blood. What particular blood-vessel is ruptured, is not determined—probably the pudic vein.

If the tumor, said Dr. P., occurs before delivery, and increases slowly, and does not interfere with delivery, it might be trusted to nature. If it increase rapidly and interfere with labor, it ought to be laid open, and the coagula turned out. The same rules apply to the treatment, if it occur after delivery. If it be of slow formation, and not large, the application of cold cloths is all that is necessary; but if it form rapidly, and the pain is great, then we ought to cut it open, turn out the coagula, leaving those which are entangled in the areolar tissue, as they will tend to prevent further bleeding. This being done, the tampon should be used, to produce pressure on the blood-vessels.

Dr. Boisliniere said, that a remarkable circumstance attending this case was the rapid formation of the thrombus one hour and a half after the birth of the child. This, said he, could only be explained on the supposition that this bloody tumor had begun to form before delivery, and that the head of the child had acted as a tampon, favoring the formation of coagula over the mouths of the bleeding vessels, thus arresting the hemorrhage for a time, and that these coagula having given way only an hour and a half after the

expulsion of the child, then the hemorrhage began again to fill the tumor. He would also add, that the view of Dr. Pallen, that these hemorrhages depended upon a varicose condition of the veins of the pelvis, is confirmed by the fact established by statistics, that thrombus may occur indifferently after severe and protracted labors, or instrumental labors, as well as after easy and short labors, and that the occurrence of these tumors arose mostly from the general varicose diathesis which characterizes the state of utero-gestation, especially with certain women of lymphatic temperament. Hence, the fact that as this state of varicosity is more frequent with women who had several children, so should cases of thrombus be mostly found among these.

Dr. Pallen remarked also, that a careless observation might cause a thrombus to be mistaken for a vaginal hernia, but surgeons had established rules sufficiently explicit to guard against this error.

Dr. Gregory said that the diagnosis of tumors about the vagina was sometimes surrounded with some difficulties; he had himself seen a case diversely diagnosticated as a vaginal hernia and an encysted tumor, which proved to be the former. These thrombi might at first sight be confounded with the different tumors which make their appearance in the vagina, whether their point of departure be from the cellular investments of the interior of this canal, or from some morbid enlargement of the ovaries, or even a vesical calculus. Vaginal rectoceles or cystoceles might also be mistaken for a thrombus. However, the history of the case will be here of the greatest assistance to enlighten the diagnosis. Moreover, the form, the consistency, the situation of this tumor, its sensibility to pressure, the suddenness and rapidity of its formation, must be taken into consideration, as well as its color when this can be appreciated. To distinguish this from a vesical calculus or a vaginal cystocele, it must be remembered that a thrombus has very seldom its seat at the anterior portion of the vagina. The facility of penetrating with the finger into the sac of a vaginal rectocele, will easily enable one to eliminate this from the diagnosis.—*St. Louis Med. and Surg. Journal, Sept.*, 1857.

Art. VI.—*Vaccination.*

The January number of *The Glasgow Medical Journal* contains an analysis of sundry papers on vacination, (laid before the Board of Health and both Houses of Parliament by command of her Majesty,) from which the following extracts are taken :

Fully cognizant of the value of vaccination, and the difficulties

which stand in the way of its general adoption by all classes of a community, when it is left to their own free-will and discretion, several of the European states very soon made it obligatory and compulsory. Since 1809, vaccination has been general in the grand duchy of Baden. In 1810, Denmark promulgated a law enforcing it. In 1811, Wurtemberg, Sweden, and Norway followed in the same track, compelling every child to be vaccinated before the age of two years. Prussia and Bavaria, and several other states, have also experienced the benefit of such legislation, and proved, by the very low mortality from small-pox in these countries, the wisdom and policy of so benevolent a measure. In our free and glorious country, up till 1853, every man could do what was right in his own eyes. To compel one to vaccinate his children was an infringement on the boasted liberty of the subject; and even then, when vaccination became obligatory, care was taken that there should be no one to enforce the law, and the fine or penalty for its infraction became an empty threat. Accordingly, we find it stated in the memorial presented in 1855 to the Board of Health, by the Epidemiological Society of London—

> Among them (diseases preventible by public measures) it will be found that small-pox is still one of the most fatal, destroying on an average in London alone, nearly 1000 persons every year; and in England and Wales, little short of 8000; whilst in particular years of epidemic aggravation the mortality is still more formidable. And again: While out of 1000 deaths from all causes, there are in England and Wales, 21 from small-pox, and in some parts of Ireland upwards of 50, in Bohemia, Sweden, and some of the Italian states there are not more than two.

Further, from Dr. Seaton's notes on the present small-pox mortality of Scotland in 1856, we learn that the deaths form that disease in eight principal towns constituted 2·8 per cent. of the total mortality, which is double the average of London for the last ten years, or of England and Wales for the last seven. In these towns the proportion in each was very different. In Glasgow, the percentage of deaths from all causes was 1·25; in Leith, 3·2; in Edinburgh, 3·21; in Aberdeen, 3·91; in Paisley, 4·64; while in Dundee it was no less than 9·33. The cause of this high mortality is the neglect of vaccination.

Before the introduction of vaccination, natural small-pox exterminated whole races of men, as in Brazil in 1563. An interesting account is given in the communication from Denmark, of the various epidemics in Iceland, where the last, which occurred in 1707, and was introduced in some wearing apparel belonging to one who had died of that disease on the passage from Denmark, carried off 18,000 out of a population of 50,000. In very recent times, the North American Indians have suffered severely from its ravages; villages being swept away, and none left to bury the dead; whole tribes being almost exterminated. Not less than 60,000 Indians are computed to have been carried off by the epidemic which broke out among them in 1837.

In London, when not at its worst, its mortality averaged a fourteenth of the annual total of deaths. * * * * *

Important statistics concerning the protective powers of vaccination have been obtained from foreign governments by the Epidemiological Society of London:

Compare, for instance, in the case of Sweden, the twenty-eight years before vaccination, with the forty years soon afterwards. During the earlier period, there used to die of small-pox, out of each *million* of the Swedish population, 2050 victims annually; during the later period, out of each million of population, the small-pox deaths have annually averaged 158.

Or compare two periods in Westphalia. During the years 1776–80, the small-pox death-rate was 2643; during the thirty-five years 1816–50 it was only 114.

Or taking together the three lines which belong to Bohemia, Moravia and Austrian Silesia, you find that when formerly (1777–1806) there died 4000, there now die 200.

Or taking two metropolitan cities. You find that in Copenhagen, for the half century 1751–1800, the small-pox death-rate was 3128; but for the next half century only 287; and still later, in Berlin, where, for twenty-four years preceding the general use of vaccination, the small-pox death-rate had been 3422 for forty years, subsequently it has been only 176.

In other words, the fatality of small-pox in Copenhagen is but an eleventh of what it was; in Sweden, little over a thirteenth; in Berlin and large parts of Austria, but a twentieth; in Westphalia, but a twenty-fifth.

From such information as exists, it seems probable that the small-pox death-rate of London within the bills of mortality, during the eighteenth century, ranged from 3000 to 5000. During the ten years 1846–55, it was under 340.

According to Mr. Marson, the fatality of small-pox, when it attacks the unvaccinated, is 350 per thousand; that its fatality to such vaccinated persons as it infects, is, taking them indiscriminately, 70 per thousand; but distinguishing vaccinated persons into two classes—those (1) who have been vaccinated in the best known manner, and those (2) who have been badly vaccinated—the fatality of small-pox, if it infects the former, will be 5 per thousand; if it infects the latter, 150 per thousand; that the risk of the one will be thirty times the risk of the other.

Post-Vaccinal Small-Pox.—Two causes have been assigned as the reason of this re-susceptibility; either (1) that the protection afforded us wears out and loses its influence in the course of time, or (2) that the vaccine matter has degenerated in the course of transmission from man to man. To obviate this first cause, re-vaccination was proposed as a preventive of this re-susceptibility to small-pox. The first and earliest experiments on the large scale were performed in the army of Wirtemberg. From 1831½–5½, 14,384, chiefly between 20 and 30 years of age, were re-vaccinated; and of these 340·2 per 1000 were perfectly susceptible; the same kind of vesicle was produced which would have appeared had the operation been performed for the first time. Of 7845 who presented normal cicatrices of vaccination, the operation was successful in 310.4 per thousand; "modified" in 280.5 per thousand, and "failed" in 409.2 per thousand; of 3545 with defective cicatrices, re-vaccination was perfect in 280.7 per thousand, modified in 259, and failed in 460.4; of 2503 with no cicatrices, it was successful in 337.3 per thousand, modified in 191.1, and failed in 471.6; of 266 who had marks of small-pox, it was perfect in 319.5 per thousand, modified in 248.1, and failed in 432.3.

The National Vaccine Board, in their report for 1854, state that:

"The vaccine lymph does not lose any of its prophylactic power by a continued transit through successive subjects, and that it is a fallacy to predicate the necessity of resorting to the original source of the cow for a renewed supply."

The contrary opinion is maintained by several eminent men, as well in Europe as in Britain, that the vaccine lymph does inevitably become deteriorated; while others believe that this is only a contingent danger.

In 1818, M. Brisset alleged that there was a marked difference in the character of the vaccine vesicle; and in 1824 Dr. Meyer, of Krentzburg, stated that out of 4000 cases, the older scars of vaccination were better marked than recent ones, and that those produced by regenerated lymph exhibited the marks of the old normal type. From the experiments instituted by M. Bousquet, Professor Hering, Mr. Estlin, M. Fiard, and Dr. Steinbrenner, Mr. Simon thinks it "proved beyond the possibility of reasonable doubt, that certain original properties of the vaccine contagion have very generally declined after its long successive descent from the cow;" and "that the infective power of the newly humanized lymph is greater, and produces a more certain change;" and that "lymph of short descent has been more successful in disinfecting the body of that ingredient which constitutes its susceptibility to small-pox." This successive deterioration in vaccine lymph is well brought out by the reports of re-vaccination in the Prussian army. These extend back twenty-four years; and as from 40,000 to 45,000 operations are performed annually, they present a total of nearly a million experiments:

"When, in 1833, this system of re-vaccination commenced, the proportion of successful results (including cases in which the success came only with a second attempt) was 33 in every 100 vaccinations. Now the annual per centages of successful results for the whole time during which re-vaccination has been practised in the Prussian army beginning with that number, run thus:—33, 39, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 64, 69, 69, 69, 69, 70. *The last proportion of success exceeds the double of that with which the series commenced.* Thirty-three per cent. expresses the proportion in which persons vaccinated, say twenty years previously, had, in 1833, to a certain extent lost the influence of their infantine vaccination: it measures the impermanence of certain impressions produced by the vaccinations of 1813. And that impermanence (such as it was) in the effects of vaccination, has increased almost without exception, year by year, during this quarter of a century; *so that the vaccinations of* 1836—tested by eventual re-susceptibility to cow-pox—*were not half so stable as the vaccinations of* 1813."

What evils have been shown to attend its practice and to counterbalance its alleged advantages?

M. Carnot's notion was, that vaccination had given us typhoid fever and scrofula, in substitution for small-pox. But vaccination has no more to do with these diseases than with any other casualty which may befall human life. * * * Vaccine lymph, taken from patients with small-pox, conveys only the vaccine infection, pure and isolated. In like manner, it has been shown that scrofula and cutaneous eruptions are incommunicable. The experiments of Pro-

fessor Sigmund and Dr. Friedinger have proved "that syphilis, in its inoculable form, prevents within the sphere of its infection, the simultaneous formation of a vaccine vesicle;" in other words, the discharge of chancre, mixed with vaccine lymph, produces only syphilitic infection.

To elucidate this subject more fully, a circular letter was addressed to members of the medical profession, the departments of the public service, and to foreign governments. Replies to four questions, vitally affecting our future legislation on this subject, were requested. Five hundred and thirty-nine members of the profession, men of high repute and standing in this country, and on the Continent, responded to the invitation. From Austria we have the opinion—1. Of the Imperial Society of Surgeons at Vienna; 2. The Faculty of Medicine, at Prague; 3. The Imperial General Hospital of Vienna; 4. The Imperial Lying-in and Foundling Hospital. Baden, Bavaria, Denmark, and the Duchies of Holstein and Lauenburg, have favored us with their experience. Equally satisfactory answers have been forwarded from the governments of France, Portugal, Prussia, Sweden, Norway, and Wirtemberg. The valuable mass of information contained in these replies is published at length in Appendix J and K. We will examine them seriatim.

1. The first question was—"Have you any doubt that successful vaccination confers on persons subject to its influence a very large exemption from attacks of small-pox, and almost absolute security against death by that disease?" Throughout the whole series of 539 respondents, there are but two whose opinion is negative. No. 508 "distrusts vaccination, and would gladly inoculate his own children with small-pox." No. 219 "regards both proceedings with equal disfavor, and considers them alike to be at best but harmless trifling. All the governments who enforce vaccination by legislation, of course, answer affirmatively.

2. "Have you any reason to believe or suspect that vaccinated persons, in being rendered less susceptible of small-pox, become more susceptible of any other infective disease, or of phthisis; or that their health is in any other way disadvantageously affected?" All the replying governments who have made vaccination compulsory have discovered no drawbacks to its advantage; while, of the 539 respondents, not one has seen anything to excite such suspicion.

3. "Have you any reason to believe or suspect that lymph from a true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection, to the vaccinated person; or that unintentional inoculation with some other disease, instead of the proposed vaccination, has occurred in the hands of a duly educated medical practitioner?" In reply to the third question, by far the large majority, all men of talent, skill and experience in their profession, never had any reason to believe or suspect that lymph from a true Jennerian vesicle, ever produced, or conveyed to another person, syphilitic, scrofulous, or other infection. With regard to the exceptions, only two have reason to believe, or think it proba-

ble, that lymph taken from a party with hereditary disease, conveys that disease with it. Two have seen several instances of the transference of the syphilic taint, the lymph being taken from a supposed true vesicle in a tainted infant; while other two have seen cases of syphilitic disease following vaccination. (This last, however, is no reply to the question.) Another has suspicion of having seen a case. Three others are convinced, believe, and have no doubt of such conveyance being possible; while Professor Alison has known such affections to have been apparently communicated by attempted vaccination, or by any other incision of the skin; but such cases are not to be regarded as instances of vaccination. Mr. Acton unhesitatingly affirms, that he never witnessed a single case of syphilitic infection communicated in this manner, and points out *the fallacies* that might lead to such a supposition. Dr. Hebra, head of the small-pox division of the General Hospital, Vienna, and Professor on Diseases of the Skin, considers it proved, that by the experiments of Heim, Ricord, Taupin, Friedinger, etc., that both poisons are not simultaneously transmissible; while Mr. Paget can find no support for the suspicion, that the lymph of a well-formed vaccine vesicle can convey any other disease. With these opinions, that of the Imperial Society of Surgeons of Vienna coïncides, experiment having proved that vaccine lymph taken from syphilitic subjects, and used upon healthy persons, and *vice versa*, did not convey syphilis with the cow-pox.

With regard to the occurrence of cutaneous eruptions, out of 15 who have seen or known of such eruptions—vesicular, pustular, or erysipelatous—following vaccination, three have seen lymph from unhealthy subjects produce it; one has seen them occur in weakly children; two have seen them produced by healthy lymph, while the same lymph did not do so in other children. Hence one considers the constitution probably predisposed in such, while another considers such eruptions as sequelæ of no consequence; and several see no connection between them and vaccination.

4. "Do you (assuming due provisions to exist for a skilful performance of the operation) recommend that, except for special reasons in individual cases, vaccination should universally be performed at early periods of life?"

The importance of early vaccination is well shown by the tables published under the authority of the Registrar-General. "Twenty-five per cent. of the whole mortality of small-pox in England and Wales happens in infants less than one year old; and as much as eleven per cent. within the age of four months. Within the fifth year the proportion reaches the enormous amount of from seventy-five to eighty per cent."

Mr. Marson's tables show that the mortality "under five years is fifty per cent.; still greater, however, under two years; after twenty years it rises suddenly, and increases gradually; at thirty it exceeds the mortality of infancy; and after sixty hardly any escape."

Hence all the replying governments and all the respondents, with the exception of the "par nobile fratrum" (mentioned under question 1), recommend early vaccination unanimously.

ART. VII.—*Radical Cure of Reducible Inguinal Hernia.*

THE *Virginia Med. Jour.* (April, 1858) reproduces from a Lecture by Mr Wells, of London, thirteen pages of interesting matter concerning Wutzer's operation for the radical cure of inguinal hernia, a few extracts from which will be found below, the limited space of this Journal not permitting the admission of the entire paper.

I avail myself of the opportunity of advocating a method of treatment which, as it is proved by very ample experience to be both safe and successful, is worthy of general adoption in the treatment of one of the most common and most fatal diseases to which the human race is subject. Thirty-one operations have been performed by British surgeons, and there has not been a death or dangerous symptom in any one case. But more than this, the operation has been almost uniformly successful. Prof. Rothmund, of Munich, has done the operation about four-hundred times in the clinical hospital, and he told me that he had done it much more frequently in private practice, so that he felt convinced he must have operated a thousand times, and without one fatal result. This, I think, is more than almost any one could say of the most trifling operation. No one could expect to do a thousand operations for hare-lip—to tie a thousand nævi or piles—to remove a thousand small tumors—in fact, do the most trivial surgical operation a thousand times, without some untoward result following, accidentally, in some one case. So that this evidence is very strong indeed in favor of the almost perfect safety of the proceeding.

What Mr. W. says of the first and ancient method of radical cure by destroying or closing the sack and hernial apertures by incision, excision, setons, caustics, etc., will be omitted. Of the second plan he says :

The principal of the second plan is closure of the hernial canal by a portion of the skin, forming a sort of plug pushed into the canal, and made to adhere in its new situation. In other words, the inguinal canal must be closed by a portion of the scrotum pushed into it and fixed there. It can only be fixed there securely by adhesive inflammation of the serous coat of the sac occupying the canal, and it is necessary for the safety of the patient that the inflammation so excited should not extend to the peritoneum lining the abdominal cavity.

I will now show you Gerdy's operation on this subject. The invagination of the skin of the scrotum is the first stage of this operation, as it is in that of Wutzer, but Gerdy merely fastened the plug in its new position by a suture. You see I now form the plug, push the needle along my finger, through the canal and integuments, fasten the thread, withdraw the needle, then make a second puncture, cut off the thread and remove the needle. The result of this suture is just what you would expect—union of the opposed surfaces of the sac at one point. This slight union would appear likely to give way before very slight force, and this proved to be the case, for relapses were extremely common after operations were

performed in this manner. A more perfect closure of the canal than can be obtained by Gerdy's operation being evidently desirable, various plans were devised for effecting this closure. One of the simplest is that of Schuh of Vienna, who used the thread of Gerdy for drawing up and holding in the inguinal canal, as I now show you, a sort of wick of cotton of sufficient size to fill the canal. The pressure of this, however, is not sufficiently firm or uniform.

Here is an instrument contrived by Leroy d'Etiolles. It is a metallic cylinder and a cover, which can be so screwed together as to press the invaginated scrotum and abdominal parietes together. If this could be fastened tight enough to prevent its slipping out without injuring the compressed skin it might be a useful instrument, but it cannot. Here is another, something like the common American clothes-peg. It was contrived by Max Langenbeck, and has been used by him with success, but it was purposely left so long applied that sloughing of the compressed parts followed, and it was to the subsequent granulation that Langenbeck looked for closure of the ring. The process requires long confinement to bed, so that you see we want some much milder process than that. This Wutzer has afforded us. Here is his instrument made by his own maker in Bonn, the one I used in my early operations.

The cylinder, which is intended to take the place of the index finger after it has pushed a plug of scrotum through the ring into the canal, is made of very hard wood of different lengths and diameters according to the condition of the canal in each case. In its centre is a canal through which an elastic steel needle passes, and comes out on the upper surface near the point. Over this a concave cover, also of hardwood, is made rather wider than the cylinder, with an opening to admit the point of the needle, and another opening for a metallic staff, which rises from the cylinder near the handle, on which a screw works, by which the cylinder and cover can be screwed together. The handle of the needle can be removed by unscrewing.

After using these instruments of Wutzer's I soon found certain alterations desirable. The needle got rusty, caused undue irritation at the point of puncture, and became unfit for use again. This was soon obviated by electro-plating it. Then the necessity for varying the size of the cylinder in each case was troublesome and expensive. I obviated this, though imperfectly, by rolling strips of adhesive plaister round the cylinder until I made it as large as I desired. But this led to irregular pressure, and in Mr. Stretton's case caused a little sloughing, which, though it did no harm, might as well have been avoided. The cylinder, too, was too round for the normal shape of the canal. The point of the needle had to be guarded by a piece of cork, which would perversely be knocked off sometimes; and worse than all, there was nothing to prevent the needle itself from slipping back—the only thing keeping cylinder and cover in their place.

I had thought of different plans for correcting these faults, but

not of any thing so good as those for which we are indebted to Professor Rothmund. Here is his instrument. It is made, as you see, precisely on the same principle as that of Wutzer, and at first sight resembles it exactly in appearance, but the cylinder is oval, not round; the needle is not steel but silver, with a movable steel point, and there is a knob which can be screwed on after the point is removed, and at the handle there is a spring which is an effectual safeguard against the needle slipping from its place. Lastly, in order to admit of the same cylinder serving for the occlusion of canals of various dimensions, it is so made that side pieces of different sizes can be fitted on to the central portion of the cylinder. I have here covers of different sizes, and you see that by altering the side pieces I can make the cylinder quite as large as one needle could fix well. In cases of very large rings and canals it is necessary to have the central piece perforated for two, or even for three needles, but I shall use to-day this one with the single needle, as that which you would employ in the great majority of cases.

Now see what power this instrument gives us. We can alter the dimensions of the cylinder so that while perfectly filling the canal it may push the plug of the invaginated scrotum before it well up the internal ring, and even project some little distance into the abdomen. We can fix the plug much more securely and evenly than by a suture, or either of the other compressing instruments, and we have complete command over the degree of pressure exercised.

Now for the manner of using the instrument. I need hardly say that the patient's bowels should be opened a day or two before the operation, and the rectum cleared the same day by an enema of warm water; the bladder is emptied, and of course the hernia returned if it be down. This being done, you place the patient on his back, with the shoulders raised, the thighs semi-flexed, and the knees separated, just as if you are about to reduce a hernia by the taxis. You place yourself, as I do now, on the side you are about to operate on.

If you can use right and left hand equally well, you may use the left fore finger for invaginating on the left side, and the right to hold the instrument—the reverse for the right side. But the left finger will do for invaginating on either side very well. You see I operate on the left side—so I stand on the left side of the patient. I place my left forefinger here on the scrotum, about an inch below the external ring, and then push a fold of the scrotum before my finger with a little rotary movement slowly and steadily into the canal, keeping the palmar surface of the finger turned forwards and a little outwards, until it is well under the tendon of the external oblique, and the plug of scrotum is well pressed up to, or through, the internal ring. If you place one fore finger on the abdomen, just over the internal ring, while the other is in the canal beneath the tendon of the external oblique, you will feel the tendon very distinctly. As you move the finger backwards and forwards it

rolls over the tendon. You should accustom yourselves to feel this, because it is the test by which you know and can be certain that you are well in the canal, and when you have introduced the cylinder, that it is in the canal, and has not slipped anterior to it. When you are quite sure that your finger is in the canal, the next thing is to get the cylinder into the place of the finger, and then to fix it there by the needle. To do this you take the cylinder with the needle passed within it as far as you can without permitting the point to project, and hold it as I do now in the right hand with the thumb before the screw, the fore finger and second finger on either side of the prong which supports the cover, and the ring and little finger on the under side of the cylinder. Then bend the left fore finger a little, draw it forwards, and slip the cylinder along its dorsal aspect at the same time as the finger itself is being withdrawn. This is the most important step of the whole operation, and the only step which is at all difficult. Without care the plug may follow the finger. Without care the cylinder, instead of slipping beneath the tendon of the external oblique, may slip between it and the integument. You must be very careful that this does not occur, by feeling as you felt when your finger was in the canal, that the tendon rolls over the cylinder. If you do not feel this, and if the cylinder moves freely beneath the integument, you may be quite sure it is not in the canal, and you must begin again.

I wish particularly to guard you against this mistake, because I *know* it has been made at least once, and I *suspect* oftener. If you feel the tendon rolling over the cylinder you may be quite sure it is in the canal, and the end well up to the internal ring. To fix it there I have only to push on the needle till its point appears through the abdominal parietes, then to put on the cover, and use the horizontal and perpendicular screws until the cover and cylinder are evenly pressed together. Then the point of the needle is unscrewed, the knob put on, the handle of the needle removed, and all is done. You leave the patient with the instrument secured thus.

The whole affair does not take a minute, and as the only part of it at all painful is the simple puncture of the needle, it is quite unnecessary to give chloroform.

Then as to the degree of pressure you apply with the screw upon the cover—it should be very slight the first day to allow for a little swelling. The next day the screw may be tightened or loosened, according to the state of the skin. The patient should be kept in bed lying on his back, with the knees raised by a pillow, and the scrotum supported by a cushion or folded towel or two put between the legs. Every day the cover should be raised, to see whether the pressure is equal and not too great. If it does not press evenly a little cotton wool may be inserted, or if it press too much at any point this may be raised by a little cotton put near it.

About the fourth or fifth day you see a little inflammatory redness and swelling round the needle puncture and commencing suppuration. On the sixth or seventh some serous fluid begins to escape

round the end of the cylinder. This shows that the epidermis is separating. There is more purulent discharge from the puncture, and the instrument may then be taken away by withdrawing the needle and carefully removing the cylinder. You will find that the skin of the scrotum may be pulled tolerably hard without yielding at all, showing that pretty firm adhesion has taken place. If it appear to yield on pulling slightly it would be well to replace the instrument for two or three days longer.

The after treatment will vary a little, according to your desire to produce adhesion of the opposed surfaces of the plug to each other or not. Wutzer filled the cavity left by the withdrawal of the cylinder with charpie, dressed the puncture simply, and then put on a common bandage. Rothmund endeavors to obliterate the cavity. He keeps the patient in bed, raises the scrotum by a suspensory bandage, keeps off the pressure of the bed-clothes by a cradle, applies simple dressing to the puncture, and over this a graduated compress which is made to exert some pressure and keep the two sides of the cavity together by a common figure of eight bandage. The bandage is changed as often as it becomes dirtied by the secretions, and is carefully reäpplied. It requires from twelve to fifteen days to secure obliteration of the cavity and cicatrization of the puncture. As I said before, it is only Rothmund's great experience which would lead me to think much of the importance of this obliteration. I tried it in my second case, but no union took place, and in cases where union has taken place, the condition of parts a few months after the operation has been precisely similar to those in which Wutzer's practice was followed. Perhaps a middle course will eventually be followed, not filling the cavity with charpie, nor keeping the patient to the bed or sofa after cicatrization of the puncture, but keeping him quiet until this cicatrization is firm, and leaving the cavity to close if it will under the pressure of a compress and bandage, but not being sufficiently anxious about it to prolong the confinement of the patient. In either case the inguinal canal is filled by a firm plug, and for some weeks a sort of puckered depression is seen at its mouth, but this disappears after a few months, although the plug may still be felt. After five or six years the plug itself can scarcely be felt, and the most careful examination can hardly detect any thing abnormal. Rothmund convinced himself that this could not be explained by the sinking of the invaginated scrotal plug to its former position, by tattooing round the opening of the cavity, and observing that the marks did not change their position as they would have done had the plug descended.

If the pressure has been unequal, or the instrument left a day or two too long, there may be a little sloughing around the needle; but I have never seen a slough larger than a sixpence, and this only once. The only ill effect of it is some delay in the cicatrization. I believe if there is ever more sloughing than this, it must be from carelessness of the surgeon, or some unfortunate constitutional con-

dition of the patient, or the crowding and defective ventilation of some hospital ward.

Allowing six or seven days for the period the instrument is applied, and from seven to fourteen days more till cicatrization is complete, you should generally prepare a patient to expect a confinement of three weeks to his room After this he must wear for two or three months a very elastic truss, with a weak spring and a large well-stuffed pad. If the pressure is too great, or the pad too small, absorption of the plug may take place, and relapse of the hernia follow. If no truss be worn, the adhesions, which are still soft and yielding, might give way. It is also well to have a suspensory bandage worn as long as the truss, that the weight of the testicles may not drag down the skin of the scrotum to its former position.

You should also forbid violent exercise until the truss can be left off. After three months the truss may be left off, and I believe the patient is fully as secure against the occurrence of hernia on the side operated on as on the other side, perhaps more so ; in other words that he has not only been cured radically of his hernia, but he has been protected against its occurrence—he is less likely to become subject to hernia on that side, than a healthy man.

The commentary of the *Virginia Med. Jour.* on the general subject of hernia which follows is highly suggestive :

A safe, speedy and permanent method of relieving hernia, would undoubtedly be regarded as one of the greatest improvements in the province of surgery. It is not strange, therefore, that this subject is constantly occupying the attention of surgeons, and that novel suggestions and experiments are being frequently made, in the endeavor to gain this desirable result. Among the most recent and really practical articles on this subject, is the interesting lecture of Mr. Wells of London.

Amidst the many triumphs of modern surgery, we can but wonder that so little has been done towards the radical cure of hernia. According to careful statistics, not less than one in twenty of our population is afflicted with this misfortune.*

In the United States, then, we may assume the number to exceed *one million*, and we can imagine with what eager gladness this immense class of the community would resort to any mode of permanent relief, whose success had been sanctioned by time and experiment. Such a brilliant prize is indeed an inducement to the profession to think seriously and long upon any proposition which promises such a result. We therefore regard with great interest the modifications of the method of radical cure by invagination, first suggested by Gerdy, improved by Wutzer, and practised by the last named surgeon and Rothmund on the continent, and by Wells and Erichsen in England.

Sooner or later the successful cure of reducible hernia must be achieved. Its victims are numerous, the magnitude of the interests involved so large, and we will also add, the difficulties in the way too small, to make us doubt the fact. In an age when we do not hesitate to grope among the vitals for the largest artery and tie it successfully, or to extirpate enormous abdominal tumors, or overcome the most horrible deformities by the dexterous use of the anaplastic method, we have a right to assume that a cure for hernia will also be discov-

* Vide Malgaigne's Lectures, and the Observations of Astley Cooper and others. Va. Med. Journal, vol. iii, p. 42.

ered. Ask any one, not familiar with the history of surgery, which is the most difficult of execution, to obliterate a small opening in the abdominal walls, or to cut a stone from the bladder, and what answer would we expect? Yet the operation for stone is practised with increasing success every where, while we still offer no remedy for the simpler malady.

Again—hernia has been radically cured many times. We know precisely what the process is. We know by observing the results of the operation for strangulated hernia, that in a large proportion of cases the inflammation following upon the operation results in an obliteration of the opening.

It is also found that inflammation in the neck of the hernial sac, no matter how produced, will result in a radical cure. Thus, a truss, by long continued pressure on the inguinal ring and hernial sac, occasionally cures, and hence the attempt has been made to cause the desirable inflammation at this point by *injections into the sac*, an operation, which, we regret to say, has been too much neglected. If we do not fear to throw the iodine solution into the pleura or pericardium, into ovarian cysts, or even into the peritoneal cavity, why should there be any hesitation in using the remedy for the radical cure of hernia?

The whole subject is full of interest to the profession and to the public. The frequency of this disease among the negro population renders the question one of importance in a money point of view. Not less than *one hundred and fifty thousand negroes* are afflicted with hernia.

A successful remedy for this affection would add *twenty millions* to the present value of this species of property, besides greatly increasing its productive force. Are we wrong then in declaring that a successful and radical cure for hernia would be the highest achievement of surgery?

REVIEW.

Elements of Pathological Anatomy: By Samuel D. Gross, M. D.; Professor of Surgery in the Jefferson Medical College of Philadelphia; and formerly Professor of Pathological Anatomy in the Medical Department of the Cincinnati College. Third edition, modified and thoroughly revised; illustrated by 342 engravings on wood. Pp. 771, 8vo. Philadelphia: Blanchard & Lea. 1857.

Professor Gross, now a voluminous author, has in both word and deed exhibited a commendable zeal, ability, and success in advancing medical science in this Republic. Self-reliant, independent, and manly, he has advocated the necessity and has contributed to the achievement of a home medical literature having an inherent vitality of its own, not wholly dependent on the breath of foreign nostrils, for a precarious and sickly existence. American medicine has earned for itself an elevated standing worthy of comparison with that of the oldest nations. The latter more than the American

government, have stimulated scientific competition, skill, research, and discovery, by conferring rewards, honors and emoluments upon the cultivators of science. Which is the more manly—vanity, which bows itself to the dust to gain the blandishments of foreign eulogy, or patriotic pride which dares to claim for its own country a just and well-founded claim to scientific recognition in the family of nations?

The limited space remaining in this number of the Journal will not permit of a formal review of Dr. Gross' Pathological Anatomy—a most valuable work which has in a few years reached a third edition. Dr. Gross has been gratuitously assailed as hostile to foreign books as retarding the sale of his own. Now the true question in such a case is this: If valuable American books be not purchased and studied, should they not be thus honored? The merits of a book are in no degree dependent upon the rapidity of its sale, although if this test be adopted, this work must be (what foreign critics say it is) the best in the English language, seeing that it has passed through more editions than any similar work from the American press.

If a meritorious book does not sell well, the blame is not with the author but with the public. An author whose book had been unsuccessful from its profundity, avenged himself by writing a popular one so superficial and clear that every ass, as he affirmed, could see to the bottom of it without any difficulty. To write a profound and an able book is more meritorious than to write one which has no other claim than that of popularity and ready sale.

Without assuming that this book is devoid of all disputable postulates concerning inflammation, the local origin of all diseases whatsoever, etc., it may be affirmed that, as yet, this work as a whole, has not been surpassed by any similar one published in this country.

Morbid, like normal anatomy must be studied in the book of Nature rather than in the book of Art. Books, museums, and anatomical preparations, how useful soever they may be as auxiliaries, are not reliable substitutes for the human subject and its morbid alterations. Even pathological preparations rarely represent the true morbid anatomy of an organ or tissue. Color, cohesion, vascularity, etc., are more or less changed by alcohol and other means used to preserve specimens.

While books are insufficient to teach pathological anatomy, mere dissection, without a strict regard to the rules of medical logic, the rules for observing and classifying, appreciating and reasoning, will

prove equally inefficient. Facts, however numerous, and experiences however extensive, become valuable only in so far as they are correctly interpreted. Hence the importance of studying the possible sources of error, in order to avoid them in making observations and deductions.

The prevalent neglect and the imperfection of pathological anatomy, as a science, are chiefly owing to the absence of precise and universally recognized standards—standards chiefly of a physical character—for such must be the morbid character of the alterations revealed by the knife of the anatomist and by optical instruments, standards which, in the midst of complexity and multiplicity shall enable us to refer the numerous varieties of morbid alteration to a few, simple, and comprehensive principles or types common to the whole; nay it is probable there is such a unity in morbid alterations often laboriously described and classified as different, that it will be found in the progress of discovery, that the various developmental stages of morbid change are homogeneous, where, at present, they may seem different in nature. It is not expected that the morbid anatomist can, until further discovery, always give the criteria in the cadaver, especially in purely functional or dynamical diseases (if any such there be) seeing that life, function, force, statical force, (inertia, statics,) dynamical force (dynamics, motion) have not been isolated as materialistic, or spiritualistic entities.

A force is not a law. A law is but the expression of the behavior or rule by which force acts. If life be force or a dynamical condition of which functional disease is a perturbation and death an extinction, pathological anatomy is not, as yet, competent to elucidate its characteristics. Analogy, however, indicates, rather than distrusts the existence of morbid alterations, although the present means of research may be too imperfect to allow of their detection.

Sufficient is already known in pathological anatomy to authorize the inquirer to rely with the utmost confidence upon this method of explaining diseases, at least some diseases, while it justifies the expectation that greater advances are attainable by returning to it again and again as the starting point for new paths of research. Here the evidence is often so clear that a complete unanimity of opinion prevails, while symptoms, therapeutics, etc., may give rise to views of the most conflicting character.

B. Dowler.

MISCELLANEA.

1. *Hyrotherapeia: Treatment of Intermittents by Cold Water.*—The *Jour. de Méd. de Bordeaux* for March, 1858, quotes from the *Bulletin de Thérap.*, M. Dauvergne's testimony as altogether reliable for showing that the most obstinate intermittents yield to cold baths, especially of sea-water, the saline materials of which are supposed to contribute to the cure. He directs the patient to be treated from two to five minutes by the cold douche over the splenic region by means of the ordinary syringe, commencing one hour before the acces is expected; this treatment with frictions with water, is to be continued for eight or ten days after the disappearance of the paroxysms. He also recommends cold affusions for the anæmic condition which may persist as the effect of intermittent fever. He thinks cold affusions more efficacious in winter, being colder and producing more powerful reäction than in summer.

2. Dr. Schaller, (*Gaz. Méd. de Strasbourg*, March 22, 1858,) has published a paper on the *Grippe* or Influenza, which he considers a paludal catarrh (*une intoxication paludéenne*); a disease for which just as well as for intermittent, there is a specific remedy, namely, the sulphate of quinine. Dr. S. gave this remedy to twenty-seven patients, the mean duration of whose treatment was two days. This mode of treating epidemic catarrh he claims as a new as well as an efficacious one, giving nine illustrative cases. This disease, often witnessed and described during the last and present centuries, has swept with a storm-like celerity every sea, island and continent. It has prevailed on high and low, arid and paludal lands. With or without medication the patient is almost sure to recover soon or late. If the mean duration of the cure can be reduced to two days by the use of quinia, Dr. Schaller will be numbered among the gods. The disease has sometimes assumed the form of simple intermittents, tertians, quotidians and in such cases quinine could scarcely fail to be useful. Opiates, serpentaria, ammonia, camphor, cordials and tonics were formerly used in its treatment in some epidemics.

The treatment by quinia is not new. M. Ozanam says that, in the epidemic of 1742 this remedy was used in Venice: "*A Venise on employa le quinquinia.*" (*Hist. Méd.* i. 157.) It was at this period that the disease received the name of *Grippe*, in France. The same writer says that the epidemic of 1762 was treated with quinia and

rhubarb and other tonic laxatives. In London, he says, "*quinia donné a doses généreuses* rarely disappointed the expectation of the physician." Quinia seems to have been generally administered in cases which assumed an intermittent type, or were of slow convalescence. (i. 166, 173-4, 202, 208.)

3. The Academy of Sciences, at Paris, at its session on the 8th of February, 1858, awarded to M. Brown-Séquard, a prize for his persevering researches into the properties of the arterial and venous blood.

The same Academy has awarded a prize of 2,500 francs to M. Broca for his treatise on aneurisms—the same sum to M. Morel for his treatise on degenerations (*Traité des dégénérescences*)—the same to MM. Delafond and Bourguignon for their researches on the *Itch among animals.—Gaz. Heb. de Méd.*

4. *The Bréant Cholera Prize of* 100,000 *francs.*—It is essential for the successful competition for this prize that the *concurrents* shall fulfil the following or one of the following conditions, namely: Find a treatment which will cure Asiatic cholera in an immense majority of cases; or indicate in an incontestable manner its causes in such a way as to lead to their removal, thereby extinguishing epidemics; or they must discover a prophylactic as evident as that of vaccination in preventing small-pox. (*Proceedings of the Acad. des Sci.*, Feb. 8, 1858.)

5. *The Epidemic Yellow Fever of* 1857, *at Lisbon.*—Dr. P. Garnier, in his account of this epidemic, published in the *Union Médicale* (March 11) says, that the disease appeared suddenly without any precursory signs. During September, October, November and December, the admissions into the special hospitals amounted to 5,834; 4,748 men, and 1,116 women; of this number 2,063 or 1 in 2.82 died. The rate of mortality among an equal number of male and female patients, was nearly the same for each sex. Neither the number of cases nor the mortality in the city outside of the hospitals was ascertained with sufficient precision to indicate the severity of the epidemic. The number of known cases not in the special hospitals, was 7,842. According to official bulletins, the ratio of the mortality was 1 to 3.5 of the cases. Two of the victims were physicians. The epidemic reached its maximum on the 20th of October.

At the commencement of the fever the intermittent and remittent form was rare and irregular, but became more frequent and regular

as the epidemic declined. The intermittence of this disease had neither the regularity of type nor hour characteristic of paludal fevers, nor did it give way to the sulphate of quinine.

Yellowness, in severe cases was very strongly marked. Hæmorrhages from the skin and mucous membranes were extremely frequent.

The treatment consisted in Saline and oily purgatives, mineral and vegetable acids, quinine, camphor, valerian; cups over the stomach, blisters, stimulating frictions and sinapisms. The sulphate and carbonate of iron with other hæmostatics were given to arrest hæmorrhages. Death generally took place on the fourth or fifth day.

An article which appeared in *El Siglo Medico*, of Madrid, (Dec. 13,) says that the sulphate of quinine in moderate doses was the principal remedial agent used in the treatment of this disease at Lisbon.

6. Provisional Tableau of the Geography of yellow fever beyond the limits of the United States, 1857-8: Rio Janeiro; Bahia; Rio Grande (Brazil); Montevideo; St. Thomas; Kingston; Port au Prince; Havana; Bermuda; Vera Cruz; Lisbon.

B. Dowler.

Monthly Summary—Meteorological Register.—*From the Medical Purveying Office, United States Army, New Orleans.* New Orleans, La., Lat. 29 deg. 57 min. 30 sec. North; Long. 90 deg. West; Altitude of Barometer above the level of the sea, 35 feet.

1858.	Barometer.			Therm. Attached.			Thermometer.		
Months.	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
March.....	7 A.M. 2d. 30.440	2 P.M. 7th. 29.656	30.136	Several. 74	Several. 53	65.68	2 P.M. 21st. 77	9 A.M. 2d. 42	64.41
April 16..	7 A.M. 6th. 30.200	29.824		Several. 77	7 A.M. 1st. 63		Several. 79	7, A.M. 14th. 56	

1858.	Hygrometer.			Prevailing Winds.	Rain.	
Months.	Max.	Min'm	Mean		Days.	Quant'y
March.....	Several. 73	7 A.M. 2d 39	61.50	East, Southeast and Northwest.	6	4.71 in.
April 16..	2 P.M. 6th. 77	Several. 52		Southeast, S. West and North.	2	2.25 in

THE NEW ORLEANS
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ORIGINAL COMMUNICATIONS.

ART. I.—*Case of Epilepsy, trephined three times with relief:* By WARREN STONE, M. D., Professor of Surgery in the Medical Department of the University of Louisiana: Reported by JOHN MORRISON, M. D, of the *Maison de Santé.*

MR. D. C. F., aged fifty-three years, a native of Philadelphia, Pennsylvania, and a mechanic by occupation, was admitted into the *Maison de Santé,* March 26, 1858. He complained of fits, as he termed them, coming on at intervals of two weeks ; sometimes he would have one every week, and then for a time he would not have one, but a succession of them, as if to make up for the intervals at which they usually occurred. This was his statement.

The history of his case, which I think an interesting one, is this : In 1837, he was admitted into the Charity Hospital, then under the care of Dr. Stone, with yellow fever. After recovering, he was employed in the hospital as a nurse, and after remaining six or eight months, left; but was admitted again with pains in his legs, shoulders, and arms, having also iritis. His disease was pronounced syphilis, and he was relieved by a course of treatment for that disease. In 1839, he was again troubled with nocturnal pains, and was treated with mercury, which seemed to relieve him. In the spring of 1840, a soft node made its appearance on his forehead, about an inch and a half above the supra-orbital ridge and to the left of the superior longtitudinal sinus. This node gave him pain and uneasiness of the head. He was treated with mercury, blisters, etc., and relieved in the fall of that year. It was at this period the fits first came on. His

case was pronounced epilepsy, and the cause was supposed to depend on some diseased condition of the skull and internal periosteum at the point occupied before by this node. About a year after this, Dr. Stone applied a trephine over this spot, and removed a piece of bone about an inch in diameter. As soon as the operation was performed the patient expressed himself relieved. He stated that there had been something pressing on his brain at that place, and that he felt that it was now removed. He soon recovered from the operation, left the city, and resided in Vicksburg, Mississippi ; but there in the winter of 1848, the fits again returned, occurring at intervals of one month, but soon becoming more violent and with shorter intervals. He left Vicksburg in the spring of 1849, and lived in a village in the interior of the State. But finding no relief from change of residence and medicines, he determined to come to the city and undergo another operation for his relief. Accordingly, he was admitted into this hospital in the fall of 1853, and the trephine was again applied and a piece of bone removed about half an inch in diameter, below and on the margin of the former place. As soon as this, and a thin scale of bone from the point at which the trephine was first applied, was removed, he again expressed himself relieved ; that it was the pressure and sinking in of the skin on the brain that had caused the spasms. After remaining in the hospital a few days, he returned to his home in Mississippi, and nothing more was known of him until the date of his admission as above stated.

He had no return of the fits for a year or two, and worked at his trade. They then returned with the usual phenomena, becoming more severe, and coming on at shorter intervals, until a month before he came to this hospital; he would have one every week or two, and when the time passed without one, a double convulsion would follow at the next period.

The benefit which he had received from the former operations induced him to return to the city, and as he expected a fit in a day or two, he was anxious to undergo the operation at once.

His bowels were cleared with a drop of croton oil made into two pills with bread crumbs; one to be given that night, and the other at six o'clock in the morning. Low diet was prescribed.

Next morning, March 28, Dr. Stone made a semi-lunar incision through the skin, etc., over the point, including in it the two places where the

trephine had before been applied, and removed another piece of bone about half an inch in diameter, near the last place trephined. On removing the piece the dura mater was found very much thickened and very firmly attached. It was also thick and cartilaginous for a considerable space around the places of the first two operations. The edges of the skull around had become thin and sharp, little or no ossification had taken place in the places from which the pieces had been removed, but there seemed to be some pressure from a jutting down of these edges. An elevator was placed under them, and they were raised all round so as to remove all pressure from the brain. The patient said he felt relieved at once, and that his fits would not return. The wound did well, and he left for his home in Mississippi, April 5th.

Mr. F.'s case presents a very interesting illustration of the way in which the syphilitic poison may affect the parts within the cranium. The dura mater, like any external periosteum, becomes the seat of inflammation; fluid may be effused beneath it, constituting an internal node, and this by its mechanical pressure gives rise to that disturbed condition of the nervous system which we recognize as epilepsy. Sometimes, as in F.'s case, thickening of the dura mater occurs, and like any foreign substance, will produce disturbance of the functions of the brain, epilepsy, dulness of intellect, thickness of speech, etc.

It is probable that F. had an internal node, as well as one externally; and the appearance of the piece first removed seems to confirm this view. It was carious in the internal as well as the external surface—a hole, in fact, extended through to the dura mater. At all events the dura mater was involved in the same process of inflammation which gave rise to the node on the forehead, and after the absorption of this, gave evidence of its existence by the epileptic convulsions. The relief which followed the first trephining was decided, the brain was no longer pressed upon by the dense, thick dura mater ; there was no more irritation. The cause was removed, and the disease ceased. In the course of time a new formation of bone occurred at this place, and, pressing on the central surface, caused a return of the convulsions. The remedy was its removal. This second operation gave relief at once, and the last time, although it was impossible to predict a permanent result as regards relief, the temporary relief will be a great comfort to this man.

Not unfrequently patients are treated for epilepsy, and take a great deal of nitrate of silver, belladonna, and the different salts of copper, zinc, and iron, without benefit. It is always proper to inquire into their previous history. The chances are that their disease is consequent upon a syphilitic taint, and the statement of having had a lump or node on the head is sufficient ground for an examination into all of the particulars of their past history. Dr. Todd, in his Lectures "On the Diseases of the Nervous System," (Lecture xvii) details some very interesting cases of this kind, and mentions the great efficacy of iodide of potassium, given with or without mercury, as the circumstances of the case require.

These epileptic convulsions connected with inflammation of the dura mater and thickening of that membrane of the skull, were pointed out in the hospital, and treated of in the lectures at the Medical College of Louisiana, long ago.

Mr. F.'s case will be kept sight of, and the result of the last operation will be mentioned again.

Maison de Santé, April 7, 1858.

ART. II.—*Comminuted Fracture of the Thigh; Amputation; Recovery:* By WARREN STONE, M. D., Professor of Surgery in the Medical Department of the University of Louisiana: Reported by JOHN MORRISON, M. D., of the *Maison de Santé.*

MR. R. W. F., aged forty-eight years, a native of Georgia, but a resident of Concordia Parish, Louisiana, a farmer, was admitted into the *Maison de Santé*, March 9, 1858, with a comminuted fracture of the right thigh, about the union of the middle with the lower third of the femur. He stated that he was in the woods with some negroes, who were felling trees, and that a tree in falling, caught him while running from under it, and broke his thigh. He was carried home and the limb was dressed immediately with such appliances as could be obtained. Inflammation set in soon after. He could not sleep. The dressings were removed and reäpplied from time to time, and the usual treatment in such cases adopted. About two weeks afterward, the

leg became very œdematous below the place of injury; great pain was felt in the calf of the leg and dorsum of the foot. Gangrene of the great toe, and the two toes adjoining, together with a space on the dorsum of the foot set in. The gangrene did not extend and there seemed to be some prospect of saving the limb. After remaining about three weeks on his back, suffering a great deal, and finding little or no improvement, he determined to come to the city; and was admitted into this hospital at the above date.

His condition was very unfavorable indeed. The leg was exceedingly painful on the slightest movement. The structures around the fracture were red, and tender to the touch, and the whole limb was very œdematous. The gangrenous portions of the foot were beginning to be thrown off, and some effort at repair was going on. He was ordered 25 minims of Battley's sedative. The leg was placed in an easy position, on pillows, and he passed the night comfortably.

On the morning of March 10, Dr. Stone examined the leg, and a distinct fluctuation was perceptible at the place of fracture, and the crepitus was easily detected. There was no doubt a fracture of the bone into three or four pieces, and the fluid was thought to be blood which gave the fluctuation. The doctor made a small puncture, introduced a female catheter; about a pint of dark grumous fluid flowed out. This gave him great relief.

The opiates were given again that night, and he slept better. Next morning the fluid had again accumulated and was drawn off with the catheter. This circumstance induced the belief that the femoral vein was lacerated, and that the blood was constantly flowing into this false aneurism.

Solid opium was prescribed, with a pretty liberal diet of beef tea, milk, and brandy, etc.

March 12, showed another accumulation of the blood. It was again drawn off to the quantity of a pint. He did not sleep well that night. This bleeding was evidently telling on him very fast. His diet was made more liberal, brandy, opium, and sulphate of quinine were prescribed.

It was evident that he could not stand to lose a pint of blood a day, none in fact. Accordingly the Doctor decided on amputating the limb. On the following morning, March 13, an examination, however, showed some evidences of erysipelas, and he was ordered the tincture

of the muriate of iron, and water dressings to the parts. The diet and medicines were repeated.

These unfavorable symptoms soon ceased, and on the morning of March 14, he was placed on the operating table. Two drachms of carbonate of ammonia, were administered by the rectum. Chloroform was given. A circular incision was made around the thigh, about midway between the trochanter major and external condyle of the femur through the skin and cellular tissue; the flap was turned back, and the muscles, etc., were divided down to the bone, this was sawed smoothly off, the arteries were tied; and after all oozing of blood ceased, the stump was dressed with plasters, etc. A little brandy was given him, and the stump placed in a pillow. He lost but little blood, in fact his pulse was better after than before the operation.

An examination of the limb showed a comminuted fracture of the femur and laceration of the femoral vein. The artery was also inflamed and very much thickened in the popliteal space; it was not obliterated, but the caliber much diminished. The soft parts presented a bluish red appearance, but were otherwise tolerably healthy.

The patient was troubled with a diarrhœa the following day which was soon controlled with opiates, etc.

Notwithstanding the bruised appearance of the soft parts, a portion of the flap healed by the first intention, and the rest soon healed by granulations.

He will leave for his home in a few days.

Mr. F.'s case presented some points of interest. The gangrene of the foot had been caused, no doubt, by the disturbance of the return of the venous blood, together with the disturbed function of the nerves. I should think an obstruction of the venous current would as likely produce gangrene of the extremities of a limb, as destruction of an artery or nerve. It may be objected in the above case, that the collateral venous circulation was sufficient to compensate for the loss of the femoral vein. But Mr. F. was worn down, haggard, miserable; his powers of life were nearly exhausted, and a slight cause would have been sufficient to destroy the vitality of the badly nourished limb. In the absence of any other tangible reason, it was attributed to the laceration of the vein, and the disturbed circulation consequent upon it.

The precaution of stimulating him with carbonate of ammonia just

before the amputation, I have no doubt enabled him to survive the depressing effects of the chloroform, and the shock to the nervous system consequent upon the operation. A stimulating enema given just before a severe operation, has made all the difference between the life and death of a number of patients treated in this hospital.

Maison de Santé, April 9, 1858.

Art. III.—*An Essay on the Cause, Treatment and Cure of Asiatic Epidemic Cholera:* By T. B. Pinckard, M. D. (Concluded from page 378.)

We mentioned that we had practised in epidemic cholera, in Versailles, Kentucky, in 1835. This wealthy village contained about twelve hundred inhabitants, and is twelve miles from Lexington, in the richest and most beautiful country in the State, having no stagnant pools of water, or damp marshy ground, in the town or near it. The only running water bursts from a ledge of lime-stone rocks in the town, forming a large basin of water, called the "Big Spring," passing in a strong current through the lower part of the village in sufficient quantity to turn a mill. This "big spring" was the only resource for water, in dry summers, for the whole population (man and beast).

It was in August, on a Sabbath, during the Meeting of the Presbyterians, in great numbers, that the cholera made its appearance after dinner. Numbers were attacked, principally the citizens, and many died before the next morning. The attack was so sudden, and no cholera being in the State, that it was reported the water of the big spring had been poisoned. In a few days the disease spread in the neighborhood. All the citizens who could leave, fled in alarm, even some physicians; others were taken sick, among the latter a young physician of talent, and very popular, who was untiring in his labors and very successful, and was the last to succumb to that dreadful disease.

We, with two other physicians from Lexington, immediately went down at this crisis, and volunteered our services without compensa-

tion. We soon had constant employment, though only a remnant of the population remained, numbers having left for the country or died. We will only detail one case to illustrate the curative powers of water. The first evening of the day we arrived, we heard of the illness of a youth of twelve or thirteen years, who was a general favorite in the town, from his comely person, politeness and accommodating manners. He was known as he went round to all the housekeepers with meal and flour ground at his father's mill; hour after hour we heard he was getting worse, and finally late in the night that he was collapsed, and that his two physicians had left him, and said he would die before morning. As I knew the physicians were opposed to drinking cold water in cholera, I had several times, during the night, expressed a wish to those who told me of his hopeless situation, that his physicians would give him plenty of cold water. At midnight, in passing the street to retire for the night, I met a friend of the family in search of me to visit the dying youth. In a few minutes I was by his bedside, now apparently in the last stage of collapse, and though dying, only very small quantities of drink were allowed him, with small pieces of ice to dissolve in his mouth. We immediately had a pitcher of ice water prepared, and gave it to him freely, affording him the most exquisite pleasure, and we confess we shared it with him. Next morning we found him entirely relieved of all disease. He convalesced rapidly.

We will give the case of an uncle of this youth, which occurred in the epidemic of Lexington, two years previously. He was not our patient, but we heard all the circumstances of his case from those that were with him; his illness and recovery were of public notoriety, as he was considered the most enterprising, the largest and most successful manufacturer of cotton and wool in Kentucky. His loss would have been a public calamity and especially to numerous poor families; he was liberal and kind to his white operatives, and beloved by them. His two physicians from the time he was attacked, (which was violently,) were untiring in their attention to him. They were experienced and thoroughly educated men; they exhausted all their resources in the science of medicine on his case, through the day and till late at night; but the disease progressed rapidly on till the last stage of collapse appeared. The physicians were exhausted, and as they could no longer be useful, they left him, telling his family and

numerous friends that he would die before morning. He had during the evening and night begged incessantly for cold water, which had been denied him by his iron-willed physicians ; for two such determined physicians in enforcing obedience to their imperative orders we never knew, the elder especially, though extremely kind and devoted to their patients.

The patient, a man of great determination and self-will, long accustomed to command, submitted only because he could not procure water, and they had impressed him that it would be dangerous in the extreme to take it. But so soon as they had left him to die, he ordered his servant imperiously to bring him a pitcher of cold water fresh from the pump in his yard, as he intended to enjoy cold water the few hours he had to live. His friends attempted to remonstrate against his indulging, he lost his temper at their being so unreasonable, and without a moment's delay he slaked his consuming thirst, and continued to drink till sleep overcame him. Next morning he was convalescent, producing great joy in the town.

In January, 1848, we removed to New Orleans on account of chronic bronchitis, with which we had suffered for several years. During the summer we practised medicine on the sea-shore, where many persons and families from the city, and coast of the Mississippi river, visited and lived through the summer. On our return to the city we opened an office. In December the cholera appeared very unexpectedly and in an epidemic form, proving very fatal, especially among the lower and imprudent portion of the population, and strangers. Having formed the acquaintance of many respectable persons at Pass Christian, on the Gulf shore, the few cases we were called to see were in the first and second stage of cholera, except two slaves, who were collapsed. Those in the first and second stages were easily relieved by the very same treatment we had adopted in Lexington and Versailles, as after a space of fifteen years, cholera was the same and the treatment required the same.

The two slaves collapsed were as thirsty in New Orleans, in 1848, as any we had seen in Lexington, in 1833, or in Versailles, in 1835, and pure cold water had the same efficacy.

We visited the Charity Hospital and saw many dead and dying. All the patients appeared to be in collapse whom we saw, or near that stage. Those we saw at different times brought in, were in collapse.

Different modes of treatment were pursued by different physicians. Some patients were rubbed with ice. Many took quinine, and all, we believe, calomel, besides various other medicines according to the modes of practice. As there was no uniform mode of practice established in the hospital, the different attending physicians were properly trying to ascertain what remedies would relieve the greatest number of cases. We had for fifteen years, wisely or unwisely, (we leave common sense unprejudiced people to judge, not old physicians,) adopted a certain course of practice ; *we thought* we knew why it was a good one, in two stages of the disease ; and in collapse we did not think, but *we knew* in spite of the sneers of those called scientific, that we had discovered the *very best remedy.* We did not consider it infallible, for what imperfect human being would presume, would impiously dare to contravene the laws of the Almighty—"*Man is born to die.*"

We saw several of the attending physicians engaged in their arduous and melancholy duties, and told them, in a few words, the curative powers of cold water, and urged them to try it. They replied that they gave them ice, and that cooled their mouths and throats, and furnished water sufficient, and that more water would be injurious. Drs. Stone, Fenner, and others, we knew were absent. We met two eminent physicians at one of our visits, at the hospital, from Havana, sent by the Governor-General, to ascertain the methods of cure adopted in the New Orleans Charity Hospital.

We shall close this essay by giving *our opinion* of the *cause and prevention* of this fearful disease, differing, so far as we know, from any heretofore made public.

CAUSE AND PREVENTION OF CHOLERA.

We will not attempt to enumerate the various *causes* that have been assigned to the production of epidemic cholera, but merely mention a few of the most prominent that we recollect.

In Lexington, Kentucky, it was a popular opinion that the use of lime-stone water produced it in 1833. At that time, there were not a half dozen of cisterns in the city; now, and for some years, they are almost universal. Yet cholera has existed as an epidemic since the nearly exclusive use of cistern-water. But it is a matter of medical history, that cholera, in the United States, in lime-stone regions of

country, is more frequent and malignant than where granite formations prevail.

The theory of insects or animalculæ in water, has never been established. The theory of malaria was, at one time, more generally believed in than any other. The general spread and fatal form of cholera, in Lexington and Versailles, in two epidemics, ought to satisfy the warmest advocate of malaria, that the theory has no facts to sustain it, though Professor Cooke did, in his writings and lectures, attempt to bring forward proofs in support of it. Those acquainted with the different localities of Lexington know that the Doctor was entirely mistaken in what he considered facts to sustain his favorite theory.

A deficiency of salt in the blood first advocated by Dr. Stevens and many others, was at one time popular, and so of some others.

As to our Theory of the Cause of Cholera.—When we were quite young, and before we commenced the study of medicine, but had read the writings of Dr. Franklin on electricity, besides several others, we became firm believers in the wonderful powers of electricity in operating and controlling, according to fixed laws, all the kingdoms of Nature; we considered it as the principal and active agent in chemical science, which we studied with great interest, from the intimate connection of the two sciences, being so blended together as to make it impossible to understand chemistry without calling in the aid of electricity; for chemistry is entirely dependent on electricity for all the changes that take place in the great laboratory of Nature, as also the various analyses, and compounds produced by the scientific chemists.

After commencing the study of medicine in 1820, and reading several of the then standard works on physiology, we were convinced that electricity, galvanism, magnetism, solar and artificial heat were one and the same great agent in producing and continuing life in animals and vegetables, and that the nerves were the natural conductors of the electric fluid which is received into the lungs, producing respiration, and that function stimulated the heart to act, so as to produce continued circulation to the most remote parts of the body; and when a healthy nervous influence prevails, and consequently circulation, all the secretions are elaborated by the agency of this truly *vital fluid.*

We were in part led into these views of the vital and life preserving powers of electricity from reading in our text-book on physiology, that we knew nothing of life, the essence of life ; but that it was the science that instructed us in the phenomena and laws of life in a state of health. We immediately remembered that we had read in "*the Book*," from which all true knowledge is derived: "And God said let us make man in our image, after our likeness:" "And the Lord God formed man of the dust of the ground and *breathed into his nostrils the breath of life;* and man became a living soul." It was God's breath that produced *life* in the first formed man, and all mankind have become living souls in the same manner, from the first man, Adam, to the present time, and will in all futurity. God breathes the breath of life into every living and healthy infant directly as it enters the world, and continues to breathe into all mankind while life continues; the withholding his breath from any one is immediate death. His breath is not only necessary to produce life in man, but it is constantly required for his preservation, and for all animated nature; all the productions of the earth perish if God's breath is withheld from them. The whole mineral kingdom would fall into a chaotic state and be without form, if they were not preserved in their regular forms by God's breath. *The electric fluid is God's breath.* The sun, comets, meteors, aurora borealis, volcanoes, earthquakes, tornadoes, storms ; the first four are accumulations of the electric fluid. The sun is the great reservoir of that fluid, and the other three are supplied regularly from that luminary, and from the accumulations in the earth. The sun and the earth are constantly changing their electrical state, more especially the latter, and when comets form, and vast quantities are conducted to certain points and form volcanoes and earthquakes, a portion of the earth's surface is deprived of its natural quantity, and fearful epidemics prevail ; for a certain portion of this life and health-preserving fluid is essential to the well-being of man. Certain states of the atmosphere cause an entire change of the amount of the electric fluid in it. A warm, moist atmosphere, surrounding any particular portion of the earth's surface, conducts from the earth and all bodies on that portion, too much of the electric fluid for health. At the same time the regular supply of the fluid is not received from the sun; for it is an unchangeable law in the science of electricity, that if one body is receiving the electric fluid from another, it cannot fur-

nish in return till it receives its full supply. Every form of matter is either *positively or negatively electrified.*

Volcanoes and earthquakes, so destructive in their effects, are accumulations of the electric fluid in the bowels of the earth, and when it acts on the mineral and bituminous substances, the most fearful and destructive effects are produced. Who could witness these and not feel that no other power but the Almighty's could make the earth tremble and tear assunder his own mighty works, which he had created before he formed man?

The present age has been designated as one of science and sound philosophy. Our wise men say that a large portion of ancient knowledge was intermixed with absurd superstitions, and one of those superstitions was believing that comets, when they appeared and approached the earth exercised great influence on it. This we believe can be demonstrated to be true, so far as heat and cold, dry and wet weather are concerned, and also in the production of disease. A knowledge of everything connected with the electric fluid will demonstrate this proposition.

We assume the *data* that comets are vast accumulations of the electric fluid which has in its natural passage from the sun to the earth been attracted by bodies (*nuclei*) which have accumulated in open space between the sun and the earth, and are composed of heterogeneous materials which are from the earth's surface; these masses were originally small collections of earthy matter, etc., which small collections are often seen, bright and brilliant from the electric fluid, which they have attracted from the sun and earth, and are designated meteors, (they are small comets) and they often explode, and the materials forming them fall to the earth, showing of what they are composed. It is by the power of attraction and gravitation that these meteors are collected into one great mass constantly accumulating, as their power of attracting the meteors increase with their size, and at the same time depriving the earth of its electric fluid in proportion to its near approach, and also receiving from the sun that which should have been given to the earth; the greater the distance of the comet from the earth the greater the degree of cold will prevail, and the nearer it is to our planet the warmer it will be. It is an axiom in pathology, that if there are great changes in the amount of electricity, that disease will be the inevitable result; a deficiency

always producing *asthenic*, debilitating diseases, and a dry cold atmosphere in which electricity is positive in excess causing *sthenic* inflammatory diseases. These facts are known, or should be.

The type of diseases are formed and regulated by the amount of electricity received into the lungs.

The excessive cold winters of 1856 and '57 were caused from the comet depriving the earth of its full supply of electricity (heat) from its proximity to the sun, and being between the sun and earth, and its receiving that fluid, which is heat, in place of the earth. When comets approach the earth's surface, excessive heat is produced ; but in either case, the effect of large comets is to derange the seasons by interfering in the regular established laws of the universe, causing either excessive cold or unnatural heat, too little rain or too much, and, under certain circumstances, devastating epidemics. There is no sound philosophy in the ancient opinion that comets were the forerunners or warning of bloody wars. It may be that the influence of the changed state of the atmosphere and of the altered amount of electricity may dispose the human mind to be acted on and dispose those in power to engage in wars which they would not if the mind had been properly balanced and in a healthy state.

In modern times, there has been much labor by educated medical men in trying to demonstrate that all forms of fever, and many other diseases, are produced from *malaria ;* even some of the contagious diseases, as yellow fever, plague, etc., are placed in the same category. Now all admit that we know nothing of malaria or marsh miasmata, analytically. Chemists who have analyzed the atmosphere of the Pontine Marshes, and the river Niger, where fevers of the most malignant type attack healthy persons by remaining a single night, have found the same properties of oxygen, etc., as in the mountain air. Malaria is only known to exist from certain effects being produced on the human body in certain marshy, damp situations, and the active mind of man naturally wishes to trace certain effects to some cause producing them.

We now present our theory as accounting, as we conceive, much more rationally for all forms of disease, *except contagious and specific diseases.* But we will only illustrate our theory by the effects produced in the human system by epidemic cholera, and give a passing notice to intermittent fever.

The first impression in all diseases of body or mind is first seen on the nervous system (except, probably, in contagious and specific diseases) in the blood, which is contaminated, and the nerves suffer secondarily. We noticed in the three epidemics of cholera which we witnessed, that the atmosphere was close and moist, and in that very state to have a negative (deficient) portion of the electric fluid, and this was ascertained to be the case, and in addition, in the best possible state to conduct off rapidly from the surface of the body, the deficient quantity received by the lungs; for the lungs inhale the oxygen of the atmosphere which always contain the electric fluid; so soon as the lungs receive this agent, the electric fluid, with the rapidity of lightning, is conveyed by the nerves to every part where nerves exist, and the skin being a net-work of nerves, is the part charged with that vital fluid. In health, the capillary blood-vessels perform a most important part in the animal economy. The oxygen which was combined with the electric fluid has performed its office so soon as it has conducted that fluid to the lungs, and at the first expiration is thrown off as effete matter.

The true cause of cholera is the deficient quantity of electricity in the atmosphere, in certain infected localities, and the favorable state of the atmosphere that existed for its being conducted rapidly off from the surface of the body; this produced great debility generally, and especially affected the exhalants which poured out the fluid part of the blood. That this is the true cause of cholera, and will generally be received, when presented in a more elaborate and polished manner, there can be no doubt, and the same may be said of what are called miasmatic fevers. We could enlarge very much on the effects of electricity in preserving health, causing diseases, and preventing them, and also its therapeutic effect.

We again urge our claim for the 100,000 francs left by Mons. Bréant, of France, to be awarded by the members of the Academy, at Paris, to the person who discovers *the cause of cholera and its cure*, and if they fail in doing their duty, we appeal to the medical men of France.

ART. IV.—*Contributions to the Hydrographical Thermology and Hygiene of the Mississippi River:* By BENNET DOWLER, M. D.

THE Mississippi river annually discharges about thirty times more water than the Rhine, five or six times more than the Danube, or Volga—a third more than the united waters of the Rhine, Loire, Po, Elbe, Vistula, Danube, Dnieper, Don and Volga—three or four hundred times more than the Thames, fully as much as the Euphrates, Indus and Ganges combined, and nearly twice as much as the Nile, equalling that of the Rio de la Plata, being surpassed only by the Amazon. The latter discharges thirteen hundred times more than the Thames—one hundred times more than the Rhine.

If half of the inhabitants of the world were concentrated in the valley of the Mississippi, the filth or refuse entering the river would scarcely contaminate its waters so far as lower Louisiana is concerned, to the extent described by certain authorities, (Raspail, London Lancet, etc.,) as found in the Seine and Thames, whose waters are drunk, used in cooking, etc., in Paris and London.

The immense water shed of the river is, for the most part, several thousand miles distant from New Orleans, expanding from the summits of the Apalachian chain on the East, to those of the Rocky Mountains in the West, having on its northern boundary, by far the largest fresh-water inland seas or lakes in the world. This area whence the river gathers its tide, occupies about eighteen degrees of latitude by, perhaps, twice as many of longitude, and supplies throughout the entire year a vertical column of water from one hundred to two hundred feet for hundreds of miles above the mouth of the river.

In the region of the Lower Mississippi, the rain, the filtration water of the river itself, and the water courses, do not run *into* but *from* the river, and consequently they carry off into the swamps, lagoons, bayous, lakes and Gulf, animal, vegetable and mineral matters capable of solution, suspension, transportation, putrefaction. Thousands of miles from New Orleans the tributaries of the river, every swollen brook, rivulet and stream, reaches the larger affluents of the river charged with animal, vegetable and mineral matters, which during a journey of several weeks towards the south under a hot sun, necessarily undergo great modifications. New chemical compositions and decompositions must occur, the heavy detritus sinking, mineral matters being deposited or newly combined. Here every condition

exists favorable to the putrefactive process and the dissipation of deleterious gases arising from the putreseible matter in the water. Hence, the water though loaded with detritus, on reaching New Orleans is comparatively pure and salubrious, and the more so because the lower Mississippi is no longer a sewer to receive filth, the drainage being in the opposite direction. If the sewers of London and Paris were placed a thousand miles up the river on its banks, the mass of the water would probably undergo self-purification before reaching New Orleans.

Prof. Liebig, in his remarks upon the influence of temperature upon fermentation, says that "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." The transforming influences of temperature on fermentable or putrescible substances, in connection with running water, are paramount in altering their original character, after a time.

The Amazon, the greatest of all rivers, running nearly parallel with the equator, after receiving its principal mountain tributaries, traverses from West to East twenty-two degrees of longitude, debouching nearly under the line, having, of course, no variety of climate so far as heat or cold are concerned, with the exception of that from the elevation or depression of the land, and from the rainy or dry season. Its inundations extend beyond its banks for hundreds of miles, being in this and many other respects, including its apparent hygienic influences, analogous to the lower Mississippi. A slight allusion to its medical topography will serve to show this. The sanitary condition of Para,* the largest town on the lower Amazon, will be the best test; the other considerable town, Santarem, is six hundred miles higher up the river, and has a population of only one-third as many as the former. Para is on the great southern outlet of the Amazon, called Para river.

The city of Para, (one deg. eighteen minutes, S.) situated on this

* Para formerly numbered nearly 30,000 inhabitants, and had considerable commerce in cotton, sugar, coffee, cacao, vanilla, copaiba, sarsaparilla, rice, India-rubber, hides; and hence liable to receive yellow fever, by importation from the North America and the West Indies, if it be communicable in that way. This city has fallen into decadency from political troubles and Indian massacres, and is now a yellow fever city after two centuries of exemption.

river seventy miles from the mouth, now numbering a population of fifteen thousand, was founded more than a century before New Orleans; had been one of the healthiest on the continent until February, 1850, when a most fatal epidemic of yellow fever set in, and, by April, twenty or thirty died daily.

Mr. Edwards, of New York, who resided in this city previous to the irruption of yellow fever, (see his Voyages; 1848; p. 252, *et seq.*) says: "Multitudes of persons from the Northern States now visit the South in search of health, or spend their winters in the West Indies, at great expense, and little gain, who in Para could reside for comparatively nothing, with a certainty of recovery. The novelty and beauty of the country, as well as the luxury of the climate, afford sufficient inducements to the invalid for seeking both health and pleasure, in Para. Its climate is peculiarly favorable, the extremest heat being less than in New York. Para has no kind of epidemic. The people live to a good old age." He says that whilst intermittent fevers are rare on the banks of the Amazon, where he never met with but one case, which he cured with a single dose of quinine, they abound on its tributaries, instancing the Madeira river. (163, 195.) Of the lower Amazon he says: "Its climate is singularly healthy."

In the upper Amazon where fevers are prevalent, Humboldt says, "the people have the most inveterate prejudices against the employment of cinchona, and in the very country where this valuable remedy grows they try *to cut off the fever* by infusions of *scoparia dulcis*, and hot lemonades prepared with sugar and the small wild lime, the rind of which is equally oily and aromatic." (*Narrative V.* 164. Lond.)

May not this exemption from epidemics, and fevers upon the lower Amazon be in some degree owing to the hygienic influences of the river? or to the comparative purity of its waters? or to its inundations and the cooling effects of evaporation from a vast expanse of water?

The comparative absence or diminished ratio of diseases attributed to marshes and inundations in the lower divisions of the two largest rivers upon the globe, the one debouching near the border of the torrid zone and the other in its centre, remains to be explained by some other theory than the prevalent one.

Without vouching for the doctrine of malaria as the cause of all diseases called paludal, it may be asked whether there is not reason

to suppose that the waters of the upper Mississippi and its tributaries are insalubrious either in themselves or in their concomitants, as compared with those of the lower river? For example, (and here I speak from personal observation,) the Illinois river during its periodical season of inundation, extends over an area approximating that of the lower Mississippi during the rise of the latter. The banks of the former are infested with severe intermittent fevers to an extent utterly unknown upon the banks of the lower Mississippi. The water of the former compared with that of the muddy Mississippi is clear. Do the waters of the Illinois contaminate its shores, swamp-basins, and bottom lands, and a month subsequently arrive depurated in the Delta, after a journey of fifteen hundred miles, occupying a month or more? The affirmative would be no more flimsy than that of many hypotheses, which make up the warp and woof of the received ætiology of both endemics and epidemics.

May not the periodical inundations from the river in lower Louisiana, by affording a vast surface of fresh and comparatively pure water, be salubrious and preventive of the development of miasma? Authorities might be cited among miasmatists themselves to show that noxious gases may be absorbed or imbibed or otherwise limited or totally arrested by bodies of water. Besides, evaporation from eleven thousand square miles which the river inundates cannot fail to cool the atmosphere, and thereby lessen one of the principal causes of some diseases, namely heat.

The clearing away the woods in Louisiana would, theoretically speaking be attended by an increased heat, but probably this increased heat from the action of the solar rays upon the exposed surface of the ground may, during the hot season, be counteracted by increased evaporation and consequent refrigeration which these rays and the freer admission of the winds produce. During the present century there is reason to think that the cold has not been as intense as during the last. Ninety pears ago (1768) the cold was so severe that the orange trees of Louisiana were killed, and ice was formed along and projected from the shores of the river, at New Orleans. Evidence, it is believed, could be adduced, showing that in early times drifting masses of ice descended to this city, from the upper rivers. Such occurrences having been extremely rare, may afford no reliable data as to the average temperature of the country during the entire year.

There is evidently some connection between rivulets or mill-ponds and sickness, upon many streams of the upper rivers, even in hilly regions where marshes are not found. Such a connection can seldom be traced amid the bayous, innumerable lakes, and immense swamps of the lower Mississippi, compared with which the Pontine Marshes of Italy and the mill-ponds aforesaid, dwindle into insignificance.

It is believed that the diseases usually ascribed to marsh poison, are not only rare on the banks of the lower Mississippi as compared with many of the upper rivers, but as compared with its subordinate southern affluents, and the more elevated and hilly regions contiguous, including districts somewhat remote from its banks, the zone of greatest salubrity lying directly upon its shores. Yellow fever, however, is excluded from this favorable estimate.

The levee-system may be considered as belonging to the hygiene of the river. All the early writers upon the climate of Louisiana, concur as to its great salubrity from its first settlement until near the close of the last century, when (in 1796) yellow fever made its first appearance at New Orleans. Professor Forshey, in his Memoir on the River (1850) says, "the levees of Louisiana may be regarded as in full operation for fifty years, for a distance of one hundred miles from Bayou Lafourche down below the city." This period coïncides with that of yellow fever in New Orleans. It is remarkable that the expounders of epidemics who often construe coïncidences as causes, have not taken advantage of this parallelism from which to construct a theory, since disturbances of the water as well as of the earth, might be the cause of epidemics. The plausibility of such a theory might be greatly strengthened by another and still more decisive fact which contradicts existing theories, namely, that crevasse years are non-epidimic years. Of five years in which the most extensive crevasses took place anterior to 1858, none was accompanied or followed with epidemic yellow fever, unless 1849 be regarded as an exception.

In 1816, early in May, the inundation of the rear of the city took place from the Macarty crevasse, now the site of Carrollton, nine miles above the city. The water in the houses and streets had a depth of three or four feet.* On the third of the same month, in

* This Crevasse must have inundated a vast area of the city, as its outlet to the Lake was by the Bayou St. John, and chiefly through the original or ancient portion of the city. This part of the city was less inundated by the Sauve crevasse of 1849, as the water was arrested and deflected

1849, the great Sauvé crevasse, 18 or 20 miles above the city, took place, by which two or three hundred squares and several thousand tenements of New Orleans were submerged for nearly two months.

The year 1816 was healthful; the year 1849, during and after the inundation, was generally reckoned as a non-epidemic year, notwithstanding the prevalence of a mild epidemic of yellow fever. The three following years were without yellow fever epidemics. An expounder of epidemic ætiology not very scrupulous in his logic might argue that the great inundation of 1849 was the forerunner or cause of an unusually long exemption from yellow fever anterior to 1853. Surgeon H. L. Hawkins, U. S. A., in his official report on the medical topography of New Orleans Barracks, says: "The yellow fever of this climate may be traced to the following combined causes: 1. *Low stage of water in the river*, leaving its banks, with the deposits brought from the upper country exposed to the action of the sun. 2. Decomposition of vegetable matter in the swamps in the rear of the city; and, 3. The prevalence of E. and N. E. winds. A contrary state of things, to-wit, *high stage of water* and S. W. and W. winds, are not attended with epidemic fever." (*Med. Stat. Army*, 1856, 4to. p. 255.)

The Mississippi is virtually not only the great commercial and physical feature of the southern basin or delta, but it is probably the chief element, whether for good or evil, in its medical topography and hygiene.

If the ætiology of endemics and epidemics can be solved by the present methods of meteorological and topographical research, this river and its influences must be paramount in solving the enigma of the diseases of its basin. In this latter point of view it will be examined in more detail in the closing part of this paper, or upon some future occasion, as leisure and the space of this Journal may permit. In the meantime it is intended to offer the following laborious contribution to the natural history of its temperature, being it is supposed the first extensive series of observations ever made. The contemporaneous temperature of the air will also be included for comparison.

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towards the Lake by the new canal, higher up in the newer part of the city, now the First District. Probably in no town on the lower Mississippi is the impending danger from a crevasse comparable with that of the city of Carrollton. In 1843, Mr. Pilie, Surveyor, made an official report of the levels at and in the rear of Carrollton, showing that from the summit of the levee (which is eight feet high) to the low water mark of the Lake, the fall is sixteen feet and two inches, and that the Metarie Ridge, which rises seven to eight feet above the swamp, is two feet eleven inches lower than the levee. A few years since a land-slide at this place carried a house into the river, drowning some of its inmates.

A series of thermometrical experiments made about the same time, at certain depths in the soil is not sufficiently complete for publication. Nor will all the experiments made upon the river be reproduced; as for instance such as were made at various parts of the stream and at all distances from shore to shore. These experiments were important, however, in establishing the absolute thermal uniformity of the surface water however near or remote from the shore, provided the experiment be always made in a current and not in stagnant or shoal water. In order to ascertain this fact, a skiff was slowly propelled from shore to shore, and buckets full of water were taken while ferry boats were crossing at all seasons of the year and hours of the day. On one or two occasions, filiments of water were encountered the fraction of a degree higher in temperature than the residue of the current, never exceeding a degree. It may be safely assumed that an experiment made a few inches from the shore in the current will represent the whole. Some unsatisfactory experiments made to ascertain the temperature at different depths will not be given.

However unattractive the following tables may seem, it is hoped that they will as a contribution to the physical history of the river be acceptable to naturalists and physicians who esteem facts though they may not always be able to apply them straightway to utilitarian purposes. It is not intended to apply these facts to support or to oppose any general or special theory in medical ætiology, further than their suggestive import may warrant. If the reader be of the same way of thinking with Hippocrates, he will see the importance of investigating by physical tests, rather than by mere conjecture the characteristics of places, waters, etc. Hippocrates affirms that epidemics and endemics are owing or peculiar to certain localities, exhalations, winds, constitutions of the atmosphere, "*some being caused by the nature of the water.*"

1845.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River
June.						
29	6	78°	84°			
30	8	81½	84	7	84°	84°
July.						
1	11½	83	85			
2	5½	76	83	7½	81½	83
3	6	78	83	6	81	84

1845.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
July.						
4	5	75°	81½°			
5	5	75	80			
6	6½	77	79	3	93°	79°
7	5	75	78	7½	80	79
8	5½	75	78			
9	5½	74¾	78½	7½	86	79
10	5	77	79	2	94	80
11	5	76½	80	7	80	80½
13	11	84	80			
14	5	76	81			
15	6	77½	81			
16	6½	80	81¾			
17	6½	77	82			
18	5¼	75	82	7	82	82
19	6	77	82			
20	6	78	82½			
21	6	74	83	2	97	83
22	5½	76	83	5½	97	84⅓
23	6½	84	84½	6¾	96	84½
24	6	80	83¾	7¼	87	85½
26	6	83	85	7¼	87	85½
28	5½	80	85	7¼	78	85
30				4	85	85½
31	5½	74	85	4	86	85
Aug.						
1	8	75	85	4	88	85½
				7¼	83	85½
2	8	85	85	7	85	85
6	6	75	85	4	86	85
7	8	79	84¼	2	87	84¾
12	6	72	83	7	77	84
16				6	87	84
17	7	79	83½	6½	89½	84
18	6			6	89	84
19	6	79	83½	3	98	84½
23				2	93	85
Sept.						
8				6½	85	84½
11	8	78	84½			
15				5½	81½	84½
16	8	74	84			
18	8	79	84			
23	8	70	82			
Oct.						
6	8	59	70			
18	11¾		70			

1845.	A. M.		P. M.	
Day.	Hour.	Air.	River.	Hour.	Air.	River.
Dec.						
2	8	29°	52°	2	45°	52½°
28	8	30½	42	5	48½	43
29	11¾	54	43			
30	8	49	42			
Jan. 1846.						
1	10	66	42½			
2	7	50		5½	59	42
3	7	48	41			
4	7	48	41			
9	12	50½	41½			
10	9	39	41½			
11	7	32	40	3	52	24
12	8	40	40			
13	8	35	40½			
16	10	53	42	5½	60	42½
17	11	48	42			
18	11	49	42			
20	7	56		2	57	44
24	7	39		5½	54	45
25	11	66½	44	5½	65	44
28	8	50	43			
29	6¾	59	43¾	2	70½	45
30	10	52	44½			
Feb.						
2	9	61	44	¼	60	45
3	6¾	50	44½			
4				5½	61	45½
5	6½	57	45			
6				1	61	45¾
7	6½	47½	45			
8	6½	49¾	45			
11				5	64	46
12	6½	50	46	M.		46¾
14	6½	48	46	5½	50	46
15	6½	39	46	5	56	46
17	6½	49	46	4	66	46¾
18	6½	59	46	3	67	46½
19		53		5	52	45½
20	6½	40	45	5	52	45
21	6½	44	45	M.	56	45½
22	6	40½		M.	52	45
23	8	51	45½			
24	8	54	45¾	M.	71	46
25	6½	57	46	M.	66	46¾
				3	70	47
27	6½	38½	45½	2	59	46½

1846.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
Mar.						
2	11	53°	46½°	2	58°	46¾°
3	9	49	45¾			
4	11	54	45¾	3	71	46¾
5				4½	68	46½
6	6¼	58½	45¾	M.	69	46
8	10	62½	45½			
9				5	70	45
10	8	58	44⅓	6	56½	45
11	9	66½	45¾			
12				2	74½	45¾
13				M.	57	45¾
14	6¼	41	45½	M.	67	46½
15	6¼	53	46½	5¾	68	47½
16	6¼	51½	47	2	67	47⅜
17	6¼	54	48	2	72	49
18	6¼	58	49	3	75	50
				6	70½	50
19	6¼	60	50	3	74	50
20	6¼	66½	51			
21	6	56	50½	2	65	51½
22	6	60	51			
23	6	65	51			
24	6	61	51¼	3	76	52½
25	6	50	51¼	2	62	52
26	6	50	52	2	72	53
				6	67	53¼
27	6	55	53	3	75	54
				6	68½	54
28	6	56	53½	6	74	54½
29	6	61	54	3	71	54⅓
30	6	55	53½	3	69½	54⅓
31	6	56	53½	3	63	54⅓
April.						
1	5¾	60	54	3	69	54
2	5¾	59	54	3	74	54
3	5¾	63	55	3	65	55½
4	5¾	60	55	3	67	55½
5	5¾	63	55½	3	65	56
6	5¾	65	55½	3	68½	56
7	5¾	67½	55½	3	68	56
8	5¾	61	55½	3	71	55½
9	5¾	56½	55	3	70	55½
10	5¾	63	55	3	76	56½
11	5¾		55¾	3	80	56¾
12	5½	61	57			
13	6½	53	56	6½	51	56

1846.

Day.	A. M. Hour.	Air.	River.	P. M. Hour.	Air.	River.
April.						
14	6¼	50½°	56°	4	64°	56°
15	6½	56	56	3	78	56
16	6	55	56	6	71	56¼
17	6½	60	56½			
18	6½	68	58	2	73	59
19	6	67½	58	3	74	58½
20				3	78	59½
				6½	70	59
21	6	67	59	4	72	59⅓
22	7	67	59⅓	12	78	59½
23	6	66	59¾	3	84½	60¾
				6½	74	60¾
24	6½	71	60½			
25	8	69½	61½			
26				5	79	62¾
27	6	74	63½	M.	78	63¾
28	5½	66	63	4	80	64
29	5½	65	64	3	83½	64¾
30				6	74	65
May.						
1				3	82	65½
2	6	61½	65			
3				6½	74	66¾
4	6	65	67	6	75	67⅓
6	5¼	74½	68	3	86	69
7	5¼	76	69	3	86	70
8	5¼	67	69½	3	84	70
9	5¼	66	69¾	3	85	70½
10	5¼	68	70	3	86	70⅔
16	5¼	67	70½	3	84	70¼
17	5¼	67½	70¼	3	84	71
19				3	92	72
20	5¼	73	71⅓	3	92	72
21	5½	70	72	3	84	72½
22	5¼	70	72			
23	5¼	77	72			
26	5¼	72	73½	3	82	77⅓
27	5	73	74	2	87	74½
28	5	75¼	74½	2	87½	75
29	5	73½	75	2	87	75½
30	5	75	75			
31	5	76	75¼			
June.						
1				2	91	78
22	5	78	78½			
27	5	77	78½			
30	5	79	81			

1846.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
July.						
11				6	89°	84°
19	9	82°	84½°	7	81	84
25				7	86	86
31	5	75¾	86¾			
Aug.						
5	5	79¾	86⅓	1		86⅔
8				3½		86⅔
10				6½		85¾
17	5	79	86½			
18				6	82	86¼
22	5½	76	84			
23	7	78½	84	5	86	84½
26	5½	71	84⅓			
30				5	82	85½
31	8	80½	84½			
Sept.						
2				6¼	81	85½
8	5¾	77	84½			
13	5¾	78½	84			
	8	85	85	5¾	88	85½
14	5¾	82	85			
15	5¾	81	85⅓			
16	6	79⅓	84½	4	91	86¼
17	6	79	83½			
19	6	73¾	85½			
20	6	72	85			
22	6	74	85			
26	6	78	84½			
27	6	73¾	84½			
29	6	63½	83½			
30	6	69	82½			
Oct.						
3		62¾	81½			
6	9	77	80¼	M.	85	80¼
8	9	78	79½			
9	*			6		79
15	*	55½	76			
16				3	76	76
19	*	47¾	73½			
Nov.						
6	*	52½	67			
29	10	71½	58½			
Dec.						
6	10	73	57½			

* Sunrise, unless the contrary be indicated.

1846.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
Dec.						
28	*	60¼°	44½°			
29	*	63½	45½			
30	*			4	75°	48½°
31	*	64½	46½			
Jan. 1847.						
1	*	70½	48½			
2	*	52½	46½			
3	*	50	46½			
6	*	49	47½			
7	*	29	46½			
8	*	28½	46½			
15	*	64½	46½			
22	*	31½	40½			
30	10	48	38⅓			
Feb.						
2	*	59	39½			
9	*	66	44			
16	*	58	42			
March.						
6	9		46½			
18				M.	75	46½
19	9½	65	46¼			
20		62	46½			
22	11	66	47½			
25	9	61	49			
30	9	70	51¼			
April.						
5	*	69	55½	6¼	78	56¾
8	*	72	57¾	3	80½	59
12	*	58	59			
13	*	58¾	60½			
14	*	61	61¼			
	10	71	62			
17	*	54	62½			
18	*	61	63	6¼	70	63½
23	*	67½	66			
24				6½		66½
25	*	60	66¼	3	70	66¾
27				3		67
28	*	65	66¾			
May.						
8	*		69			
9	*	68	69½	3		70
10	*	65½	70	3		70½

* Sunrise, unless the contrary be indicated.

1847.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
May.						
11	*	68½°	70°			
12	*	74½	70⅓			
13	*	65	70½			
14	*	68		6		71°
15	*	69	71			
18	*	66	71			
22	*	65	72½			
29	*	71½	73¾			
31	*	74	74¾			
June.						
1	*	73	74¾	3	84	75¼
2	*	72½	75			
12	*	73	80			
13	*	75	80	3	84½	81
14	*	75¾	80½	3	85	81
15	*	77	80¾	3	86	81
16	*	77	80¾	3	81	81
20				3	82	81½
24				†	80	80½
27				†	81	80¾
July.						
13				†	77	81
14				†	82	81½
20	*	77	82			
26				†		85
30	*	78	85¾			
Aug.						
1				†	83	85¼
2	*	78	85¾			
9	*	71½	86	3		86½
13	*	76½	83	3	85	83¾
						83⅔
18	*	76	83½			84
22	*	76½	83½			
29	*	77	85			
Sept.						
1	*	76	84½			
3	*	75	84			
16	*	74	81			
23	*	68½	79			
Oct.						
6	*	67½	76½			
12	*	67½	77			
15	*	57	76			
24				M.		74½

* Sunrise, unles the contrary be indicated. † Sunset, unless differently noted.

1847.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River
Oct.						
30	*	56½°	69½°			
Nov.						
4	*	58	66			
11	*	65½	64½			
12	*	62½	64	3		64½°
Dec.						
5	*	44	49			
8	*	56	49			
11	*	59½	48½			
26	*	40	42			
Jan., 1848.						
5	*	48		M.		45
14	*	54	44			
16	9		45			
Feb.						
7	*	38	46½			
22	*	72	55			
29	*	53	51¼			
March.						
1	*	63	51¼			
11	*	46	49½			
13	*	59	49½			
21	*	72	52			
26	*	71	56			
31						59½
April.						
1		66	59½			
8				2		60
11	*	60	61			
14	*	66	61			
17	*	54½	61			
21	*	67	62½			
23				5		63½
30	*	67½	65½			
May.						
1	*	69	65¼			
3				†		67½
6				†		68
7				†		68½
12				†		70
June.						
11				3		78½
30	*	77½	83			
July.						
1				†		83
8	*	80	84			

* Sunrise, unless the contrary be indicated. † Sunset, unless differently noted.

1848.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
July.						
13				†		84°
30	9			†		83
Aug.						
2	8		84½°			
24	*	78½°	84			
28	*	78	84			
29				†		84½
30	8½		84½			
31	10		84½			
Sept.						
14	*	76	83½			
16	*	75	83			
24				2		79½
26	*	73	79			
Oct.						
27	9		71			
29				†		70½
Nov.						
2				3	63	68
12				†		63
22				†		56
Jan. 1849.						
1				3		46
5	9		44½			
14				4		44½
25	10		43½			
Feb.						
1	*	68¼	46½			
3	9		48			
6	9		48			
13	8		46			
14	*	40½	46			
19	*	27	44			
22						44½
23				M.		44
25	*	56	44	2		44½
26				†		45½
27				†		45½
March.						
2				†		47½
4	10		47½			
17				4		58
18	*	70	56			
19	*	64	56			
20	10		57½			
23	8		57½			

* Sunrise, unless the contrary be indicated. † Sunset, unless differently noted.

1849.	A. M.			P. M.		
Day.	Hour.	Air.	River.	Hour.	Air.	River.
May.						
29	.....	.....	.....	†	.....	.....59½°
31	.....	.....	.....	†	.....	..... 59½
April						
6	.....	.....	.....	†	.....	..... 61½
8	.....	.....	.....	†	.....	..... 62½
13	.....	.....	.....	†	.....	..... 64½
20	.....	.....	.....	†	.....	..... 63½
26	....	.....	.....	5	.....	 64
29	....	.....	.....	†	.....	 65
May.						
8	..... *	.....	..... 69½°	.....	.....	.....
18	.....	.....	.....	..... †	.....	 71
20	.....	.....	.....	..... †	.....	..... 71¼
23	.....	.....	.....	 4	.....	 72
25	.....	.....	.....	..... †	.....	 71
June.						
1	.....	.....	.....	..... †	.....	..... 72½
12	.....	.....	.....	..... †	.....	..... 76½
15	.....	.....	.....	..... †	.....	..... 76½
24	.....	.....	.....	 3	.....	 79
July.						
2	..... *	..... 76°	 79	.....	.....	.....
16	..... *	.....	.....	..... †	.....	 81
Aug.						
1	 10	.....	..... 82½	.....	.....	.....
31	..... *	..... 76½	..... 83½	.....	.....	.....
Sept.						
1	..... *	 77	.... . 83½	.....	.....	.....
10	 8	.....	 82	.....	.....	.....
24	.....	.....	.....	..... †	.....	..... 79½
Oct.						
2	.....	.....	.....	 3	.....	..... 78½
3	.....	.....	.....	 4	.....	..... 78½
11	.....	.....	.....	 2	.....	..... 75½
12	 9	.....	 75	... .	.....	.....
30	.....	.....	.....	..... M.	.....	 64
Nov.						
1	.....	.....	.....	..... M.	.....	 64
10	.....	.....	.....	..... M.	.....	 62
Dec.						
22	..... *	 56	..... 46½	.....	.....	.....
Jan. 1850.						
5	 9	.....	 44	.....	.....	.....
Feb.						
17	..... 11½	 55	 47	.....	.....	.....
18	.....	 52	 47	.....	.....	.....

* Sunrise, unless the contrary be indicated. † Sunset, unless differently noted.

1850. UPWARD VOYAGE TO CINCINNATI.*

April.	Air.	River.	Hour.	Place.
26		65½°	6 p.m.	New Orleans.
27	74½°	64½	8 a.m.	Baton Rouge, La.
		63	3½ p.m.	Above Red River.
28	59	61½	6 a.m.	30 miles below Vicksburg.
	59½	58½	8 a.m.	near Vicksburg.
	64	56½	6 p.m.	30 miles above Providence.
29	53	60	Sunrise	Just below Arkansas river.
		57		300 rods above Arkansas river.
		56½	10 a.m.	20 m. above, and half a mile above [White river.
		57	Noon	
		57	4 p.m.	St. Helena.
30	65	57½	7 a.m.	50 miles above Memphis.
		57½	10½ a.m.	25 miles above Randolph.
		58	2 p.m.	120 miles below mouth of the Ohio
May.				
1	44½	60	6 a.m.	Ohio, 20 miles above its mouth.
	48	57½	7 a.m.	Metropolis.
		60	8½ a.m.	Mouth of the Tennessee river.
		60	10 a.m.	Mouth of the Cumberland river.
2	44	56¼	6 a.m.	25 miles above Green river.
		57	8 a.m.	
		57	11 a.m.	100 miles below Louisville.
		58½	4 p.m.	Brandenburg.
3	57	59	Sunrise	Louisville.
6		59	2 p.m.	Cincinnati.
11	55½	57	11 a.m.	Cincinnati.

May. DOWNWARD VOYAGE TO NEW ORLEANS.

11		57		Near the mouth of Kentucky riv.
		58	5½ p.m.	In the mouth of Kentucky river.
12		57	10 a.m.	Louisville.
13		58½	4 p.m.	Below Louisville.
14	53½	59	8 a.m.	20 rods above Green river.
		57½		20 rods below mouth of Green riv.
15	58	59	8 a.m.	Mill's Point.
16	66	61	9 a.m.	30 miles above Memphis.
		62½	4½ p.m.	St. Helena.
17	61	62½	Sunrise	100 miles above Vicksburg.
		63	Noon	Near Vicksburg.
	76	64½	Sunset	70 miles below Vicksburg.
18	66	66	Sunrise	150 miles below Vicksburg.
19		67	do	New Orleans.

* This voyage was made in order to attend the Meeting of the American Medical Association, which assembled at Cincinnati, in May, 1850.

THERMOLOGICAL MAXIMA, MINIMA, AND MEANS OF THE RIVER FOR SEVEN YEARS:

Years.	Maxim.	Minim.	Mean.
1845–6.....	85.5°	40°	62.5°
1846–7.....	86.75	39.5	63.25
1847–8.....	86.5	42	64.25
1848–9.....	83.85	43.5	63.5
1849–50.....	83-5	44	63.75
1850–1.....	89	35.5	62.25
1851–2.....	84		
General Means.	85.75	40.26	63.147

The diurnal maximum and minimum of the river which scarcely differ in any appreciable degree, form a strong contrast in these respects with the changeful alterations of the air. The nearest approximation between the air and water occurs at sunrise, or in the night. The horary change in the river is imperceptible—in the air marked, sometimes enormous. Neither the minimum nor maximum in the former is by any means as great as in the latter. Yet notwithstanding the differences in maximum, minimum, horary, diurnal and annual, yet the annual mean of both, it is confidently believed, will be found nearly the same.

Any marked diurnal or weekly change in the river, which, however, is extremely limited, is not so much owing to atmospheric changes as to the sudden rises and deliveries of its tributaries flowing from different latitudes, and, consequently, having different temperatures, examples of which will be seen in the above meteorological table of the upper and downward voyage on the Mississippi and Ohio rivers, in 1850, during high water.

Humboldt says, that for 27° latitude on both sides of the equator, the temperature of the equinoctial seas "is almost entirely independent of the atmosphere," being 83° to 84°, while the mean of the

equatorial air is 79° to 80°. This "independence of the atmosphere" in regard to its influence on the river, is equally striking when tested hourly, or daily; but it is far different as regards the hot and cold seasons.

It may be allowable to glance at a few isolated data illustrative of the thermology of some of the rivers, which data, however, do not extend over periods of time sufficiently long to be satisfactory or characteristic.

Temperature of the St. Lawrence, at Montreal, in July: Mr. E. S. de Rottermund says, (*Brit. Am. Jour. Med. Sci.*, July 29, 1845,) that the St. Lawrence and Ottowa which run together without intermingling for several leagues, have a temperature of 66°, the air being 82°.

"In the temperate zone," says Humboldt, "in Europe, the Danube and the Elbe, lat. 48°–49° F., attain in the middle of summer only from 17° to 19° c. (63° to 66° F.) the air being in the hottest month 18–19 5° c. (65° to 67° F. (Narrative VI. 588.)

Lieut. Lynch, U. S. N., who explored the river Jordan and the Dead Sea, in 1848, says that the width of the river in the lower part of its course, averages fifty-six yards, and its depth four feet. On the 15th of April, it appears from his Journal, that the air rose eight degrees, and the river three degrees. In the same month he found the sea of Galilee ranging from 70° to 90°, according to depth.

The Dead Sea (less than ten miles wide) was heated during a sirocco, on the 26th of April, to 80°, and two days after to 82°, though on the 4th of May, a week later, the surface was 76° only. The air of the sirocco rose from 102° to 106° "blistering the eyelids." At Jericho, the river, at half-past eleven o'clock, A. M., was 74°, the air 82°. The Jordan, excepting its hallowed historical associations, is one of the most ill arranged and worthless of streams, abounding in rapids, cataracts, shoals, whirlpools, rocks, angles, and curves; its tortuousness is almost four times as great as that of the lower Mississippi, as in two hundred miles it traverses but sixty in a straight line. Such diminutive seas and tiny rivers, become heated by the hourly changes of the air, and can have little hygienic influence.

Lieut. Lynch says that the most powerful microscope can detect no animalcules in the water of the Dead Sea. The same, I believe, is true of the running water of the lower Mississippi. But the cause is different in each case. The former is truly a *mare mortuum*, having no living

animals, and being positively deleterious to organic life, its mineral and saline ingredients amounting to one part in four. The absence of animalcular organisms in the lower Mississippi, is probably owing, either to its purity notwithstanding the quantity of its suspended detritus, or to the opacity of its running water, whereby light is intercepted; or, perhaps, a cause may be found in the absence of microscopic plants or other food necessary to these animals, a peculiarity which does not obtain in the upper rivers. Prof. Drake quotes Prof. Bailey's researches, showing that "the river at St. Louis contains numerous shoals of microscopic animals," twenty species of which the latter found in great numbers. (*Valley Miss.* I. 71-2.)

Mollusks or shell-fish, at least such as inhabit the upper rivers, are not found, I believe, in the lower Mississippi. This, if true, is a fact that has not been, so far as I know, alluded to, much less explained by naturalists. The absence of the food of these animals, as the algæ or *confervæ* and other fresh-water plants and infusorial animals, or the want of a firm bottom of earth, sand, or pebbles to rest on, and prevent suffocation in the fine mud, or the turbidity of the water which arrests the transmission of light, or all of these combined, may serve to explain the non-existence of these bivalves, etc., affording, at the same time, a presumptive proof that the river is less productive of plants, infusoria, etc., than the upper shoal rivers, and consequently purer and more salubrious.* Such a course of hygienic reasoning may be far from being demonstrative, but it is at least suggestive.

The Gulf of Mexico, particularly the Gulf stream, has been represented by several authors as being from four to eight degrees higher than that of the ocean in the same latitudes. The latest estimates give the Atlantic, Pacific and Indian oceans a general or mean maximum of at least eighty-eight degrees for a distance of twenty-eight parallels from the equator, with a mean maximum of the air of eighty-two degrees. These high temperatures in vast bodies of water, reaching the littoral of the delta, have necessarily great influence upon the climate, in connection with the south wind, and would probably, but

* While writing this paragraph, a commercial gentleman, from Boston, long resident in New Orleans, whose acquaintance I made during his attack of yellow fever, in 1853, called to consult me concerning his health, and, during some general conversation, remarked that the captains of ships had frequently informed him that they always preferred the water of the Mississippi to any at New York or Boston, in sea voyages. A similar statement has often been made to myself by this same class of practical men.

for the river, make Louisiana, if not a land of siroccos, yet one of extraordinary heat.

The humidity of lower Louisiana, supposed to originate from the river, its crevasses and vast inundations, has not been proved to be the cause of any epidemic or malady usually attributed to that source, that is, malaria. The ætiological theory of humidity which of late has been invoked to account for almost all epidemic and endemic morbidity, is wholly unproved. A few years ago, Prof. Casper, of Berlin, wrote a work on medical statistics, in which, from extensive data, he concludes that humidity, not dryness of the atmosphere, is most favorable to life, and that no state of the air is so prejudicial to health as that of dryness with cold. (*B. and F. Med. Ch. Rev.*)

The statistics of the New Orleans Charity Hospital, at first sight, indicate the prevalence of intermittent fevers in the city. But that institution receives of this class of patients a large majority from hundreds of steamboats, flatboats, and rafts which navigate the Missisippi and its tributaries, from lake and coasting vessels, and from steamships from Central America. The extreme rear is the only portion of the city where intermittents prevail to a considerable extent, being chiefly among the unacclimated. The unacclimated, nevertheless, are equally liable to yellow fever, whether resident on the levee, or in the central or marshy regions of the city. While some physicians regard the recrementitious matters and drainage of the city which accumulate upon its swampy margin as null in the causation of yellow fever, or even as preventive of that disease, yet there can be no doubt as to the greater prevalence of intermittents (generally mild) in this locality, than in any other in the city, nay, greater than among the most extensive and primitive swamps which expand from the terminations of the double-inclined planes forming the coasts of the lower Mississippi. The supposed malarious banks of the river are, on the contrary, its *sanitaria*, affording the hygienic advantages which Hippocrates seeks for in his book on "airs, waters and places," with which he combines other terrene influences, together with the celestial, as the *Dog-star*, *Arcturus*, *Pleiades*, and morbidity of the humors, and also, constitutions of the year. He prefers town situations where the water is good, and where the air is neither too hot nor too cold: "All the productions of such a place are better than those of a different position,

and the place may be said to enjoy a perpetual spring in consequence of the mild temperature of the air."

The following Hippocratic tableau drawn more than two thousand years ago, by the sage of Cos, (an island of the Ægean where he was born,) applies in many respects to numerous places under similar latitudes from the Chesapeake Bay to the declivities of the Rocky Mountains, and particularly to the tributaries of the upper Mississippi, while the banks of the lower river are less involved in this *sombre* picture, epidemic yellow fever excepted.

"The waters of *marshes*, those of lakes, and, in general, all those that are stagnant, are necessarily warm, thick, and, in summer, offensive, because they are stagnant; they always receive new rains, and are heated by the sun; this is the reason of their being of a yellowish white, bad and bilious. In winter they are cold, frozen and turbid, as much owing to the snows as to the rains; hence, they are thick and pituitous, and those who use them have obstructed and enlarged spleens, the belly hard, bound and hot, the shoulders, clavicles, and visage lean, for the flesh dissolves and is received into the spleen; this is the reason they are meagre and lean. It thence follows, also, that they almost always experience the sense of hunger and that of thirst, and that they have the superior and inferior bellies very dry and warm; they require strong purges; the above affections do not leave them neither in summer nor in winter, and the greater part of them perish with dropsy. In these places, in summer, dysenteries are prevalent, also fluxes of the belly and very tedious quartan fevers; consequently, when these diseases continue long, they terminate in dropsy, and those attacked with it generally die."

After having sent the above remarks to press, I read a portion of the proofs to a friend, who called my attention to the work of the late lamented Herndon, on the Amazon river, a work I had never read; I immediately made inquiry for it, and upon consulting its pages found an additional verification of the opinions I had formed of the hygienic influences of that great analogue of the Mississippi, which has no rival in the whole temperate zone.

The following data from Lieut. Herndon's (U. S. N.) *Exploration of the Valley of the Amazon*, (pp. 414, 8vo. Washington: 1854) made under the authority of the Secretary of the Navy, deserve the attention of inquirers into medical topography, hygiene and ætiology.

14th of December, 1851, at ten o'clock, A. M. The air, the river Amazon, and the river Jurua, a mile above its junction with the latter, gave each eighty-two degrees. Four days later, one hundred and five miles lower, on reaching the mouth of the Japura, a tributary of the Amazon flowing nearly under the equator, he says, that to his surprize, he found its temperature to be eighty-five degrees. The Amazon, a quarter of an hour later, was at eighty-one degrees. The explanation is doubtlessly the same as that already indicated in the above table of a voyage on the Mississippi, in 1850, namely, a river flowing through a comparatively warm region, has a higher temperature (other things, such as volume, depth, velocity being equal) than one from a cold region.

Among the Upper rivers, tributaries of the Amazon, Lieut. Herndon begins to note the prevalence of intermittent fevers which he always denominates tertian or tertianas.

When at Santarem on the lower Amazon, he visited Mr. G., three miles from town, whereupon he says, "I supposed that this house, situated in the midst of a cocoa plantation, on low land, near the junction of two great rivers, under a tropical sun, would be an unhealthy residence; but I was assured there was no sickness here. (323.) We drifted with the current all night, and stopped at a small cocoa plantation belonging to some one in Santarem. The water of the river was nearly up to the door of the house, and the country seemed to be all marsh behind. I never saw a more desolate sickly looking place; but a man who was living there with his wife and six children (all strong and healthy-looking) told me they were never sick." Of a town ninety miles below Santarem, he says, "We saw at this place several persons who were suffering from tertianas, but all said they took them whilst up the neighboring rivers. If general accounts are to be relied on, there seems to be really no sickness on *the main trunk of the Amazon but only on the tributaries. I have no doubt of the fact that sickness is more often taken on the tributaries than on the main trunk.* * * *

"Para was a remarkably healthy place and entirely free from epidemics of any kind, until February, 1850, when the yellow fever was taken there by a vessel from Pernambuco. The greatest malignancy of the disease was in the month of April, when it carried off

from twenty to twenty-five a day. About the same time the next year (the fever being much diminished) the small-pox broke out. About twenty-five per cent. of the population died."

Of the lower Amazon he says, "the climate of this country is salubrious, and the temperature agreeable. I met with no epidemics on my route, except at Para; the country seemed a stranger to yellow fever, small pox, or cholera. There seemed to be a narrow belt of country on each side of the Amazon, where bilious fevers were particularly prevalent. These fevers are of malignant type, and often terminate in fatal jaundice. I was told that six or eight days' navigation on each tributary, from the mouth upwards, would bring me to this country, and three or four more would pass me through it; and that I ran little risk of taking the fever if I passed directly through." (369, 370.)

Here is a river not only the largest in the world, but altogether southern—a river which in some places expands to the width of one hundred and fifty miles (far above its mouth)—which inundates the vast marshes of the torrid zone for hundreds of miles beyond its shores —which has upon its banks just enough of the Indian, European and African races to test its hygienic influences, and yet it is almost wholly destitute of the so called malarious fevers, while those pasture lands called the Pontine Marshes, near Rome, and the most contemptible mill-ponds and creeks of the upper valley of the Mississippi are charged with the causation of deadly epidemics and endemics.

Gangetic hydrography is analogous to that of the lower Mississippi in its hygienic character. The celebrated Heber, Lord Bishop of Calcutta whose voluminous travels in India, show him to have been a very careful observer, (a believer in the doctrine of marsh poison,) says that the lower Ganges is far more salubrious than the regions of its tributaries. After much travelling through the southern districts, he says that he observed no sickness until he had passed central or northern Hindostan, and had arrived near the Himalaya mountains. A very sickly district, which he calls the Belt of Death, lies between Bareily (Central India) and Almora, where it seemed that the annual ague and fever took away all energy from the people, causing tumid bellies, slender limbs, sallow complexions, killing very slowly, and sometimes assuming the form of typhus. (I. *chap.* 17.) Almora,

according to the latest authorities, is 5,337 feet above the sea, being situated on a ridge.

"The country around Calcutta is a perfect flat alluvial plain, intersected by pools, canals natural and artificial, and innumerable arms of the Ganges, teeming with a population like an ant-hill, scarcely to be paralleled in China, and like all the rest of Bengal, overflowed annually by the river; the only plague, and a sore one too, are the mosquitoes." (II. 203, 220, 223.) Later researches show that the sickly belt is not along the outlets of the river, which occupy two hundred miles, but in the interior, that is, higher up the stream.

The topography of Natchez, on the Mississippi, furnishes an illustration of some remarks above mentioned, concerning the low lands on the river banks. Natchez, which is perched on a hill, has been, at intervals, severely visited by yellow fever during forty years, while the subordinate or secondary town called Natchez-under-the hill, has either escaped or suffered comparatively little. Some writers maintain that this exemption is owing to the absorption of the yellow fever miasm by the river, which washes and often inundates the alluvial foundations of the lower town which is the wharf or landing of the city. That portion of the miasm which is supposed to escape the hygienic action of the river, climbs nearly two hundred feet above, and, according to this theory, affects the upper town or city proper. Such was the explanation of the first epidemic, 1817, not to name others. In his account of the epidemic of Natchez, in 1823, Dr. Cartwright says that the inhabitants of the low lands were healthy, and that such lands afforded a safe retreat from the epidemic. (*Am. Med. Recorder*, IX.)

The late Professor Drake, in his voluminous work on the Diseases of the Valley of the Mississippi, maintains that the turbid water fresh from this river "produces some effects on this system, which transparent water from wells and springs and our other rivers does not, being to a certain extent *medicinal*, and especially adapted to the cure of chronic functional disorder of the stomach, bowels and liver. That its daily use *averts* some forms of disease may be admitted as probable." These opinions, the result of much travel, observation and inquiry, and universally fortified by the experience of the residents upon its banks, and the boatmen on its stream, had been promulgated by writers on the sanitary condition of Louisiana during last century.

and are now alluded to as at least favoring the doctrine of the salubrity of the water for common use, even though it be neither medicinal nor preventive of epidemic or endemic diseases.

The waters originating in the vast carboniferous formation, particularly in the carboniferous limestone of the upper rivers, contain, without doubt, calcareous and other foreign matters, to a greater degree than can be found in the lower portion of the Mississippi, excepting suspended silica, which latter amounts to about three-fourths of the whole of its detrital matter.

The mud of the delta, taken from the river bank, and dried by a heat of two hundred and twelve degrees, gives only, according to Professor Riddell, a small fraction over two parts of lime in the one hundred, while twelve hundred miles higher up, according to Dr. Drake, the proportion is about four times as much. According to M. Regnault, the Nilotic mud contains eighteen parts of carbonate of lime—an enormous amount as compared with that of the lower Mississippi.

Professor Riddell's analysis:

Silica	74.15	Phosphoric Acid	0.44
Alumina	9.14	Sulph	0.07
Oxide of Iron	4.56	Carb. Acid	0.74
Lime	2.08	Chlorine	0.01
Magnesia	1.52	Water	8.12
Manganese	0.04	Organic Matter	3.10
Potassa not determined	0.00	Loss	1.03
Soda	0.00		
Total			100.00

The solid matter by weight is as 1 to 1153 of the water. (Drake I. 74-5.) The water drinkers of New Orleans who use hydrant water, drink a good deal of flint.

In the great work, *Description de L'Égypt*, M. Regnault, one of the *savants* of the French Expedition (1799 to 1801) gives the following analysis of the Nilotic mud: In one hundred parts: water 11; carbon 9; oxide of iron 6; silex 4; carbonate of magnesia 4; carbonate of lime 18; alumina 48. (T. II. p. 406. Paris; 1813.)

The infusorial animals which generate in the reservoir of the water works, when the water is allowed to stagnate, are probably derived from ova in the air, or from ova which have been carried down from the upper rivers, and which, owing to the want of light or other cause, do not develop themselves until the water becomes stagnant, partially clear, and elevated in temperature.

The following correspondence occasioned by an unusual offensiveness in the hydrant water of the city a few years ago, will serve to illustrate this topic:

"Street Commissioner's Office,
New Orleans, Sept. 23, 1854.

"*Sir:* On the suggestion of the Mayor, you are herewith furnished with three samples of water, viz: One taken out of the river at the point where the suction pipe of the Commercial Water Works actually obtains its supply; one from the surface of the river, immediately above the point last mentioned; and one from the compartment of the reservoir now being discharged into the mains supplying the hydrants.

"You will please analyze said samples, and report whether or not you find in either of them any foreign substances which may, from the use of the water, be injurious to health.

"This inquiry has been deemed advisable, because many persons are fearful that said water has been by some means corrupted.

"You will be furnished, as soon as it can be obtained, with samples of the river water, taken from various depths and places, in order that this investigation may be satisfactorily entertained.

"You are requested to furnish, without delay, such pertinent remarks as you can arrive at, pending the progress of your experiments.

"Your obedient servant,

"Aug. S. Phelps, *Street Commissioner.*

"Dr. J. L. Riddell, Professor of Chemistry, University of Louisiana."

"Louisiana University, Medical Department,
New Orleans, Sept. 23, 1854.

"*Sir:* In compliance with your wishes, I herewith furnish you with the crude results and conclusions which this day's labor has enabled me to arrive at.

"1. The river water obtained this day from the source of the Water Works, after having been carefully filtered from suspended mechanical matters, yields upon evaporation to dryness, one part of residue to two thousand parts by weight of water. This is about five times as great as what trials made years ago induced me to fix upon as the normal amount, (one part in ten thousand.) Of this residue, one to two thousand, about four-fifths is saline mineral matter, which ought to show some slight effects upon persons using the water. About one-fifth of the residue is organic matter, of animal and vegetable origin, either in solution or so finely comminuted as to pass through a fine filter.

"2. The sample of water obtained from the Water Works reservoir, proved, upon examination with the microscope, to contain countless myriads of green, cylindrical, living and moving filaments, each about one-three thousandth of an inch in thickness and of variable

length. They belong to an obscure race of organisms, known to naturalists under the technical name *oscillatoria*. I have been informed by the Superintendent of the Water Works that this reservoir water has been distributed to the city only during the night and early morning; at other times the city is supplied directly from the river. Now I am of opinion that the water swarming with *oscillatoria* might produce unpleasant results with persons drinking it. To develop them, the concurrence of three conditions is requisite, namely: organic materials, a high temperature, (75 or 80° F.,) and stagnant water, more or less limpid, so as to be penetrable by light.

"3. I have found as yet no special cause of disease in the samples of water furnished, excepting that taken from the reservoir, as before mentioned.

"In conclusion, I am of opinion that if, during the continuance of hot weather and low water in the river, the Commercial Water Works will be careful to supply the city directly from the river; or if through the reservoir they be careful not to allow it to stagnate there, there will then be no reason to apprehend any bad results from its use.

"Respectfully, your obedient servant,

"J. L. RIDDELL.

"A. S. Phelps, Esq., Street Commissioner."—*N. O. Picayune.*

The general and paramount importance of hygienic hydrology, justifies another quotation in relation to a region of the Republic remote from the "Father of Waters:"

"*On the Composition of the Water of the Delaware River:* by Henry Wurtz, New Jersey State Chemist, etc.—The water of the Delaware which was submitted to analysis, was collected from the river on the 11th day of September last, at a spot opposite the pump-house of the Water Works, and immediately over the grating through which the water is drawn to supply the reservoir. A specimen was also obtained upon the same day, of the water in the reservoir, for the purpose of comparing the portion of foreign ingredients in the reservoir water, with that in the river water. The spring water analyzed was obtained some days later, from one of the tanks from which the city was formerly supplied, in the rear of the residence of Mr. Closson.

"The proportion of solid matter found in the several waters was as follows:

	Grains.
In one gallon of Delaware water	3·5346
In one gallon of the reservoir water	3·8555
In one gallon of the spring water	3·6077

"For convenience of comparison, I will here quote the results of Professor B. Silliman, Jr., obtained some years ago with the Croton and Schuylkill waters.

"Solid matter found by Prof. Silliman:

	Grains.
In one gallon of Croton water	10·93
In one gallon of Schuylkill water	5·50

"The most remarkable thing about these results is that notwithstanding the quantity of sand, mud, and other sediment which is suspended in the river water so much as to injure the pumps, and which must be in great measure deposited and separated from the water in the reservoir, the latter nevertheless actually contains more solid matter than the river water itself. This can only be accounted for by the favorable conditions presented in the reservoir for the growth of minute animals and plants, whose remains add of course to the weight of the solid residue obtained on evaporation. My pupil, Mr. Howland Bill, has at my request submitted the water in the reservoir, and the deposit formed at the bottom, to a microscopic examination, and reports to me that he finds in the water several varieties of animalcules and lichens or minute plants, and that the sediment especially is almost wholly composed of forests of minute plants through which roam herds of such animals as *Volvox globator*, or "globejelly," *Vibrio anser*, or "goose animalcule," and several species of *Baccillaria* and *Navicula*. On the surface of the water he found a slight green scum, which when magnified resolved itself into collections of the *Cercaria mutabilis*, an animal production characteristic of stagnant water. Numerous large green water weeds may also be seen floating in the reservoir.

"Recurring to the results given above, it may also be remarked that the river water is really somewhat less charged with foreign ingredients than that of the springs, although the latter is so much more pleasant to persons possessing delicate organs of taste. This probably arises from the fact that the principal mineral ingredient in spring water, as shown by the analysis, is *chloride of sodium* or common salt, while the river water is principally contaminated with carbonates of lime, magnesia, potash, etc., which give water a bitter taste. * * * *

"On comparison of this analysis* of the waters of the Delaware with other analyses of river waters, the fact is rendered apparent that few rivers exist whose waters are so free from impurity. All causes of complaint which have arisen are due to the improper mode of storing the water for use. Open reservoirs, in which the water is kept standing for several days to stagnate in the heat of the sun, are perfect hotbeds for the growth of animal and vegetable life. Finding every necessary requisite to their germination, light, heat, and an unlimited supply of fertilizing mineral substances, phosphates, sulphates, carbonates and silicates of lime, potash, ammonia, etc., infinite numbers of minute seeds spring forth into growing plants, which in their turn furnish nourishment to innumerable swarms of living animals engendered from their embryos preëxistent in the water. The breeding of these microscopic creatures, under favorable circumstances, is so rapid that in a very few hours the water will become alive with them." *Am. Jour. of Sciences and Arts.*

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* The tabulated analysis of the river water, together with its comparison to that of spring water, is omitted. The carbonate of lime abounds in the former—the chloride of sodium in the latter.

The velocity of the Mississippi seems to have been for countless ages nearly the same. Although great elevations and depressions must have occurred throughout the delta, yet these have been so gradual, and have alternated with such uniformity as to be mere repetitions. The alluvial deposit as far as ascertained (nearly 700 feet) is without coarse sand, gravel, stones, boulders or other *débris* which a strong current pushes forward towards the sea. A swift descent of a great perpendicular volume of water moves rocks as large as a house, a diminished one boulders, gravel, pebbles, coarse sand, fine sand, etc. The alluvium of the delta is virtually an impalbable powder held in suspension by a current far less rapid than is generally supposed. This deposited powder forms the low lands of the delta to an unknown depth, being probably not less than one thousand feet in thickness.

Professor Robins says, that when the current at the bottom of a river moves three inches in a second, it will separate and lift up fine clay—six inches per second, clay—eight inches per second, coarse sand—twelve inches per second, fine gravel—twenty-four inches per second, gravel one inch in diameter—thirty-six inches per second, angular stones the size of a hen's egg. Now, taking for granted the usual but the exaggerated estimate of the velocity of the river at New Orleans, during high water, it would be eighteen inches per second, which, if the same at the bottom would transport large gravel.

Hundreds of observations by Professor Forshey are thus summed up:

"The result shows a mean surface velocity at high water of 2.61 miles per hour, at Carrollton, (six miles above New Orleans,) and 2.60 miles per hour at Vidalia, (nearly three hundred miles above New Orleans,) respectively, 3.80 and 3.82 feet per second of time. The one was derived from 176 observations, and the other from 70 observations. At low water, Carrollton, 1.45 miles per hour, or 2.11 feet per second; at Vidalia, 1.54 miles per hour, or 2.25 feet per second."

The difference between the depth of the river at high and low water (twelve to fourteen feet) is comparatively small in a river whose depth is from one hundred and twenty-five to two hundred feet at all seasons. It is contrary to the hyrodynamics of rivers, to suppose with some writers that the whole volume of water from the central surface to the bottom and sides should move with an uniform velocity. If the mean elevation of the principal tributaries of the Mississippi (including, of course, the Missouri) be estimated, the accelerating

velocity which a body of water would acquire in falling from this elevation to a level with New Orleans, would be almost inconceivable—probably less than that of a shot fired from a cannon, instead of being as at present two miles and a fraction per hour. The force of gravity though uniform in a falling, unobstructed body, is in proportion to the time, but accumulates every moment, accelerating the motion as compared to any degree of the preceding, and this continuous generation proceeds *ad infinitum.* But in falling down an inclined plane, this water parts with its velocity every moment, by friction against air, rocks, banks, curves, the bottom of the channel, and other resisting media.

The Mississippi, which meanders for nearly six hundred miles in Louisiana, being guarded against by eight hundred miles of levees, presents even at low water, a surface of several hundred square miles, which, for good or evil, is an element of study of rare value in medical hydrography.

In contributing some thermological data to the physical history of the lower Mississippi, accompanied with conjectures concerning its possible hygiene, I hope to direct the attention of abler observers to the great feature of our medical topography and commercial prosperity. The influences of crevasses and inundations in both urban and rural districts, deserve careful observation and study, in ætiological, hygienic and sanitarian points of view.

The crevasse nearly opposite to the upper boundary of New Orleans which has existed many weeks, may be alluded to in conclusion.

The Bell crevasse a mile above New Orleans on the opposite side of the river, now inundates the towns of Gretna, McDonoghville, and Algiers, opposite the city, and many plantations below. Other crevasses above the former for many hundred miles, have desolated plantations and drowned domestic and wild animals in great numbers. These crevasses, and the natural outlets other than the mouth of the river, have spread a sheet of turbid waters over the delta, probably larger than the united areas of the States of Delaware, Rhode Island and Connecticut. These waters gathered from one thousand seven hundred navigable rivers, and from every kind of geological formation, alluvial plain, diluvial plateau, tertiary, carboniferous and palæözoïc, have ploughed their way into the great seaward trough,

loaded with *débris* from silicious, cretaceous, calcereous, galeniferous, and coal regions; from

> "Many an ancient river,
> From many a verdant plain;"

and from more parallels of latitude than any other river on the globe.

If existing ætiological theories concerning disturbances of the soil, moisture, inundations, swamp exhalations, etc., be applicable to the lower Mississippi, the present season will be an eventful one, for doctors at least. The impending danger is lessened, however, by past experience which has not coïncided with these theories.

The increase of population in New Orleans, and Louisiana at large, has not been equal to that of New York, owing as has been generally supposed, to the prevalence of yellow fever in the valley of the lower Mississippi. But it may be safely affirmed, that if yellow fever were never more to visit New Orleans, the numerical advancement of the city, great as its natural advantages are, can never be rapid so long as it is a purely commercial emporium, destitute of manufactories, shipping, importation, and the industrial fixed elements of capital other than the raw material, and the mere profits arising from its exportation. The money or circulating capital of the city is comparatively large, while its fixed capital, such as workshops, machinery, and home fabrics is small. The unfinished material preponderates over the finished. This was the case before yellow fever invaded the city, in 1796, and would continue after the disappearance of this malady in the absence of manufactories, ships, importations, etc. If an overgrown or large population be desirable, the most favorable sanitary condition will not of itself accomplish that object.

The causes of the great epidemics of plague, influenza, cholera, dengue, dysentery, erysipelas, black-death, sweating sickness, typhus, whooping-cough, ophthalmia, scarlatina, yellow fever, etc., which have swept over the world at irregular periods, have not yet been explained in a satisfactory manner; nevertheless, many useful facts and special conditions tending to restrict or modify their action, are not only known, but may often be controlled. Among the hygienic influences which tend to avert or mitigate these calamitous events and prolong life, air, temperature, locality, and water are paramount.

This article is already a long one, but not being so long as the Mississippi, perhaps, the patient reader will allow a few additional observations, not originally intended by the writer.

Of the group of diseases attributed to malaria or marsh poison, intermittent is the fundamental type. The late Dr. Forry who wrote a systematic treatise on the medical climate of the United States which he based on the army statistics, represents the banks of the lower Mississippi as the special region of malaria. This malaria, he says, "affects the natives with a slow and concealed combustion—the unacclimated with a raging and rapidly consuming flame. It is here that the Angel of Death makes his most desolating visitations." The army statistics to which he, and later writers refer, prove just the contrary; the posts on the lower Mississippi being, with the exception of yellow fever, among the most healthy, particularly in reference to intermittent fevers, the coasts of New England excepted. Thus the entire southern compared with the middle division gives a ratio of this class, only about half as high as the lake and western divisions, while the banks of the lower river give the minimum as compared with the residue of the South itself.

Dr. Forry, and others, aver that at Fort Pike, the New Orleans Barracks, and at Baton Rouge, "the average of fevers of a malarial origin is very low." Surgeon B. F. Harney says of the Baton Rouge Barracks, that "no endemic disease has ever prevailed here." (*Med. Stat. U. S. A*, 1856, p. 254.) According to official reports, Fort Pike, thirty-four miles from New Orleans, having an elevation of two feet only, "has *no miasmata*. The soil is fertile and intersected with tortuous bayous resembling artificial canals." (*Ib.*, 255.) Former army reports show that the mortality at this station is as low, perhaps lower, than at any other in the United States, being with the exception of casualties but a little over one per cent.

Seeing, in the Southern posts, as Key West, Charleston, New Orleans, Fort Pike, Fort Jessup, and Baton Rouge, that intermittent fevers are, according to the army reports, less prevalent than in the middle division, and more particularly less than in the lake and upper Mississippi regions, some of the army surgeons and others have endeavored to explain these discrepancies between their figures and their theories—between their parallels of latitude and their latitude of logic, taking for granted all the while, "that diseases of a mala-

rial origin increase," to use the language of the Surgeon General, "in proportion as southern latitudes are reached; but this law," as they say, "receives modification in this, that inland regions, contrasted with those on the coast in the same latitude, have, *owing doubtless to the greater summer heats, a higher average.* The region of the *lakes gives the highest ratio; the posts remote from the ocean and the great lakes, also give a high average; but this is owing chiefly to locality and latitude,*" etc. (*Stat. Rep. U. S. A.*, 329.) "In the southern division, the ratio of catarrhal diseases is found the lowest." (338.) Dr. Forry maintained that the army statistics proved that diarrhœa and dysentery are more prevalent in the middle than in the southern divisions of the United States.

The medical statistics of the army of the United States based on a small number of troops, mostly of foreign birth, of irregular habits and unacclimated, and who seldom remain an entire year at the same post, cannot, however, be regarded as affording conclusive evidence of the inherent salubrity or insalubrity of a climate or civic population.

Within the last ten years, I have, in this Journal and elsewhere, published several papers showing by the most reliable documents, records and travels, both official and private, that until the close of last century, when yellow fever made its appearance, Louisiana was salubrious to an extraordinary degree. I will now offer the testimony of the late Mr. Darby, concerning the healthfulness of the country from the middle of the first to the middle of the second decennium of the present century. His work though old and imperfect, is still the best on Louisiana. His great talent for observation and his practical pursuits as public surveyor, qualified him to estimate the medical topography of the country.

"During almost ten years," says Mr. Darby, "I endured all that summer's sun and winter's wind, could inflict. Between the 9th of July, 1805, and the 7th of May, 1815, incredible as it may appear, I actually travelled upwards of twenty thousand miles, mostly on foot, between the Mobile Bay and Sabine River, and from the Gulf of Mexico to thirty-three degrees north latitude. During the whole of this period I was not confined one month, put all my indispositions together; and not one moment by any malady attributable to the climate. I have slept in the open air in some of the hottest summer

nights for weeks together, and endured this mode of life in the most matted woods, perhaps, in the world. During my survey of the Sabine River, myself and the men that attended me, existed for several weeks on flesh and fish, without bread or salt, and without sickness of any kind. (280.)

"Those places condemned to annual submersion, are a great part of the year extremely deficient in water. Often the same lake, that in April, May and June, contains ten feet depth of water presents in the fall season, an extensive meadow of succulent herbage, or a hard, dry, stiff surface. The region along the sea-shore is the real marsh. Giving credence to the correctness of the general belief in the evil effects arising from the putrefaction of animal matter, an everlasting pestilence ought to range along the Gulf of Mexico, and depopulate every spot in it. The millions of testaceous fish, that annually die and rot on this coast, are numerous beyond all calculation. The most noisome effluvia must continually arise from such a mass of putridity; yet we find but few destructive consequences arising from a cause, that would, to all human appearance, carry death on eagle wing." (252-3, 279-80.)

Mr. Darby says, "no part of his book was so much criticised as that in which he advocated the salubrity of Louisiana;" but he adds, "I could not write otherwise without doing violence to my own experience."

"If malarious diseases increase, as southern latitudes are reached," what intensity must they attain in the latitude of New Orleans and the Balize, judging from the following faint outline of a most gloomy tableau, too extended to find a place in this Journal. Assistant Surgeon J. K. Barnes, thus delineates the medical topography and diseases of Fort Scott: Lat. 38° N., Long. 17° 30′ W. of Washington, four miles west of the Missouri line, one hundred and ninety-five miles from the Missouri river; high undulating limestone table prairie; well wooded water-courses; quarters exceedingly roomy, well ventilated and comfortable; good drainage; an accurate examination of the country for several miles in each direction, has failed to discover any local feature which may be considered objectionable, or as remotely the cause of disease; no grass ponds, swamps, or lakes; streams numerous but without stagnant pools. The records show a great proportion of malarious fevers, so intense that no one escaped, no pre-

cautions were of avail; the youngest infant, as well as the most robust adult; hundreds of young cattle, also, horses, sickened and died. The troops were little exposed. We must look to other causes for an explanation, etc., as cryptogamous growths, etc. From May 1842 to January, 1849, there were 3,415 cases in a command of 3,034; of these 1,717 were malarious fevers, or more than one third per cent. over all other disease, after deducting surgical cases. (*Med. Stat. U. S. A.*, p. 158, *et seq.*, 1856.)

The Hindoos worship the sacred Ganges, and the Egyptians adore the benificent Nile, while certain sanitarians and epidemic expounders, less devout, inaugurate a myth on the banks or the lower Mississppi, namely, "the Angel of Death," whose power increases "in proportion as southern latitudes are reached."

ART. V.—*A Natural History of Dogs:* By J. C. NOTT, M. D., Mobile.

HISTORY, or even tradition, approaches no epoch in man's history, at which he was not, in the old world, surrounded by the same domestic animals, as at the present day, and all the efforts which have been made by historians and naturalists to penetrate the mystery of their origin and subjugation have proved abortive.

We find in the Roman and Greek historians, as well as among those of contemporary nations abundant accounts of domestic animals of various kinds, and the fact is not less certain that each country possessed certain types or races which seemed to belong more especially to it. First the Greeks and then the Romans, conquered almost all the world known to them, and as they extended their marches they everywhere became acquainted with strange people, and with animals and plants entirely new to them. They brought home with them new breeds of domestic animals from various quarters, and in no department, perhaps, did they find so great a variety as in that of dogs. The Greeks and Romans had their sportsmen as we have, and certain races of dogs, with which they often hunted in packs, and which were well calculated for the kind of hunting in which they in-

dulged. Among others we know that the common stag-hound, and the mastiffs were unknown previously to the Eastern campaign of Alexander the Great, the latter of which attracted great admiration in Greece, and is graphically and enthusiastically described by her writers.

At a much earlier date, on the ruins of Nineveh, Babylon and Persia, we find, well portrayed, various breeds of dogs and other domestic animals; and on the time-worn monuments of Egypt, we see unmistakably represented the grey-hound, the stag-hound, the turn-spit, and several other breeds which we cannot name, together with wolves, jackals, foxes and hyenas, all dating back to a period from one to two thousand years beyond the epoch at which Moses is supposed to have written the Pentateuch.

Wherever travellers, in more modern times, have extended their wanderings and discoveries, they have found new species of those genera, to which our domestic animals belong, forming elements in distinct Faunæ; and there is, perhaps, no one genus that shows such an infinite number of new forms as that of the *canines*. It is, too, a very significant fact, that the newly discovered countries—those most perfectly isolated from other countries—are those which present the greatest uniformity among their dogs. When the continent of America was discovered, it possessed no representative of the genus to which the horse belongs. It had nothing of the ox tribe except the buffalo, which is acknowledged to be of a distinct species from our domestic cattle. There were here no domestic sheep, goats, hogs, or any of the domestic animals known in the old world, except the dog, and this was so uniform in type, so different in appearance, habits and instincts from those of the old world, that we have every reason to believe they were of distinct origin, and were a primordial element in the Faunæ of this Continent.

In New Holland we find an analogous state of things; there the Dingo seems to be the only dog, and forms a part of the peculiar Faunæ of that Continent.

On the Continent of America there was no dog resembling the grey-hound, stag-hound, mastiff, terrier, or any dog of the old world, but they every where resembled so much the native wild dogs or wolves, and presented so little variety among themselves as to lead to the conclusion that they were indigenous to the soil. This conclu-

sion is greatly strengthened by the fact already stated, that the aborigines of America possessed not one of the domestic animals of the old world, and were surrounded by a Fauna and Flora entirely distinct from all others.

Not only is the history of our domestic animals lost in antiquity, but we are even without data for determining whether many of them ever did exist in a wild state. All those species that are now domesticated, when allowed to run wild, for many generations, are reclaimed without difficulty, while on the other hand, those species of canines, bovines, equidæ, etc., which exist in their original wild state, cannot, by any care, be fully domesticated. It is highly probable that those species which were intended for domestic uses, were, from the beginning, placed in their proper relation to man.

Before we can come to a clear understanding with the reader respecting the natural history of dogs, or any other genus, it is indispensable that we should define our position with regard to the meaning which we attach to the term *Species*—a point which has given rise to no small amount of difference of opinion and discussion.

The following expresses the idea of Cuvier: "We are under the necessity of admitting the existence of certain forms which have perpetuated themselves from the beginning of the world, without exceeding the limits prescribed: all the individuals belonging to one of these forms constitute a *Species*."

The definition given by the great majority of naturalists, convey the same idea in different words, and the one of Dr. Prichard, in his "Physical History of Man," may be received as one of the most full and lucid heretofore given. He says: "The meaning attached to the term *Species*, in natural history, is very definite and intelligible. It includes only the following condition: viz., *separate origin and distinctness of race, evinced by a constant transmission of some characteristic peculiarity of organization.* A race of animals or of plants marked by any peculiar character which it has constantly displayed, is termed a a *species*, and two races are considered specifically different, if they are distinguished from each other by some characteristic, which the one cannot be supposed to have acquired, or the other to have lost, through any known operation of physical causes; for we are hence led to conclude, that tribes thus distinguished have not descended from the same stock. *Varieties*," continues Prichard, "in

natural history, are such diversities in individuals and their progeny, as are *observed* to take place within the limits of species. *Permanent varieties* are those which having once taken place, continue to be propagated in the breed in perpetuity. The *fact* of their origination, *must be known by observation or inference*, since, the proof of this fact being defective, it is more philosophical to consider characters which are perpetually inherited as *specific* or *original*. The term *permanent variety* would otherwise express the meaning which properly belongs to species. The properties of species are two, viz., original difference of characters, and the perpetuity of their transmission, of which the latter only can belong to permanent variety.

"The instances are so many in which it is doubtful whether a particular tribe is to be considered as a distinct species, or only as a variety of some other tribe, that it has been found by naturalists convenient to have a designation applicable to either case."

Type is also a term of frequent use, and the definition of Cassini may be received as unexceptionable as any other: "Typical characters are those which belong only to the majority of natural bodies comprised in any group, or to those which occupy the centre of this group, and in some sort serve as the *type* of it, but presenting exceptions when it approaches its extremities, on account of the relations and natural affinities, which do not admit of well-defined limits between species."

Some few naturalists have added another test in their definition of *species* to those above given, viz., the production of "fertile offspring by association." In other words, they assert, that when a male and female produce a progeny which continue to be prolific without limit, *inter se*, they must be derived from the same original pair. Now of this no satisfactory proof has been adduced, and this idea must be regarded simply as an assumption.

The subject is one full of difficulties, and a long course of observation will be required to work up the material which we have at command, and many points must remain forever unsettled, for the law, "thus far shalt thou go and no farther," applies to every department of human knowledge. Anatomy is the true ground-work of natural history, and all tenable classifications must be based upon it; but while it serves us so well in the broader divisions of the animal kingdom, it unfortunately utterly fails us when we come down to the

designation of *species*. Here we are thrown back upon the history of each group or race of animals, and we can rely upon no other test than the *permanency* of types through all known time and existing physical influences. The *naturalist* is compelled to regard as a distinct species any animal form which has been perpetuated through ages, and in opposite climates.

The subject immediately under examination is the genus *Canis*, which affords an admirable illustration of the difficulties which invest this subject of *species*. This genus includes not only the endless varieties of dogs seen over the face of the globe, but also the wolves, foxes and jackals, which present almost an equal diversity of so-called varieties. All those canines, when minutely dissected, present identically the same anatomical structure; the same number and form of bones, of muscles, of blood-vessels, of viscera, etc., and Cuvier himself could not distinguish one of these species from another, were it not for their external investments and their histories. It being acknowledged on all hands that external causes influence greatly the external coverings and appendages, the uncertainty in which we are involved become, at once, apparent.

The difficulty of designating species, is by no means confined to the *canidæ*, but attaches equally to other genera, as the bovines, the equidæ, the felines, ursines, etc.; so with birds, reptiles, fishes, etc.

It has been a generally received opinion, that each species is the offspring of one original pair, but this also is entirely without proof, and Mr. Agassiz in the first volume of his last great work, approaches the whole subject from an entirely new point of view.

It having been, of late years, well settled among naturalists, that all the animals and plants now scattered over the face of the globe, as formerly supposed, *were not created at one point* in Asia or any where else, and thence disseminated over its surface and adapted to various climates; but that, on the contrary, many centres of creation exist, with natural divisions of the earth into zoölogical realms, each presenting a totally distinct Fauna and Flora. Not only is this a law of our geological epoch, but of a long succession of creations, which preceded the advent of man, by millions of years.

Nor does Mr. Agassiz hold to that narrow view of the operations of the Almighty, which restrict the origin of each species to a single pair. He believes it much more in accordance with Infinite power and wis-

dom, that a whole Fauna and Flora should have been created *en masse*, with animals and plants in harmonious proportions.

Let us take America, for illustration, without touching its minor divisions, and cast our eyes over the whole Continent. We find our forest trees, shrubs, grasses, etc., scattered broad-cast over the land from the Arctic to Cape Horn. We behold an immense variety of carnivorous, herbivorous, and other quadrupeds—of reptiles, fishes, insects, radiates and mollusks, all dependant on each other for subsistence, and existing in harmonious proportions. The idea of Mr. Agassiz is, that these elements of the American Fauna existed from the beginning much in the proportions and relations to each other which they do at the present day. When we look over our immense prairies and pine forests, can any one imagine a reason why a single blade of grass, or a single pine tree should have been created? why one pair of buffalos? of deer? of bees, etc.? Does it not seem more probable that Omnipotence should have called a world into existence, complete in all its details, than that each organic form should be allowed to struggle on through doubts, difficulties, and uncertainties? Mr. Agassiz, moreover, concludes that the aboriginal man of America, was a part of its original creation, and having no more connection with the human races of the old world, than has the Western with the Eastern Faunæ. The same chain of reasoning applies equally to all other grand divisions of the earth, each possessing a Fauna and Flora exclusively its own.

Mr. Agassiz has shown conclusively that our information is too imperfect to allow us to decide positively on the question of unity or diversity of origin of many, in fact of any species. From the best lights we at present possess, it seems clear that different species are often perfectly prolific *inter se*, and on the other hand, there is ample reason to believe that species were created in groups, and not in single pairs, and this mode of origin may explain "varieties" seen in our domestic animals much better than the influence of climates, which has been assumed without proof.

Without pretending to deny a certain amount of influence of external causes on animals and plants, we cannot but believe that very extravagant and irrational ideas have prevailed on this point. The Supreme Intelligence has been through all past time operating on a fixed "plan," as is evinced in the succession of creations which have

preceded the epoch of man. Faunæ and Floræ have followed each other at immense distances of time, and yet they all bear a harmonious relation to each other, and did our knowledge permit us to follow the chain of "thought" which dictates this mighty work, we should doubtless see an unbroken chain, without a single missing link. Every class, every order, every family, every genus, species, nay, every individual, is but the expression of an idea, which is independent of those physical causes to which so much *Creative Power* has been attributed, and which are in reality but another necessary part of the original plan.

Climate, or more properly *media*, have no influence in creating organic forms. Take the small strip of land bordering the shores of the Mississippi from the City of New Orleans to its mouth, and ask by what power were here placed the innumerable forms of carnivora, herbivora, rodents, reptiles, fishes, radiata, mollusks, etc.? We here find the most varied forms in the same *medium*, and if we travel to the "uttermost parts of the earth," there are found species almost identical in anatomical structure, though having no genetic connection. The dogs, wolves, jackals and foxes all over the globe have the same anatomical structure; so with the bear genus, the horse genus, the cat genus, and we might add the man genus, etc., etc. We repeat that all the species, of any genus have the same anatomical structure. and can be distinguished only by their history and external marks. We hold, too, that the ideas we have set forth are in accordance with the teachings of Holy Writ:

"And God said, let the earth bring forth *grass*, the herb yielding seed, and the fruit tree yielding fruit, after his kind, whose seed is in itself upon the earth; and it was so.

"And God said let the waters bring forth *abundantly*, the moving creatures that hath life, and fowl that may fly above the earth, in the open firmament of heaven.

"And God created great *whales*, and every living creature that moveth, which the waters brought forth *abundantly*.

"And said, let the earth bring forth the living creature after his kind, cattle and creeping thing, and beast of the earth after his kind, and it was so.

"God created man in his own image; *male* and *female* created he *them*."

In the language above quoted, nothing is said about one seed, or one blade of grass—about one fruit tree, or single pairs of animals, or human beings.

There is a discrepancy between the first and second chapters of Genesis respecting the origin of the human family, respecting which our views have been fully explained on a former occasion. Our subject at present is the Natural History of *Dogs* and we do not desire to wander off from it, where it can be avoided.

Unfortunately, our subject of dogs, like most others in natural history, has been much embarrassed by false chronology and antiquated theological notions. Archbishop Usher, a human being like ourselves, has fixed upon the Anglo-Saxon race the date of 2348 B. C., for the Deluge, and certain supernatural naturalists have derived all our dogs from a pair of curs preserved in Noah's Ark; not only the canines, but every plant and every animal over the face of earth have been for ages traced back to the same prolific source, and naturalists who would not subscribe to this doctrine have been branded with the *soubriquet* of infidel. This view, if substantiated, would greatly simplify the labor of zoölogists, but the newly established doctrines of the geographical distribution of Faunas and Floras has cut out some centuries of hard work for us. No naturalist of our day doubts that the present Fauna of the earth goes back many thousand years beyond Usher's date, or that the organized beings of the earth have started from many different centres of creation.

In a scientific point of view, all our domestic animals stand precisely in the same position, and their zoölogical history resembles so closely, and has been so long associated, with that of man, that their study presents peculiar interest. The *dog* must take precedence above all others, for while the horse, the ox, the sheep or other domestic animals have been wanting in certain newly discovered countries, *he*, in some form has everywhere, and at all times known to history or tradition, been the companion of man, and would seem to form an integral part of every zoölogical realm. Nor would any naturalist ever have called in question the distinct origin of races of dogs, had it not been for one stumbling block, viz., an assumed law of *hybridity*. It has been assumed as a fact, that when offspring of a male and female are prolific *inter se*, without limit, they are of one species, and descended from one original pair. All dogs, cattle, horses, hogs,

sheep, goats, tried by this test have been decided to be of common origin. The same process of reasoning has been applied to the races of man and with the same want of solid ground-work. It is not my purpose here to treat the natural history of man, but it is so blended with that of domestic animals that the two cannot be separated if we would.

In "*Types of Mankind*," and the Appendix to Gobineau's "*Moral and Intellectual Diversity of Races*," translated by Kotz, I have, I think, shown conclusively, that prolificness is no test of common origin. I have not space or inclination at present to go fully into this subject of hybridity, and will merely allude to a few of the well established points.

When animals of different races or species are brought together, even members of the same genus, experiment has shown that there are *degrees* of prolificacy varying between absolute sterility and perfect prolificacy.

I adopt the division of Morton: 1. When *remote* species are bred together, they produce sterile offspring, for example, the horse and ass.

2. *Allied* species; where the offspring are not prolific *inter se*, but with either of the parent stocks.

3. *Proximate* species; where the offspring is prolific for a time with a tendency to run out after a few generations. Interesting examples of this are found in the dog when crossed with the wolf or jackal.

The experiments of Flourens in the Garden of Plants, at Paris, show that the cross between the dog and wolf become sterile, when bred together, after the *third;* and the dog and jackal after the *fourth* generation.

Now, here is a regular gradation, and no reason can be assigned why the last link, the only one necessary to complete the chain, should not exist; and we contend that dogs and other domestic animals, to say nothing of man, do furnish this link. Gradation seems to be a universal law of nature.

Now, how stands the case with the *canines*, the history of whose types is involved in so much obscurity? Many opposing opinions have prevailed, of which the following are the most prominent:— 1. By some it is believed that the canines compose one genus, with several sub-genera, viz., dogs, wolves, jackals, foxes; and by some, the hyenas are added. 2. Each one of these groups, or sub-

genera has been divided into several species. 3. Another opinion is, that no subdivisions should be made except *varieties*, all being of one species of common origin. 4. That the domestic dog is a mongrel, made up of a mixture of all the above sub-genera.

We have already shown that the anatomical identity of the whole genus is such as to leave us no structural basis for an opinion; and, moreover, that the fossil remains of extinct species cannot be distinguished from those of living Faunæ.

The dog is prolific with all the members of his genus, but in different degrees. He breeds with the wolf, jackal, fox, and hyena, but to what extent has not been determined, except with the first two. As far as we know it is a law in every genus composed of several species, that they differ in degrees of prolificacy *inter se*. We have given examples above in the canines, and in the equidæ we have equally well marked instances. The horse and ass produce unprolific offspring. The horse and dziggetai a prolific offspring, etc. And if we had a complete series of experiments on all the *canidæ*, I have no question that we should find every conceivable grade.

It is certain that the prolificness of dog with wolf and jackal has limits, nor is it demonstrated that all dogs are equally prolific *inter se*. The bull-dog and mastiff are said to be careless about intercourse with other breeds, and the breed of the Irish stag-hound, and some others are kept up with great difficulty.

The vulgar idea is, that the different breeds of domestic dogs are attributable entirely to the art of man. It is supposed, that desiring particular qualities, *dog-fanciers* have selected and bred for generations with particular objects, and that our bull-dogs, pointers, grey-hounds, stag-hounds, terriers, etc., have been the result; but we are prepared to show that such has not been the case. Although we can manufacture mongrels to any extent, we cannot originate a well-marked new type, such as the grey-hound, bull-dog, terrier, etc. Most of the well-marked types can be traced back for several thousand years, and when unadulterated have been bred for ages, in all climates, without change of type.

The decyphering of the Egyptian monuments has, within the last few years, thrown an entirely new light on the history of domestic animals, and the following important points are now conceded:— 1. That the present Fauna of the earth has existed many thousand

years longer than has been supposed. 2. That the principal types of dogs now seen may be traced back several thousand years, and that they are *permanent* forms in all climates. 3. Not only several breeds of dogs, but wolves, jackals, foxes, and hyenas are depicted on the Egyptian monuments five thousand years ago as distinct as they now are.

The several races of dogs have been distinct through this long lapse of time, and no example of transformation of one into another can be adduced. The sub-genera have also remained distinct. Now, we can see no reason for going back beyond the reach of history and supposing certain things to have occurred which would lead us to suppose that the nature and habits of animals had changed. We certainly have as ample material, among the dogs, to work with now as could have existed at any former time, and still no such "*permanent variety*" has been or can be produced as a grey-hound, stag-hound, bull-dog, terrier, etc. Their habits, instincts, forms, senses, psychical characters, etc., are totally different, and cannot be accounted for by anything short of original diversity.

The canines form an immense chain, all more or less prolific *inter se*, and the sub-genera run into each other by such insensible grades, that it is impossible to say where one ends and another begins. They differ much in the facility of their domestication. All the dogs are easily domesticated, and when they run wild, are easily reclaimed after many generations. None other of the sub-genera can be fully domesticated. The same difference exists in the species, of the equidæ, bovines, etc.

All those wild animals which can be perfectly domesticated, as the deer, raccoon, etc., become so at once, and all others can never, in any number of generations, be brought fully under the guidance of man.

Various attempts have been made to classify the canines at large, as also our domestic dogs, which present such varied forms and wide distinction ; but all classifications have been, and must be, from the nature of the case, entirely arbitrary. We have already seen that there is reason to believe that this is a very old world—much older than was supposed even in the early part of the present century, and that dogs, as well as man have existed some thousands of years prior even to Usher's date for the Creation. Human nature and dog nature, in all probability, were the same from the beginning as at present,

and the physical laws which now govern them, it is presumable, have never varied. We know that during the last five thousand years the races of man have been engaged in commerce, wars, migrations and amalgamations, and the dog has everywhere been his companion, and has been subjected to the same endless mingling of races ; and we may fairly conclude that a perfectly unadulterated breed of dogs does not exist at the present day on the face of the globe, unless it be in some unfrequented spot of the earth, where no opportunity for amalgamation has existed.

It has been asserted that every animal when domesticated has a tendency to run into endless varieties, and display not only every possible variety of color and form, but of instincts and habits. There has been too much taken for granted, and it is most probable that those species that have been most mixed, give the most remarkable varieties. The camel and the ass, which had, in early times, a more restricted geographical range, do not present striking varieties. The horse and the domestic ox, on the contrary, were found at the dawn of history more widely spread, were probably derived from many original sources, and present endless types. The deer in the English parks, though domesticated for ages and derived from more than one stock, do not present the varieties of color, form, size, etc., of horses, cattle and dogs. Our domestic fowls show the same differences under the influence of climate, etc. Turkeys and chickens present endless varieties; Guinea fowls and pea fowls few.

There is every reason to believe that the dogs are composed of more primitive elements at the present day than any other domestic animal. Different races have been found in every country on earth, and the most numerous varieties seen are just where we should expect to find them, viz., in civilized countries, where they have been brought together from all quarters. If dogs were all of one original stock, as many forms should have existed in Australia or America, as in Europe or Asia; but the reverse is the fact. The Australian and American native dogs present remarkable uniformity of type, and never run into the endless cur varieties seen among the dogs of the old world.

Let us now draw some illustrations from a few of those races of dogs with which we are all most familiar in this country. The natural history of dogs is a vast one, and we only here pretend to

skim the surface. The reader who desires more full information is referred to the two volumes on the subject in the *Naturalists' Library*, to say nothing of numerous other sources of information.

Whence come our domestic types, so well known for ages? We may commence by saying that it is not perfectly certain that an original wild dog has been anywhere perpetuated in a wild state. It is believed by many naturalists that the domestic dogs of the Australians and the American aborigines still exist in wild packs; but on the other hand, these wild dogs may be descendants of original domestic dogs, as we have well authenticated examples of such occurrences.

Feral Dogs.—By this name have been designated the descendants of domestic dogs that have run wild, and which are presumed to have assumed their original habits and instincts. There has been much written about the feral dogs of Natolia of Russia, and of South America, but the statements are too contradictory, and their history too confused to be treated as matters of science. But there is one feral race at least, whose existence is not doubtful and whose history is interesting, viz., the *feral dog of St. Domingo.* This breed is descended from the impure Spanish hounds brought to America, and used in hunting the Indians. They have been for very many generations living independent of man, in a wild state, and are as destructive as wolves to stock. They are described as having a good deal of similarity to each other in color, form, etc.; they are of a dark-ash or slaty brown, large, strong, active, well formed, rough-haired, and look like a cross between the blood, and Irish grey-hound. They do not resemble, at all, wolves, and, unlike wolves, are easily domesticated, and assume the habits of common domestic dogs.

Nor do the feral dogs of Mexico and South America at all resemble wolves. They come to resemble each other very much in color and form, as any stock of animals will in time, when kept separate from others. A pack of curs will, in any instance, after many generations form one type, some prominent blood taking precedence.

Grey-hound.—This is one of our best marked types, and his characters are too well known to require description. He is the fleetest of all dogs—chases by sight alone—has little smell, little intelligence and little affection. The breeds of these dogs differ in size, hair, color, etc.; some are smooth, others rough, some have long, some short

hair. It is a question whether these varieties are due to climate, or to original diversity of origin. The latter is probably the case, as the different breeds are perpetuated for ages, in the same climate, when kept separate, as is seen all over the civilized world. In Great Britain the rough Scotch and Irish grey-hounds, and the common smooth stock, have been kept up from time immemorial. Much less can any example be adduced of grey hounds, of any breed, being transformed by climate into curs, bull dogs, stag-hounds, terriers, etc.

Not only are different breeds of grey-hounds now found in many parts of the old world, but they may be traced up through all dates for five thousand years. They were common in Great Britain in the time of Cæsar; they were common in Greece and Rome, and are abundantly portrayed on the monuments of Egypt, on the earliest monuments.

The best breeds are those of Persia and surrounding countries, and there is reason to believe that one race, at least, is indigenous in the East. The Irish and Scotch hounds are probably from those of Russia, which are large, rough and strong. Grey-hounds were used in Ancient Egypt in hunting gazelles, as we know from hunting scenes well portrayed on the monuments. The grey-hound of the monuments is the common Nubian dog of the present day.

Stag-hounds.—The long eared stag-hound is another well-marked type which contrasts strongly with the grey-hound. He is comparatively slow in speed—chases by the scent—is sagacious in pursuit—"gives tongue" freely—has long pendulous ears, broad nose, and altogether different in form. These hounds were common in Rome, but unknown in Greece anterior to the time of Alexander. Although, like the grey-hounds, they all have common characters distinguishing them from other races, yet they present many varieties, some of which are known to be mongrel, while others are probably of distinct origin. We have the talbot, the Oriental hound, the blood-hound, the stag-hound, the beagle, etc., and they come from such different points of the compass, in many cases, and preserve so well their characters in all climates, that the heterogeneous mass of hounds of our day, are, we have no doubt, of many origins. One fact at least is certain, viz., that the long-eared stag-hound was contemporary in Egypt with the grey-hound, five thousand years ago, and they were used in the same way as we now employ them, in the chase. The reader has only to

refer to the splendid works on Egypt of Champollion, Rosellini, and Lepsius, to be convinced that the grey-hound and stag-hound existed together in Ancient Egypt as distinct as they are now, and no climate has ever transformed either type into any other.

Terrier.—This presents another ancient and well-marked type. It is unlike any Eastern type—was common in Great Britain in the time of the Romans—flourishes there better than elsewhere, and it is believed to be indigenous to that soil. There is no dog with more strongly marked characters than the *terrier*, and he preserves them in all climates where he can live.

Pointer.—This race, as far as we are informed, was not known among the early nations of the East, and its history is involved in obscurity. The pointer, which resembles the hound more than any other, has been classed with this race and regarded as a mere *variety;* but the habits, instincts, etc, are so different that we cannot agree with this classification. No facts in the United States lend any support to this theory. Pointers or setters are not mentioned in England until the latter part of the seventeenth century and were brought there from Spain, to which country they were probably indigenous. There is good reason to believe that the pointer is a distinct species, while the setter is believed to be a cross of pointer and spaniel. And although the pointer and setter are trained and hunted in the same way, if bred together for a few generations they become worthless.

Mastiff and Bull-dog.—These have been classed together with very questionable propriety. The bull-dog has been common in Great Britain for two thousand years, and is well portrayed on Roman monuments. As the bull-dog was not known in the East, it is highly probable that he may be indigenous to Great Britain.

The mastiff was unknown in Greece, or other nations more westward, before the time of Alexander, who brought them home from his Eastern campaign. In Layard's work will be found mastiffs beautifully portrayed on the monuments of Nineveh, and they are, doubtless, indigenous to Asia. The finest breed is found in Thibet. It is asserted that bull-dogs and mastiffs do not mix very willingly with other dogs.

Turnspit.—This dog is well portrayed on the monuments of Egypt two thousand years before Christ, in addition to the grey-hound, stag-hound, and several breeds unknown to us.

Water Dogs.—There are several races of these with peculiar habits and instincts, which preserve their types perfectly in all countries when kept pure. They are, doubtless, composed of one or more original species.

But it is time to draw this rambling article to a close. We have alluded only to a few of those races of dogs, that are most familiar to us in the United States, deeming it quite sufficient, for our present purpose, to establish the distinct origin of a few species. If *but two species* exist, of distinct origin, and perfectly prolific *inter se*, it establishes the *principle* we are contending for. If there be two species of dogs, there may with equal reason be a hundred.

Asia, Europe, Africa and America present an endless number of types with which we might occupy our pages, and strengthen the positions taken. The arctic, temperate, and tropical zones present several species of different genera, which are acknowledged to be distinct. The bear is an example. Although bears every where present the same anatomical structure, they are distinct and cannot exchange climates. So with the dogs. The arctic and tropical dogs have the same structure, are prolific *inter se*, and yet are indigenous to their respective zones. We have stated, and the authorities are easily adduced if required, that stag-hounds, grey-hounds, bull-dogs, mastiffs, turnspits, terriers, etc., may be traced back among ancient nations from two to five thousand years, in forms identical with those of the present day, and having the same instincts, habits, and uses.

These different forms have preserved their types through all this time unchanged by climates, and resisting all influences but amalgamations. The grey-hound, stag-hound, terrier, etc., never lose their types in any number of generations, or in any climate where they can exist, if the blood be kept pure, and it is idle to talk about such distinct physical and psychical types being created by chance or any caprice of man.

That bull-dogs, grey-hounds, stag-hounds, etc., are accidental varieties, become *permanent*, is an assumption unsustained by facts, and contradicted flatly by all experience. Whatever animal form is *permanent* for five thousand years must be regarded as a *species*, or the naturalist had as well burn his books and cease to study nature.

The natural history of dogs is of extreme interest in itself, but its importance is greatly enhanced from its connection with that of other

animals, and especially man. I regret that circumstances have compelled me to treat it in such a hasty and negligent manner. I promised our worthy editor an article on some subject by a certain day for the present Number of the Journal I have been compelled to perform the task in the midst of the most incessant and fatiguing professional duties, during the "small hours" of the night. If this fragmentary sketch serves the purpose of exciting others to reflect and examine, it will effect all the good I hope for.

ART. VI.—*Outlines of Lectures on Yellow Fever:* By JAMES JONES, M. D., Professor of the Practice of Medicine in the University of Louisiana.

THESE Lectures are published in compliance with the wishes of my class, and will be issued periodically in this Journal. From respect to the privileges of other contributors I have sacrificed much to brevity, and have omitted many statistics and references. Reproducing in this concise form the outlines of my discourses on yellow fever, I have sought to avoid dogmatism and desire to avoid controversy. Possessed for more than twenty six years, both individually and in association with the most eminent of my profession, of every opportunity offered in this city for the investigation of this disease, I feel guilty of no presumption in adhering to the results of my own observations, however adverse to those of many whom I have always held in the highest estimation.

Gentlemen—In our late description of the exanthematous and malarious affections, we touched very superficially upon those general doctrines of fever—of which the most theoretical are not devoid of truth—the most philosophical not devoid of error. The indisputable importance of an accurate knowledge of febrile maladies—the scientific and the practical interest of every principle that observation

or reason can derive from their study, have, doubtless, concentrated your attention upon the pathognomonic characters of those groups and series of morbid phenomena upon which their diagnosis, their classification, and their nomenclature are universally established. Would that it was given to demonstrate as palpably the real and the efficient etiological elements whereby these types and forms are so differently yet so regularly constituted. Whatever be their physical qualities, whence derived and however generated and developed, there is a period in their toxic manifestation wherein all attain an obvious and visible significance, however diversely in time, and in mode, they have accomplished the degrees of their obscure evolution.

An eminent living pyretologist, in his graphic delineations and analyses of the general characters of fever, has indicated the paroxysmal as the most prominent and distinctive. To my conception, the order and progression of stages, the regular and successive incipience, culmination and decline of the morbid phenomena are more obvious, more constant and more generic. There are fevers of one paroxysm —none essentially of one stage. We do not here study, however, the succession of stages, but the efficient sources of their origin and the essential modes of their sequence. It is, indeed, from the obscure and undetermined nature of the material and morbid agents received or generated in the organism that science offers no acceptable solution for the manner of their action. Nevertheless, with this confession of ignorance, I say material agents—be they malarious, infectious or specific—be they parasitic and fecund organisms, animalcular or cryptogamic—be they morphological or effete resultants of vital metamorphoses, fibrinous, pyoæmic or septic—uræmic, choleic or lactic, bred in whatever acute or chronic diathesis, I believe only in blood contamination, and hold to so much of the humoral pathology as advocates a materies morbi in every form and degree of fever.

This doctrine does not contest the reality or the importance either of the special functional derangements or of the established anatomical characters of fevers. We do deprecate the undue consequence attached by any school of pathologists, to the lesion of any organ, or series of organs, in the mechanism of fever, to the exclusion of general changes in the fluids. This is particularly applicable to the organs of innervation. Physiology has well determined the psychical, sensitive and motory functions of the cerebro-spinal axis, and it has

amply demonstrated its coöperation with the organic or ganglionic system, in sustaining and regulating the nutritive, secretory and calorific processes. The abnormal conditions in which, however, we perceive the evidences of acutest pain, the most violent convulsions, and the direst perversion of the intellectual and moral manifestations, present in their purer and uncomplicated forms no symptoms of pyrexiæ, and obtain in diatheses antagonistic to their exhibition. I have, indeed, attempted elsewhere to establish that in the neuroses, the apparently pure dynamic conditions of innervation are frequently as in the pyrexiæ secondary pathological conditions—vices of nutrition, only to be connected by therapeutic agents directed to the medication and restoration of the circulating fluid.

In determining the etiology or special cause of any distinct type of fever, it is proper and philosophical in the present state of our knowledge to refer it to the operation of a peculiar and special cause. Common sense would revolt at a dogma that referred all of the exanthemata to the operation of a single toxic agent, and when we approach the consideration of other eruptions now universally attributed to certain parasitic organisms, I will prove that each is invariably associated with a peculiar growth. There is a vicious proclivity to discover and establish the relations, either real or imaginary, of every obscure disease with some more familiar and professedly better understood. You have already beheld the confusion obtaining in the earlier history of the exanthemata, from the supposed relations of measles, scarlatina and varicella to small-pox; and thus you will discover that there has been and is a settled determination to establish a pedigree for yellow fever, and to trace and fix its relations to every other known form of fever. First, it was a form of the plague—now a bilious or remittent bilious fever—then an American, African, or ictorode typhus —a causus—a yellow or malignant synochus—as if all were afraid or unwilling to view it as a disease *sui generis*, an independent and distinct malady in cause, in symptoms, and in its pathological and anatomical characters.

The early history of few maladies is well authenticated; it is not extraordinary that the times and localities in which yellow fever first made its appearance should be sources of controversy. Small grounds exist, in my opinion, for asserting that it was in any manner known to Europeans anterior to the discovery of America. Native traditions, both in South and in North America, appear to confirm a rational con-

viction that it had its favorite haunts and its victims long before commerce and civilization contributed their ample resources to its development and propagation. "There is nothing to prove," says Humboldt, "that yellow fever has not existed in the equinoctial regions for the period of many centuries. We should not confound the epoch at which a disease has been for the first time described, because it has made great havoc in a brief space with the epoch of its first appearance." Long erroneously associated with diseases more familiar, and first appearing among Europeans when neglect of naval and military hygiene added the fatal evils of scurvy to the hæmorrhagic disposition always exhibited in its graver forms, yellow fever suddenly came to be recognized as one of the most fatal diseases recorded in the annals of medicine. I will not undertake, on this occasion, to detail when and how it made its appearance among the Europeans who first established themselves upon the intertropical islands and shores of this continent; suffice it to say, that it was not until nearly two hundred years ago, in the latter part of the seventeenth century, that it became acknowledged as a peculiar, if not a new pestilence. First recognized in America, we subsequently discover it at a period not many years later, prevailing with fatal effect in certain ports of Spain, Portugal, France and Italy, which offer many of the climatic characters of its original localities in the Spanish Main, the West Indies, and the Carribean Sea, and maintained with them the freest and most unlimited commercial intercourse. For the minutest and most accurate information on these interesting details, I will refer you to the late extensive monograph on yellow fever, by Dr. La Roche, of Philadelphia, one of the most able, thorough, and learned productions ever devoted to the consideration of one disease.

I indulge in the anticipation of producing, on a more suitable occasion, a chronological and circumstantial history of everything worthy of note in this disease, as it was formerly known in the old colonies of Louisiana and Florida. Its early appearance in St. Augustine, Pensacola, Biloxi, Mobile, New Orleans, Natchez, and other familiar localities present to my mind subjects of extraordinary interest. I refer you now to the admirable Tableau of Yellow Fever published by Dr. Bennet Dowler, in 1853, which, in a concise form is replete with the most accurate and valuable information.

In treating the etiology of yellow fever, we will anticipate the dis-

cussion of the efficient morbid element, by indicating the regions, the seasons, the meteorological conditions and the ethnological, social, and other personal relations of those amenable to its influence.

The first in reality constitutes the study of its geography, and of all the topographical and physical conditions of the localities in which it may be generated or propagated. Yellow fever is essentially a disease almost peculiar to southern latitudes. It has, probably, in no form ever progressed beyond the forty-eighth degree of north latitude or the twenty-seventh degree of south. In America, and I will limit myself to this hemisphere, it does not ordinarily prevail beyond the latitude of Charleston, to the north, and we are informed rarely a few degrees below the equator, to the south. We have thus established several geographical parallels denominated yellow fever zones. In the first, including approximately the northern torrid zone in which is embraced Cuba, and certain parts in the West Indies, the Gulf of Mexico, the Carribean Sea, etc., this disease prevails for long series of years annually. In another, including New Orleans, Charleston, and the intervening ports, although present nearly every year, it prevails only epidemically for considerably less than one-half of the current years. Thus, in the City of New Orleans, within the last quarter of a century, we have passed through twelve epidemics, of which those only of 1833, '37, '39, '41, '43, '47, '53 and 54 have been general and severe, while in Mobile and Charleston, the number has been more limited. From the latitude of Charleston to that of New York, we construct another zone in which we discover that the disposition to the recurrence of epidemics is brought down to a longer series of years, from five to ten and even twenty-five and thirty, and that those born in the localities thus situated, enjoy no immunity against it. Other zones have been equally constructed south of the equator, but in these, as in the others, I have adduced examples rather than exact lines of demarkation.

The localities generally indicated as possessing the topographical characters essential to the developement of the cause of yellow fever are described: 1. As situated for the most part, on the sea cost or on large rivers, at no great distance generally from the sea. They are otherwise described as being frequently in the vicinity of swamps, marshes, lagoons, and low grounds more or less covered with rank vegetation, and with other elements for decay and putrefaction; physical conditions, however, in some localities, entirely absent.

2. It is usually held that in the localities otherwise favorable to the development of this fever, the parts first and most severely affected, and from which it is supposed generally to extend, are wharves, docks, vessels, warehouses, and other structures immediately in the vicinity of the water. We find, however, in this and other cities particularly exposed, that the epidemics frequently commence in a variety of foci, at a distance from the landings, and clearly free from the special influences there supposed to be in operation.

3. It is admitted that this is a disease only to be found where there is an accumulation of a number of individuals for a certain length of time, such as we find in cities, in garrisons, in camps, and in ships. This is perfectly true of the endemic yellow fever or of ordinary epidemics, but in the more diffusible epidemics like that of '53, it will, contrary to usual experience, extend itself to small villages, plantations and dwellings in the most sparsely inhabited districts.

4. It is generally maintained that cities or portions of cities amenable to the imputation of uncleanliness, foul accumulations, want of ventilation, and overcrowded dwellings and courts are specially exposed to visitations of this disease, and that the same law is applicable to ships and to garrisons. This proposition is certainly to be received with considerable reservation. It is not established that either the uncleanest cities or districts of cities are the most liable to invasions of this disease. It spreads often with equal intensity and celerity through the dirtiest and through the cleanest portions of our southern cities; it spares them at times when they are particularly unclean, it invades them in its most violent forms when they present nothing but their ordinary condition. Ships, also, subjected to perfect sanitary regulations suffer often alike with those in the most neglected and impure condition. We do not deny the agency of vegetable and of animal putrefaction in the generation and localization of yellow fever. We advocate, under all circumstances, both in cities and in ships, the strictest attention to hygienic regulations and to the use of every mode of purification, but we contend that filth is generally rather a contingent and auxiliary than a necessary element, and that this disease may prevail independently of its agency.

5. It is also held that low situations are more exposed than the elevated, the flat more than the rolling or the inclined, a porous surface rather than a compact or rocky, and that excavations and other dis-

turbanees and exposures of the soil are productive of tellurie exhalations far more aetive than those from a surfaee long undisturbed.

These are propositions all entitled to a certain degree of consideration. Yellow fever, for instanee, does not prevail on the table lands of Mexico, although in close proximity to Vera Cruz, yet it has been very fatal in Gibraltar and in Spanish cities many hundred feet above the sea. Gibraltar is also remarkable for its rocky surface, as are several other localities more or less elevated in Spain and in the West Indies which are, nevertheless, peculiarly obnoxious to epidemic visitations.

Yellow fever is not only a disease of regions but of periods and of seasons. It is an interesting faet in its history, that even in the latitudes and places most exposed to its invasion, it has oeasionally disappeared, more or less completely, for long series of years. In the torrid zones generally commenceing its ravages late in the winter or early in the spring, it prevails with more or less intensity until the return of winter, when, under ordinary circumstances, it greatly declines but does not always disappear. In this city, although it has been reported in June, it rarely becomes epidemie until July or August. It terminates here in October or early in November. I have seen fatal cases up to the end of Deeember. In 1853, the disease had nearly ceased in the city before it invaded several localities in the interior.

The duration of the disease in its epidemic form, is subject to great variations, at one time due to the exhaustion of the miasm, at another to that of the subjects. The averages, at least in this city, are between forty and ninety days. If it appears early, it departs early; if it commences in the end of August, it may prevail beyond the ordinary period of its decline. In 1832, the epidemic was announeed after the middle of September. It was in full tide in the latter part of October, when, by the sudden intercurrence of cholera, there was an interesting exhibition of the total eclipse of one epidemic influence by the more potent intervention of another. When the cholera declined, in November, I saw, in the Tremé Street Hospital, several patients with blaek vomit, who entered with cholera. To those who indulge in traeing the relations and affinities of diseases, this termination is certainly as evidently corroborative of the influence of one miasm in the production of cholera and of yellow fever as the oeca-

sional substitution or sequence of the latter by intermittent in the autumn, constitutes a manifestation of their common and similar origin.

The principal meteorological conditions connected with the development of yellow fever, are those of heat, of humidity and of electric states the atmosphere.

From the fact that the disease is annual in the torrid zones, and is said in the temperate to prevail in the seasons of the year when the solar influence is at its highest point, it has been reasonably concluded that heat is one of the most effective agents in its generation and development. It is not now, however, always contended that the hottest climates or the hottest months are specially obnoxious in this respect, independently of other influences, because it is well established that the heat of summers, even above the forty-eighth degree of north latitude, is generally superior for a certain period to that of the yellow fever zones, and that within these limits numerous observations have satisfactorily demonstrated that the warmest seasons are not neeessarily the most sickly, nor the coldest the most healthy. Caldwell and others, by numerous observations, attempted to establish during the early part of the present century, that a protracted average temperature of from seventy-nine to eighty degrees Fahrenheit, was necessary for the production of yellow fever in an epidemic form. This may hold true for the latitude of Philadelphia, if not for several degrees further south, where this fever does not frequently prevail; but it has been established by Humboldt, that in the equinoctial regions, seventy-five degrees are amply suffieient, and afterwards by others that in the same latitudes it has proved epidemic in localities, where, by reason of the altitude, the summer range is not much over sixty degrees. It thus appears that although in the general opinion, a high temperature is the most efficient coëxisting element in the etiology of yellow fever, the proposition is subject to several exceptions contingent upon the action of other influenees. It is also well established that after the appearanee of an epidemic, an elevated degree of atmospheric heat is not requisite for its continuanee or propagation; some even contending that a deerease of fifteen or twenty degrees below the accepted standard of seventy-five or eighty degrees, operates more fatally, both through the increase of the epidemic eonstitution, and of

individual susceptibility. In the last aspect it is certainly to be admitted, that undue exposure to solar heat is a powerful agent in hastening and aggravating attacks of the malady. Dr. Rush discovered that bakers, blacksmiths, and hatters, from their exposure to artificial heat, suffered similarly with those much exposed to the sun, by the tendency of fires to propagate and develop the activity of the miasm. Boudin confirms this by announcing that firemen and cooks on the French steamers are more frequently its victims than the rest of the crew. In relation to quarantine and purification, it is well to know also that yellow fever has been found to prevail more severely, and to remain longer on steamers than on any other class of vessels.

As heat has long been held favorable to the development of yellow fever, it was of course to be inferred that while a certain reduction would be hostile to its production, that a lower decrement should be entirely incompatible with its continuance. As it appears that equal degrees of heat are not necessary for the production of yellow fever in dissimilar latitudes, so do we find that it does not require for its decline, under such variations, equal degrees of cold In the tropics, it frequently disappears under a reduced temperature of from forty-five to fifty degrees, in this latitude it nearly always is immediately arrested by a fall of from forty to thirty-two degrees, while you can easily see by the tables of Rush, Carey, and Rittenhouse, that in Philadelphia it has continued in defiance of frost, and that by the old records of 1762, it was more fatal in that city in November and December, than in the preceding months. It may be controverted that a considerable diminution of temperature can add to the quantity of the miasm, it cannot be denied that sudden or even gradual changes add to its force and malignancy. This is probably more by the effect of cold on the individual than on the miasm. It has been too well established for over fifty years, in this city, that our late epidemics and late cases are often the most fatal; that the sudden intervention of cold nights and of dry north winds adds greatly to the catalogue of the sick and to the fatal tendency, while our hottest but equal weather, and our soft southerly breezes give a favorable aspect to the gravest cases. I will inform you more fully hereafter, that want of resistance to cold is an extraordinary feature in the symptomatology of this disease.

Humidity is another meteorological element mentioned by the ma-

jority of sanitarians, as the most efficient and active agent in the generation and propagation of yellow fever. It is to this that they attribute, in a measure, the peculiar liability of seaports and ships. It is to the great precipitation of rain in tropical and southern temperate latitudes, in which the wet seasons of the year are held to be the most unhealthy, that they ascribe, principally, its regular endemic disposition. Farther, it is maintained that in the localities farther north, and only occasionally visited by this fever, the rainy seasons, combining also with them other attributes of southern zones, will be found almost universally as periods most characteristic for their epidemic proclivity. In this city, general observation establishes that the epidemic seasons are, as a general proposition, particularly humid, and we thus witness on the occurrence of an epidemic, that the unpaved streets become altogether impassable from the continual showers and the limited evaporation. Referring to the hyethal or rain charts of Professor Blodget, it appears that the admeasurement of the fall of water, in Louisiana, averages sixty inches annually, of which thirty-five descend in the spring and summer, being fifty per cent. higher than in the middle States. This is also far greater than that in any other region in the Union, of the same extent, and is to be received as a strong evidence of the peculiar tropical constitution of the climate and of its special adaptation to the generation and propagation of this malady.

The humidity of a climate is not only or always directly characterized by the precipitation; whenever the soil, surface, and winds supply the sources, a high temperature will always be attended with a corresponding degree of saturation, so that the thermometer indicates approximatively the quantity of moisture nearly as well as a special hydroscope. For many interesting observations on the etiological value of the dew point, as contended for by Hopkins and others, I refer you to the comprehensive chapter of Dr. La Roche. Did our limits authorize the digression, it would be important to study, in this connection, the possibility of peculiar fermentation in a moist atmosphere, when charged with certain emanations hereafter to be indicated, and to establish the degree of dilution in which the latter must necessarily exist. The volatile emanations from ordinary evaporation, and the gaseous products of putrefaction, are certainly not proper elements for any known process of fermentation, and if the discharge of all

the great sewers of Paris into a small river like the Seine, produces a contamination of its waters, scarcely appreciable at a moderate distance from their conjunction, what must be the degree of dilution of limited telluric or putrefactive emanations into the ever-moving atmospheric ocean above us, impelled in frequent diurnal currents far swifter than those of the most rapid rivers?

Truth compels us to acknowledge, however, that yellow fever has often prevailed in localities very arid, and in and after seasons remarkable for the absence of humidity, and that in this city as in many others more liable to its invasions, summers and autumns memorable for their excessive humidity, have frequently occurred without a case to demonstrate their insalubrious tendency. Antecedents are, indeed, not wanting to establish that in the opinion of some, the opportune advent of heavy rains has exercised a happy influence during the prevalence of epidemics. It was to this that Rush and Carey attributed the decline of the great fever of 1793, not to refer to other similar examples. Popular opinion, in New Orleans, does not favor the morbific influence of the humidity of the soil, as the escape of the city on several occasions from epidemic visitations, has been attributed to the agency of crevasses and overflows from the lake, which have left the greater part of its surface in a saturated condition for many days during and after their decline.

No space at this time will be devoted to the examination of the supposed etiological influence of electric atmospheric disturbances. I will terminate the subject of meteorological conditions by a quotation from Dr. Craigie's Practice of Medicine:

"Whatever resemblance there may be in the characters of the atmospheric temperature in tropical countries, it is clear that every season is not in an equal degree productive of fever. I have already referred to periods of immunity from fever in situations in which the decrease is an epidemial product; and it has been often remarked, that previous to 1793, though sporadic cases were common, and occasional small epidemics appeared on the arrival of a ship of war in the West Indies, most of the islands had enjoyed a period of comparative health, varying from ten to fifteen years. This fact indicates, I conceive, a sort of cycle or periodical revolution in the train of atmospheric phenomena, which, at the end of certain periods, assume the same position and combination in regard to each other. It appears

that yellow fever becomes epidemic neither in consequence of intense temperature alone, nor humidity, nor filth, nor the presence of foul docks and wharves, nor desiccated marshes, nor decomposed cabbages or coffee, or mangroves, nor even charred ships' holds, but in a certain condition of the atmosphere which recurs at very uncertain intervals, and of the recurrence of which the circumstances now enumerated are indications. The rapid decomposition of vegetable and of animal matter is to be regarded *not as a cause of fever, but as an effect of the febriferous state of the atmosphere*, which thus displays its insalubrious influence not only on the human race, but in the vegetable world, and on dead animal and vegetable matter."

That the miasm of yellow fever operates with special potency after night, is the result of universal observation. This is not a peculiarity, however, confined to the cause of this fever as it is more or less the case with every zymotic fever. So commonly prevailed the belief in the special insalubrity of the night, that during the prevalence of our milder epidemics, some of the unacclimated considered themselves safe by sleeping out of the city and returning to their business in the day time. The lower temperature of the air appears to be equally injurious both in concentrating the miasm and in developing its effects. It is found that the larger number of those affected sicken between bed-time and six o'clock in the morning.

Coming suddenly from a pure atmosphere into that of the epidemic, is nearly always followed by a speedy and severe attack of the fever; going out of the infected into a pure air, is almost certain to terminate in a similar manner. Those in the former category often sicken within the first twenty-four or forty-eight hours, for there is no fixed period of incubation in this disease. If those in the second are not confined in an infected ship, their chances for immunity become very good after the expiration of the first forty-eight hours, and after that of a week, escape, with a little prudence, is almost certain; cases, however, are recorded in which the disease has been developed after a longer period.

Fresh immigrants arriving from sea are, of all others, most exposed to immediate and violent attacks, and we have noticed inceptions of epidemics in which they have constituted for a number of days the majority of cases admitted into the Charity Hospital. On this account many ships have been presumed to introduce the disease into certain

localities, when it has been proved that no sickness existed on board previously to their arrival, and when it became very evident that their peculiar susceptibility renders the passengers and crew, in a large degree, the first victims to the epidemic constitution of the vicinity.

The opinion prevails in yellow fever regions that individuals residing on the ground floors are more exposed, and have severer attacks than others occupying the more elevated stories. It is so common for a considerable number to pass unscathed through the worst epidemics, that it requires a number of observations to establish any rationale or any general rule to account for their escape. Yet there is, I think, adequate cumulative evidence to prove that many have enjoyed immunity solely through insulation in the upper parts of high houses. It is thus that the domestics in large hotels occasionally pass without sickening. It has also been observed that through insulation alone, the inmates of our city prisons generally escape the epidemic.

Yellow fever had committed frequent and extensive ravages in the world before it became established to the satisfaction of medical men, that persons who had once experienced an attack of this disease, in the zones where it frequently appears, enjoy thereafter an immunity against a recurrence at any future epidemic. In the way of non-recurrence, yellow fever presents a remarkable analogy to the fevers generated by specific miasms. The protection acquired by this extinction of susceptibility has been in many places very improperly termed acclimation; as it is in no manner connected with the changes effected in the organism by migrations from one region to another of different latitude and temperature, and can only be acquired like the protection against small-pox, measles, and the other exanthemata.

It is well established in the history of all diseases of malarious origin, that the liability to their influence is greatly increased by their previous development. Recurrence and relapse is a pathognomonic and striking character in their symptomatology. By this law of non-recurrence universally admitted to obtain in yellow fever, there is not only a dissimilarity but a species of antagonism to the action of malaria altogether irreconcileable with the hypothesis of a common origin.

It is a common opinion, gentlemen, in many localities exposed to frequent visitations of this fever, that the natives born within the

opinion, is more erroneous. Protection is only acquired by undergoing the disease. I am perfectly satisfied by long observation, that it is true, at least, of the epidemics in this city. The symptoms in all young children, during our ordinary epidemics, are by no means grave, and among the native are frequently so light and transient as to escape the observation their scientific importance demands. Children born after one epidemic suffer on the occurrence of the next. I am confirmed in this view by many respected *confrères* in this city, and in Mobile, and find the same doctrines laid down by Dr. Dickson, of Charleston, one of the highest authorities in everything relating to our southern epidemics. In severe and extensive epidemics similar to that of 1853, the sickness and mortality among children born in this city were unexampled. One of the worst cases I saw was in the person of a child five days old, born while the mother was in the most critical period of the disease. The protection acquired against recurrence is said to be impaired by long absence, particularly during childhood and adolescence. I have never, myself, witnessed a case under such circumstances, and suspect that in this vicinity examples of the nature are very rare.

Some maintain, also, that there is a species of protection acquired by remaining at first during the healthy seasons, and afterwards for increasing periods during each epidemic until there is a final loss of susceptibility. This is called gradual acclimation. There is no such process. A man born and reared out of this city, within the sound of the evening gun, never loses his susceptibility to it. I have attended, in every one of our great epidemics, people who have remained in the city for twenty years, and absented themselves during the sickly seasons, who by confiding at last in the completion of this gradual acclimation, have given the most palpable evidence of its unreliable nature. Popular opinion sanctions the doctrine that passing through one epidemic untouched, is a perfect protection against another. I have seen many cases to prove its nullity. The late Professor Harrison, in his valuable essays on yellow fever, published in the New Orleans Medical and Surgical Journal, in 1846, contended that they who pass safely through one of our great and searching epidemics, will thereafter be found to enjoy immunity; but that the exempt from our milder and partial epidemics cannot be considered protected until they have been subjected to the test of the former. I

do not believe, myself, that, under any circumstances, exposure to the miasm, produces any change in the system. Exemption is only to be considered as an evidence of the absence of susceptibility.

The susceptibility of different types, nations and communities of men to the influence of yellow fever, exhibits great disparities. The white is more liable than the black, Europeans more than Americans, natives of northern latitudes more than those of southern. Antecedently to the epidemic of 1853, I have heard several aged and respectable physicians in this community deny having ever seen a fatal case of this fever in a negro, and until that period, I myself never attended but two who died. The negro not only has the fever in its simple and non-malignant form, but often passes through our ordinary epidemics with immunity. The mulatto is more susceptible, and affected more severely than the negro, apparently in proportion to the white element in his blood. The aborigines are said to have been grievously afflicted at various times with this disease. We are informed by Humboldt, that the natives of the table lands of Mexico contract it and die of it more readily than either North Americans or Europeans. In our very severe epidemics we note that the liability of negroes is much increased—in 1853 they perished both in the city and on plantations in considerable numbers, a circumstance occasionally remarked elsewhere.

In the New Orleans sanitary report, and in the statistics of the Charity Hospital, you will find tabular statements of the comparative mortality of Europeans, and of the natives of the different States of the Union. It is difficult to establish an exact scale of susceptibilities, because they who are domesticated and seasoned to the climate are always less exposed than strangers. This would be my classification: Germans, French, Irish, Spanish, natives of the North and North-Western States, Italians, natives of the South. Spain has suffered more with this fever than the whole of Europe, her people are not entitled to the moderate susceptibility accorded to them in this city.

In proportion to thei rcomparative number, women escape the disease more frequently than men, and generally have it somewhat more mildly. This is, doubtless, due to their seclusion and their habits. After the epidemic of 1853, I counted within a circuit of less than a hundred yards sixteen women, principally Irish, who went through

unscathed, where I did not hear of a single man. Under similar exposure I acknowlege no modification of liability by mere differences in age. I have attended patients with this disease from five days old to eighty-five years, and have found those between these extremes equally amenable to its influence.

I will now terminate this rapid survey of subjects entitled to a more minute examination, by a few brief observations on the special and efficient etiological element, and on the propagation of yellow fever.

We have already invited your attention to the fact, that within a limited range of latitude in the western hemisphere, yellow fever prevails for certain cycles, with so much regularity as to be therein considered an endemic malady. A phenomenon so constant is suggestive of a constant and peculiar morbific influence. It has been further indicated that within these limits there are considerable variations. Beyond the torrid zones, particularly to the north, we observe increasing intervals of comparative immunity. In that extensive parallel between thirty-three and forty-six degrees of north latitude, where the non-epidemic intervals occupy long series of years many have denied the probability of an endemic influence so rare and so partial.

We have examined the alleged independent and concurrent etiological action of the topographical and meteorological conditions of the localities subject to this disease. We have tested each member of these hypothetical formulæ, and we are satisfied that there is some more efficient power than that of ordinary physical and familiar agents which have been operative in every form and degree of combination, for thousands of years, in thousands of localities, without the known generation of this disease beyond its proper geographical limits, where it reigns often irrespectively of the influences invoked.

In advocating the independent, peculiar and specific character of yellow fever, it is not requisite to establish an exact conformity in its mode of development and propagation with every disease universally termed specific, for among the latter there are great generic differences and many individual variations. There are regions in which many remained long exclusive—others where they are more or less dormant—several they have never invaded, and some where they have attained a sudden evolution without a trace or evidence of their origin. Each has its degrees and its variations, at one period benign, at another malignant; at one sporadic or much limited, at ano-

ther raging with irresistible and fatal energy. By what term shall we designate the fecund and disseminative forces by which the exanthemata rise, as it were, from their ashes, to recommence their wide circuits of desolation? If, in our ignorance, we invoke an epidemic constitution of the atmosphere or other coëfficient and contingent agencies, they and others equally hypothetical must be admitted in the development and propagation, whether infectious or contagious, of every zymotic and epidemic disease.

If there be analogies in causation there are analogies in morbific accumulation. Thrush, measles, whooping cough, and scarlatina, in the narrow wards of public nurseries, asylums, and hospitals, multiply their toxic agency with a tenfold fatality. Examine the mortuary statistics of our large hospitals and they will exhibit a loss of from forty to sixty per cent. from epidemic yellow fever. I believe that you can select short periods in severe epidemics in which it is nearer eighty.

It is due to the reputation of the Charity Hospital to state, that a large proportion of the fatal cases enter that institution moribund, and at advanced stages of the disease, but the proportionate mortality is so far above that reported among the patients of the charitable associations treated in their own miserable dwellings, belonging to the same class and subject to similar, though not equal deductions, and is so high a multiple of the mortality in private practice that I cannot avoid the conclusion that the great increment is attributable to the same cumulative proclivity observed in maladies universally held to be specific. There are lodging-houses and dwellings in which after the occurrence of a few malignant cases, the other unprotected inmates nearly all perish in despite of every care that science or humanity can bring to their aid. It is an established principle in tropical military hygiene, to abandon all quarters thus infected, and to dilute the miasm by moving and dispersing the sick and the exposed. Never, gentlemen in this fever, for this and other reasons permit two yellow fever patients in a bad season to remain in the same room, and under no circumstances allow them to occupy the same bed.

Notwithstanding the general confession of ignorance in relation to the real and efficient cause of this fever, it is a disease associated with so many interests of science, of humanity, of property and of commerce, that there is no other known in history of which the etiology

has been more thoroughly and faithfully investigated, or more ardently and acrimoniously discussed. Physicians and civilians, Government and academic commissions have brought together a vast accumulation of monographs and of statistical and documentary reports so voluminous, so confused and so contradictory, that it would be a life's labor to render them subservient to any scientific or practical object.

(*To be continued.*)

Art. VII.—*Practical Remarks on Epigenesis and Sterility:* By Sam'l A. Cartwright, M. D.

The Graafian vesicle is the little bag containing the ovum. It corresponds to the shell of an egg. It is called ovisac. The ovum is one-tenth of a line in diameter. Immediately before or after menstruation, or its first day, or the last day or two of its continuance, is the time that ninety-five in one hundred conceptions occur. It often takes place immediately before. Then the menses, if they appear at all, soon cease. At these times the ovaria receive more blood, and the ovisacs are inflated and discharge the ova by bursting. The uterus at this time is also supplied with more blood for the formation of the deciduous membrane. There is usually no menstrual flow during pregnancy; it may appear once or twice. The discharge that some women have every month is not menstrual blood properly so called. Before the ovisac or outer covering of the egg or ovum bursts, it becomes vascular in consequence of the determination of blood to the ovaria at the monthly turns, and deposits a fleshy-looking substance of a yellow color. The discharge of ova or little eggs from the ovaries, occurs independent of conception or sexual intercourse, and it is not essential, therefore, that the spermatozoa should reach the ovaries to effect the fertilization of the ova. They may be ferti-

lized while passing to the uterus, through the Fallopian tubes or in uterus itself. There are cases proving that they are fertilized while in the ovaria. The farther the ova are extruded the less their capacity for impregnation in the lower animals. But this is not so much the case in the human female. The sexual orgasm is greatest during the evolution of the ova at the commencement of catamenia, and a short time after the cessation of the flow. It is only when the ova are set free, that is, burst from the ovisacs or shells, that they are prepared for fecundation. Absolute contact of the spermatozoa with the ovum is essential to fecundation. The spermatozoa are minute particles or germs endowed with self-locomotory powers. They are not animalcules, as have been supposed, from their moving rapidly under the microscope. Globules of blood have the same self-locomotory powers. They are particles or germs, male and female, being absolute parts of man's body, parts of the whole man himself, as the bud of the oak is part of the tree, or the bud of a plant, part of a plant. Fecundation is the engrafting of this bud in the ovum, which is a part of the body of the female. Woman's part in the reproduction of the species, is to furnish the stem for the graft or bud, and to supply it with nutriment afterwards.

After the ovum is impregnated, it continues to move downwards to the uterus, and acquires, in its passage through the Fallopian tube, a gelatinous covering, corresponding to the white of an egg. This covering is called the chorion, so called from its being a chorus or company of blood vessels; and from them the graft or bud, transplanted in the ovum, derives its nourishment by means of absorbing radicles, until it arrives in the uterus. By the time it gets there, the deciduous membrane is formed in the uterus to wrap it in, and this membrane is supplied with nourishment to feed it. The villi or little projections or tubes of the chorion are embedded in the fleshy substance of the deciduous membrane and supply nourishment to the embryo, as the homonculus within the ovum is called, after it reaches the uterus.

As the embryo grows and wants more nourishment, another coat is formed over the decidua. This second layer is called the decidua reflexa, to distinguish it from the decidua vera. Little tufts shoot out from the decidua, which imbibe nutriment for the embryo, and ultimately form what is called the after-birth or placenta.

The placenta does not begin to form until the latter part of the second month. The blood vessels of the uterus are greatly enlarged at the part where the placenta is attached more than elsewhere, and the blood, in moving through the enlarged vessels, produces a peculiar murmur, which is audible at an early stage of pregnancy and is one of the positive signs. It is a cooing sound, like that of blowing over the lip of a wide-mouth vial, always heard in the same spot, but often heard at one time and not at another of the same day or week. It is sometimes heard in the eleventh week, but cannot be relied on before the sixteenth. The mammary gland becomes more developed and the areola of the nipple, and the nipple itself undergo marked changes by the fourth month, being larger and darker. A sense of distension is often felt at the end of the second month, and a tenderness and knottiness occur in the breasts, with occasional shooting pains; but this is not an invariable sign. May not happen if the woman be pregnant, and may occur if she is not; which are exceptions, not the rule. A puffy turgescence of the nipple, and an increased size of its little tubercles with a dewy moisture on them, is a more constant concomitant of pregnancy. Sick stomach and excitability of mind attend. Women do not generally know when they conceive, others do, as it produces in them a shuddering sensation, a fluttering movement, attended with dizziness and fainting feelings. Others often feel similar sensations nearly every time they are embraced, and some faint. After conception there is no consciousness of change until quickening. Then a fluttering movement and some degree of syncope are apt to be felt, with a show of a little blood. These sensations are renewed once or twice before the motion of the fœtus is felt by the mother.

Some mothers never feel the motion of the fœtus. The usual or average time of quickening is the eighteenth week. The usual time for parturition is the fortieth week, or two hundred and eighty days. Seldom over that time, but very often a few days under it.

Fecundation does not depend much on the general health of either party, because the generative system has, as it were, a life in some degree independent of the general system, as very sickly looking women often are good nurses, and have healthy children.

The generative system does not come to perfect maturity until after the general system begins to decline. The activity and vigor of per-

feet manhood, and the freshness, plumpness and beauty of womanhood begin to decline before they have arrived at a stage of life when the powers and capacities of the sexes in reproduction have gained their maximum intensity, which is reached about the 30th year with woman, and 40th with man. It is in the beginning of the sear and yellow leaf that the fruit matures after the flowers have faded. The children of very young parents have not the same viability or life-power as the children of parents of maturer age and declining age, nor are they so intelligent. But, after a certain age, about 45 in women, they cease to have children ; but up to that time their children are generally more vigorous and intelligent than those they had in early life.

The power of enjoying repeated sexual embraces declines to less than one-half before the power of fecundation has arrived at its greatest intensity. Nature seems to have had two objects in view in bringing the two sexes together. One, that of love and pleasure, and the other, the perpetuation of the species.

Both are natural instincts, but do not run parallel, and one may exist without the other. In early life, pleasure is uppermost, but in middle life, a desire for offspring is the strongest instinct. Mahometans, and all the Eastern nations, deny women sexual pleasures at any other time than when in a fit condition for conception ; after they have conceived, they are set aside and not again embraced until they have had the child and weaned it. The Christian practice is different. Among Christians, the wife is used as a wife during pregnancy and lactation, as at other times, except the month of confinement, and sometimes the last few days or weeks before that event. During the catamenial period, it is the practice to excuse the wife. Many women are sterile from too strict an interpretation of Moses' laws, which are made to include the evolution of the ova, the first and last days of the vaginal discharge as menstrual, because it is colored, often more deeply than the regular hemorrhagic flow, or menses proper. The lower animals do not menstruate ; yet a discharge attends the evolution and bursting of the ova. With them, it is called the pride or heat. In woman, it is too apt to be confounded with the catamenia proper, and sexual indulgence prohibited, under a mistaken idea of its being the prohibited flowers of the Old Testament. The prohibition there, could only have been intended to apply to morbid issues and

to the menses proper—certainly not to the natural secretions attending ovulation, in a healthy female. Four hundred years before the Christian era, Hippocrates, whose works are still extant and still regarded as the best authority, recommended the period of what is now called ovulation as the proper time for prolific sexual intercourse, viz: the first and last days of the so-called menstrual discharge, but not during the two or three days of its free and regular flow. He advised fasting on those days to his barren patients, and steaming the uterus with volatile aromatics, by means of an instrument we now call a speculum. To use no bathing or washing, but to wipe off the moisture with dry cloths perfumed, and even to cover up and retain the speculum in its place, and not to remove it until a minute before the embrace is received. Then to lie still, with the limbs crossed, not to make much exertion, or sudden motion of the body, and to live on a plain and very light diet for five or six days, to prevent the sporangium from perishing and being extruded. If not extruded then, the mouth of the uterus is sealed. There is then no danger or inconvenience from subsequent embraces. Twins are supposed to be the product of a second embrace, before the orifice of the uterus has been sealed. It may, however, endanger the first if it is not successful. After impregnation, the fecundating fluid does not reach the cavity of the uterus at all. It either returns, as any other foreign substance, or is absorbed. The vaginal walls, in a healthy state, possess great powers of absorption. Medicine can be given that way. When the stomach is irritable and the patient requires cod liver oil, it answers as good, or a better purpose to inject it into the vagina, or introduce a wad of cotton or wool saturated with it. During pregnancy and lactation, and also during the three weeks between the periods, it matters but little whether the fecundating fluid be retained or not.

Some women, about five in one hundred, are impregnated at some time or other of the three weeks from some stray ovum which has been extruded from the ovaries under some strong excitement out of its proper time. Moist and phlegmatic women who cohabit often had better soak out the fluid with a dry cotton tampon, otherwise health and delicacy require nothing to be done. Many women improperly use injections of cold water or other things. They hurt themselves by injections of an astringent kind, particularly lead, white vitriol, alum, oak-bark, and such substances. They tan the mem-

brane and destroy its natural elasticity. It is a common error to use such injections for the fluor albus and for laxity of the parts. The better plan is to encourage the flow by warm mucilaginous applications, to enable the engorged vessels to empty themselves. Then the discharge, an effect of the engorgement, will cease, and the elytron will contract and regain its usual healthy elasticity. Whereas, if the blood in the engorged capillary vessels of the vagina and uterus be brought to a status and fixed in the capillaries by astringents, the parts cannot contract, and the membrane will lose its pliancy and elasticity. The elytron's inner coat is rugose, in some respects resembling the rugosity of the scrotum. When healthy, it is more rugose than in a morbid state. Maladies of a grave kind destroy or nearly obliterate the rugosities of the scrotum. The relaxing and debilitating effects of hot weather act on both those parts pretty much alike. In woman, it is more necessary to keep cool than in the other sex. The practice of wearing drawers, or anything to heat the parts, is injurious. The thighs and hips are much larger in proportion in woman than any other parts of her body. In proportion to the development of a part, it bears heat less and cold more, because there is more blood distributed to it. There is more blood in that portion of the skin covering the face, and more glands, than in any other portion of the whole surface of the body ; and hence the face stands cold better than other parts. The neck and breasts of women are better supplied with glands and capillary vessels than men, and hence they have not the same need of covering. It would be unfortunate if this was not the case, as women would be liable to catch cold from exposing their breasts to nurse their children. The breasts, like the thighs, perineum, and labia majora, should be kept cool. A very large, plethoric woman, with large breasts, had been married some ten years and had no children, though she had had abortions. The treatment advised was, to release her breasts from the flannel garment that encased them and kept them hot enough almost to roast an egg. There was a febrile heat constantly maintained in the uterine region by wearing tightly fitting drawers, which actually excluded the air from the vulva. She was also advised to follow the letter of that portion of Scripture which forbids woman from using man's apparel, or any garment resembling breeches, and consequently to take off her drawers, in obedience to the true original meaning of the Mosaic law, and not fear the

cold. She did so, and soon conceived and had a healthy child. Long stockings to protect the legs, fastened above the knees, are necessary. Thin pliant shoes are better than bootees. India rubber or thick soles are not as healthy as thin, when not walking on damp ground, as they sweat the feet. The thighs should be bare, and the dress should stand off from them. But the body should be kept warm. The breasts should not be pressed upon by the dress so as to be immovable, and the upper part should have air, and be very thinly and loosely covered. Nipples, which have been pressed into the breasts and excluded from the air by a false mode of dressing, become so tender and irritable as to bleed and cause the fashionable young mother to scream every time the child's mouth is applied to them. A little learning is a dangerous thing—uneducated women seldom suffer with their breasts during lactation, nor do the highly polished and thoroughly educated. Two-fifths of all the food and drink pass out by the exhalents of the skin. The integuments covering the pudenda, thighs and nates, throw off more effete matter than any other portion of the periphery of the body. If the effete matter does not pass off by the skin, it causes congestions of the womb and other viscera, and is thrown off by a discharge from the mucous membranes, or is exhaled from the lungs, poisoning the breath, or is cast off by the kidneys, causing great irritability in the bladder and inability to retain the urine. Less harm is done when it passes off by the utero-vaginal membrane in the form of what is called whites. If the latter be dried up by astringents, without removing the cause, the health and complexion of the woman will suffer, and her power of giving and receiving conjugal pleasure will be impaired. The utero-vaginal membrane is best cleansed by its own secretions. Cold water injections fix the matter that ought to pass off. Soap and water immediately relaxes the elytron and destroys the natural acidity of its mucus.

The vulva needs exposure to the external air, as it is the embouchure of a deep current of gaseous and volatile matters from innumerable conduits, follicles and lacunæ of an extensive number of exhaling organs, which have no other outlet. When that is obstructed by tampons or pessaries, by perineal bandages, or any anti-scriptural mode of dressing, the vaginal secretions not only become highly offensive, but so putrescent, tenacious and acrid as to destroy the vitality of the spermatozoids of the liquor seminis, or greatly to impede their motion.

The pudic region not only requires air but fanning, which is effected in walking, by the flapping of the loose skirts of woman's garments when properly dressed. Hence, sedentary women are less prolific and more subject to uterine diseases and offensive odors, than those who volatilize and dissipate the effete excrementitious humors of the generative system by the fanning exercise in walking. The tenacity and puriform nature of the vaginal secretions, necessarily occasioned by sedentary habits and an improper, unscientific, anti-scriptural mode of dressing, is a great impediment to the fertilization of the ova, even if those gross acrid humors did not destroy the viability of the zoospermes. It impedes their motions. Their own fluid, the liquor seminis, is too tenacious to admit of their free motion until it is made thinner and lighter by being mixed with a tenuous watery, almost gaseous fluid, secreted by the ovaries and the utero-vaginal follicles, which adds greatly to their motivity. The indelicate and hurtful practice of using cold water injections or astringents under the mistaken idea that they are demanded by cleanliness and give tone to the parts, actually cause what they are intended to prevent, and by astringing the conduits of the follicles, stop up the channels of that thinner and lighter fluid, intended by nature to remove the tenacity of the liquor seminis, and to give activity to the spermatozoa. Women who have abused themselves in this way, or who are afflicted with uterine maladies, are seldom conscious of any discharge from themselves in sexual embraces. They have little or none, and are not susceptible of any very high degree of pleasurable excitement; while with others it is very abundant, and so light and gaseous as to gush forth under strong excitement like liberated gas or ether. It is important to insure the fecundation of an ovum that it meet and commingle at the same instant of time with the liquor seminis containing the zoospermes, to which it imparts a most active motion. That portion of it which comes down from the ovaries washes out the mucosity obstructing the Fallopian tubes and blocking up the mouth of the womb, thereby opening all the conduits to the self-locomotory germs to seek the ova. In a healthy condition of the organs concerned in reproduction, love alone is generally sufficient to insure that sympathy and harmony between the parties so essential to carrying out the designs of Nature in perpetuating the species. But there are temperaments so different, and so many functional derangements of the

generative system, as utterly to annul the harmonizing influence of the purest conjugal love, unless science be invoked to remove the physical causes obstructing the play of the sexual sympathies between the parties.

PROGRESS OF MEDICINE.

ART. I.—*On the Perceptive Power of the Spinal Cord, as manifested by experiments on Cold-blooded Animals:* By GEORGE PATON, M. D., of Galt, Canada West.

[THIS original communication which appeared in *The North American Medico-Chirurgical Review*, for May, is too long to be admitted entire in the present issue of the N. O. Med. and Surg. Jour. The following interesting extracts, will, however, indicate its general import.]

Dr. Marshall Hall proceeded further than M. Flourens in accounting for the movements of an animal deprived of its cerebrum. Pursuing the views that had been advanced by Prochaska respecting the reflex action of the spinal cord—that an impression on the extremities of the *afferent nerve* is conveyed by that nerve to the medulla, and reflected by the *efferent* or *motor nerve* of the part; and that this is the principle on which the movement is effected; he reduced this class of movements into a system, and illustrated their importance in the functions of the body. He showed that the contraction of the iris, the act of deglutition, the associated movements of sneezing, coughing, defecation, etc., all depended on the principle of reflex action; that it was the impression on the *afferent* or *sensory nerve*, conveyed to the medulla and reflected by the *efferent* or motor nerves of the part, by which the movement was produced, each being excited by its appropriate stimulus, as contraction of the iris by the application of light to the eye, etc. But he generalized these views so far as to maintain that this is the character of all those movements that are witnessed in decapitated animals; and concluded that the spinal cord is a distinct system in itself, and the seat of reflex action; and that the brain is the centre of the senso-volitional movements; each organ performing its specific function in the animal economy.

Mr. Granger endeavored to corroborate the views of Dr. Hall, by his investigations into the structure of the spinal cord. He stated that he could trace some of the fibres of the anterior and posterior roots of the nerves into the cineritious matter of the cord, which he considered to be connected with the reflex function ; and that the other nervous fibres did not enter the substance of the cord, but proceeded, as the medullary fibres, to the brain, and that these constituted the senso-volitional system. Hence, each spinal nerve contained both reflex and senso-volitional fibres.

1. In reference to these views, we may state that the theory of a true spinal system is decidedly opposed to the movements performed by cold-blooded animals after removal of the cerebrum. We shall afterwards see that these movements indicate a power resident in the spinal cord distinct from that of reflex action.

2. There are circumstances connected with the reflex movements of the body which do not coincide with the theory of Dr. Marshall Hall.

We admit that an important class of movements of the body depend on reflex action ; that contraction of the iris, deglutition, respiration, etc., are produced by the impression on the *afferent nerve* being conveyed to the medulla, and reflected by the efferent or motor nerve of the part ; and that these movements take place independently of sensation. But if by the reflex function it be understood that the motor nerve is always excited by the afferent nerve without the intervention of sensation, then we dissent from the view, for there is a class of reflex movements into which sensation appears to enter as an essential element, as necessary to constitute the act. A doctrine opposed to that of a true spinal system.

The theory of an excito-motory system, brought forward in explanation of these facts, has been considered premature, and as by no means resting on data sufficient to command the general assent of the profession. It does not follow, as we shall afterwards see, that, because reflex movements may be excited in the lower portion of the cord in cases of disease, that this is the specific function of the whole cord, after ablation of the cerebrum. And the investigations into the structure of the spinal cord, which were formerly adduced in support of these opinions with as much boldness as if they had been mathematical truths, cannot be relied on, as they appear to have proceeded on previously assumed views, rather than to have been directed by strict observation and a rigid induction of facts. The recent microscopic observations of other physiologists respecting the structure of the spinal cord, differ materially from those propounded by Mr. Granger in support of Dr. Marshall Hall's theory.

Dr. Paton proceeds to give a summary of the recent, minute anatomical investigations into the nervous system by German and other physiologists, showing that—

These researches into the structure of the spinal cord must, in a great measure, supplant the views of Marshall Hall and Granger, as they show that all the roots of the motor nerves, including the senso-

volitional fibres, take their origin from the gray substance of the spinal cord.

But it is not the minute structure of the spinal cord that we have to determine in order to establish its perceptive power, though that is of great importance in itself, and may be urged in corroboration of the doctrine. But it is the specific functions of the cord as manifested by an animal after ablation of the cerebrum, or, in other words, the movements which an animal in this state is capable of performing. And we maintain that an element enters into these movements perfectly distinct from that of reflex action, and which cannot be accounted for on any other principle than that the animal manifests distinct power, and exerts a definite control over the muscles of voluntary motion in taking cognizance of the irritation to which it is subjected.

And in determining the character of a reflex as distinct from that of a perceptive movement, it is to be observed, that a reflex movement being produced by the impression on the afferent nerve conveyed to the medulla, and reflected by the motor nerve, always takes place in the same manner—admits of no change or variation—and may be termed a uniform sequence of that which preceded it. Hence there can be no evidence of design, nor of any determinate movement exerted at the moment to the attainment of a specific object; the adaptation of means to ends connected with these functions of the body being inherent in the structure of the part, and not dependent on the will of the animal.

But in perceptive movements, when the impression on the afferent nerve reaches the medulla, the motor nerve is excited by a distinct act of volition, and the movements performed are varied in accordance with the circumstances of the case and the degree and mode of irritation; for the muscles on which these movements depend are not limited or restricted in their action, but subject to the control of the will for the very purpose of accomplishing our desires and intentions. Hence they are termed the muscles of voluntary motion—engaged in walking, leaping, the varied and combined movements of the limbs—of the larynx, etc.; and every specific act performed by this class of muscles, in which we witness design and adaptation of means to ends, must be considered the effect of a controlling power or agency, as there is nothing in the structure of the parts to account for it.

On the other hand, when reflex action is excited in the muscles of voluntary motion, as in the limbs by irritation of the integuments, the movements consist of a successive contraction and relaxation of the muscles of the part, similar to what we observe in convulsions or the application of galvanism; but there is nothing like design, or the attainment of a definite object in the movements.

From these statements, then, it will appear that in determining the character of the movements of an animal after ablation of the cerebrum, we must not limit our views to the existence or non-existence of sensation, which has too frequently been done by physiologists in discussing this question; for sensation, according to the strict and

philosophical import of the term, expresses the mere feeling, or state in which a sentient being exists at the moment, without reference to the cause that produces it. And we maintain that we can have no evidence of sensation in an animal subjected to experiment, except so far as we observe proofs of perception or recognition of the stimulus. An animal may feel on being touched, but we can have no knowledge of the fact, unless it be manifested by the particular movements which it executes at the moment in response to the irritation, and this necessarily implies a certain degree of voluntary power exerted by the animal in regulating and controlling its movements to the attainment of an end—which phenomena we define by the term perceptive movements.

Another circumstance on which we must insist, of great importance in order to arrive at a proper decision on this subject, is, that we must base our views on strict observation and a rigid induction of facts. We cannot ascertain the functions of the brain or spinal cord, except so far as these are manifested by experiment or pathological research. And it is contrary to every principle of logic and sound philosophy to suppose that because an animal is deprived of its cerebrum it can give no evidence of perceptive power. We can assign no reason, so far as our investigations into the structure and functions of the nervous system are concerned, why the cerebrum should be possessed of this power, and not the medulla oblongata and spinal cord. The question is, what are the facts of the case? "The foundation of all human knowledge," says Mr. Stewart, "must be laid in the examination of particular objects and particular facts, and it is only so far as our general principles are resolvable into these primary elements, that they possess either truth or utility."

What then, are the phenomena manifested by an animal after removal of the cerebrum or brain? Have we evidence that it is capable of performing distinct perceptive movements?

Experiment 1.—I removed the cerebrum of a frog with great care, and observed the phenomena. Respiration continued. The frog no longer manifested spontaneous motion, but remained in a shallow vessel, amid a little water, with its hind legs drawn up, the posture that frogs assume when they rest; but on being irritated, moved with great vigor, and gave every indication of recognizing the stimulus. On being seized by the foot, it struggled much to be relieved, and on being freed, bounded from the grasp, and made several leaps before it became quiescent. On irritating the integuments of its left cervical region, it croaked; on repeating the irritation, it again croaked, and scratched the part with its left hind foot. On touching with the point of a needle the integuments over the right scapula, it raised up its right hind leg, and scratched the part with its foot. On continuing the irritation, it again raised up its right hind foot, and pushed away the instrument with force.

Experiment 2.—Removed the cerebrum of a frog with great care, and allowed it to remain quiescent till the effects of the operation had subsided. Respiration continued. The frog had lost the power of spontaneous motion, as it did not move from the place on which it

was seated till irritated, but appeared most sensible to the touch, and gave every indication of recognizing a stimulus. When placed upon its back, it immediately turned upon its face, and made several leaps before it became quiescent. When I irritated slightly the integuments of its thorax on the right side, it pushed away the instrument with its right hind foot. On a stronger irritation being employed, it withdrew its body in the opposite direction, and leaped to a distance. When I compressed slightly one of the toes of its right fore foot, it withdrew its foot, and placed it below its abdomen. When I touched with the point of a needle the integuments of its left dorsal region, it scratched the part with its left hind foot. And when I touched with the point of a needle its right cervical region, it raised up its right hind leg and pushed away the instrument with its foot. In short, it comported itself as regards the sense of touch like an animal that had sustained no mutilation, the only difference being that a slight irritation was required, to be employed before the perceptive movements were manifested.

Experiment 3.—I removed the cerebrum of a frog, dividing the medulla oblongata a little anterior to the origin of the par vagum. After the operation a few slight respirations were observed which gradually ceased. I touched with the point of a needle the integuments of the right cervical region, and it quickly raised up its right hind leg and scratched the part with its foot. I irritated the integuments of its left dorsal region, and it raised up its left hind leg and pushed away the instrument with its foot, and then leaped to a distance, and made several successive leaps before it became quiescent. On irritating its right dorsal region, it withdrew its body in the opposite direction, and scratched the part with its right hind foot. In short, it gave every indicatiion of recognizing the stimulus, and endeavoring to avoid it.

I have performed the same experiment on many other frogs with similar results. The animal, after ablation of the cerebrum, losing the power of spontaneous motion, but continuing most susceptible to every irritation, and performing distinct perceptive movements in response to a stimulus. And experiments on the alligator, where the phenomena can be witnessed on a much larger scale, are equally conclusive in establishing the same doctrine.

"A large alligator (four feet long) being decapitated,* the headless trunk, as on many former occasions, performed numerous actions, indicative of sensation, intelligence and volition. Resting perfectly quiet, deprived of all the special senses, it possessed only the general sense of touch, and responded in an accurate manner to all tactile impressions, even the simplest. No extreme agent, neither pricking nor fire, was required to elicit definite and defined movements. The slightest touch with the finger seemed to be perceived by the whole

* The division must have been made about the origin of the par vagum, as Dr. Dowler informs us that respiration, though irregular, continued actually thirty minutes by estimation, after removal of the encephalon.

It is a well-known fact in physiology that respiration continues in a decapitated animal only when the origin of the par vagum has been respected.

trunk, the tail, and limbs, as perceived by their movements. The animal seemed to be aware of the nature of the touching body, which, if producing little irritation, was borne without any violent efforts to escape from it. But fire, punctures, etc., called into agonized action the body, limbs and tail. The body curved in a manner so as to recede from the offending agent, and the limbs were directed so as to remove it. From its actions, far more impressive than words, it was evident that it judged accurately, as to the degree, duration, and place of painful or painless impressions."—*Dr. Dowler on Nervous Action.*

We perceive from these experiments that an animal is capable of perception and the manifestation of volition, after being deprived of its cerebrum; and that it is not necessary that the impressions received by the nerves should reach the cerebrum that sensations may be produced.

The animal leaps on being touched, or raises its foot and scratches the part of its integuments that is irritated; or, if an alligator, directs its limbs to remove the cause of irritation, and judges most accurately concerning the stimulus to which it is subjected. And to assert that these are not senso-volitional movements, is to give a new definition to the term. There is in these acts—1, an evidence that the animal has perceived the impressions made on the extremities of the sensory nerves; and 2, a proof that it exerts distinct control over the muscles of voluntary motion, in regulating and directing them to the attainment of a specific end. Because if the animal did not feel, and had no power of volition, why should one limb be raised up, in this manner, in preference to every other, to the part that we touch with the needle? or why should the hind foot be moved forward to the identical spot in the dorsal region that is irritated? Movements perfectly similar to what we observe in other animals, when a particular part of their body is irritated.

It is no doubt true that the animal has lost the power of spontaneous motion, as it does not move till irritated. And it does not retain the recollection of impressions received by the nerves, as it does not learn to avoid the obstacles to its progress—functions which it is admitted belong to the cerebrum. But, in the absence of these powers, the animal evinces such design, and adaptation of means to ends in resisting every irritation applied to its body, that we must conclude it is capable of distinct perceptive movements.

From these experiments, we deduce the following doctrines:

I. That the spinal cord enables an animal to feel, and manifest its perceptions by performing definite and combined movements in response to a stimulus.

II. That the cerebrum is superimposed on the spinal cord to act on the possession of these powers, and as the seat of memory and the intellectual faculties.

III. That the associated movements of deglutition, respiration, sneezing, coughing, contraction of the iris, etc., depend on the principle of reflex action possessed by the spinal cord.

It will be observed from the preceding experiments, that our re-

searches have been confined to the functions of the entire spinal cord, and not of a separate section; as our object has been principally to ascertain if the movements performed by an animal after removal of the cerebrum be illustrative or not of the theory of an excito-motory system. And we consider that the phenomena we have described satisfactorily establish the doctrine that cold-blooded animals, after ablation of the cerebrum, are capable of sensation, and of performing distinct perceptive movements, on the application of a stimulus. And this is the proposition for which we contend.

But another important subject presents itself for consideration: what is the particular seat of this power? Does it belong to the medulla oblongata and upper portion of the spinal cord?

From the experiments we have performed on this subject, in dividing the spinal cord immediately behind the cranium, and removing every portion of the encephalon anterior to this, destroying at once the function of respiration, we maintain that the movements performed by an animal in this condition, on being irritated, afford distinct evidence of perceptive power. * * *

We do not believe that we have any proper criterion for determining the existence of sensation in animals subjected to experiment, except the manifestation of perception. For the movements of an animal are the signs by which its feelings and sensations are manifested to us, and we can only properly determine that there is sensation, when we have distinct evidence of perceptive power. And this, we conceive, is afforded us by the following experiments:

Experiment 1.—I divided the medulla oblongata of a frog immediately behind the cranium, and reviewed every portion of the encephalon anterior to this. Respiration ceased. Having allowed the frog to remain quiescent for a short time, till the effects of the operation had subsided, I irritated the extremities when the movements appeared most characteristic of design and adaptation. But the irritation caused the frog to perform progressive motion, as in the former experiments, so that a particular effect was produced by this division of the cord, viz: that the animal had lost the power of locomotion, and was unable to move from the place where it was sitting, as was the case with frogs when merely deprived of the cerebrum. I irritated, with the point of a needle, a toe of its left fore foot, and it quickly withdrew it and placed it below its thorax. I compressed with the point of the forceps a toe of its right fore foot, and it raised up its shoulder and endeavored to withdraw its foot, but being retained, it placed the toes of its right hind foot over the point of the instrument, and threw its body in the opposite direction. After a short interval, I irritated the integuments of its left dorsal region, and it scratched the part with its left hind foot.

Experiment 2.—I divided the medulla oblongata of a frog immediately behind the skull, and removed every portion of the encephalon anterior to this. The section was made over the origin of the first pair of spinal nerves which are distributed to the larynx and the tongue. Respiration instantly ceased. I allowed the frog to remain at rest for a short time, till the effects of the operation had subsided,

and then observed the phenomena. I compressed a toe of its hind foot with the forceps, and it withdrew its foot and placed it below its abdomen. I irritated the integuments of its right dorsal region, and it raised up its right hind leg and scratched the part with its foot, and then raised itself up and made an effort to move forward. I seized the integuments of the right side of its abdomen with the forceps, and it raised itself up as if to withdraw the part, but being retained, it placed both the fore and hind leg of that side over the part that was irritated. On touching its cloaca with the point of a needle, it drew up both its hind legs over the part that was irritated. I irritated slightly the integuments of its left cervical region, and it raised up its left hind leg and scratched the part with its foot.

Experiment 3.—I divided the spinal cord of a frog at the origin of the second pair of spinal nerves, which sends a branch to the larynx, and removed the encephalon, and portion of the cord anterior to this, as formerly. After a short time I applied irritation, and it was observed that only a slight movement could be produced in the anterior extremities, on account of the injury inflicted on the second pair of spinal nerves, which in the frog go to the fore legs. But on touching its hind foot with a needle, it immediately withdrew it from the source of irritation. On irritating the groin, it drew the thigh into close contact with its body. On seizing the integuments of the side of its abdomen with the forceps, it moved forward the hind leg of that side and scratched the part with its foot. On irritating the integuments of its abdomen on the opposite side of its body, it moved forward its hind foot forcibly to the part, drawing its body in the opposite direction. I then seized the upper part of the dorsal region on the right side with the forceps, and it actually raised up its hind leg and scratched the part with its foot. I continued the irritation, and it repeated the movement. I next touched with the point of a needle the left dorsal region, and it raised up its left hind leg and passed its toes over the part

I have repeatedly observed the same phenomena on a similar division of the spinal cord ; the animal affording evidence of the recognition of the stimulus, by raising up its leg and scratching with its foot the part that was irritated. But an interval frequently required to elapse before the irritation was renewed, as the animal was weak, and its nervous energy soon exhausted. But on a lower division of the cord the phenomena became much less distinctly developed, and in the inferior portion of the cord we could seldom excite any but a few spasmodic movements of the limbs on applying irritation to the extremities.

These experiments, we conceive, warrant us to conclude that cold-blooded animals, after decapitation are capable of recognizing a stimulus, and performing distinct perceptive movements, and that this power is seated in the medulla oblongata, or upper portion of the spinal cord. When an animal, after losing the power of locomotion, lifts its foot on being touched with a needle, and places it below its thorax or abdomen, or raises it up and repeatedly scratches the part of the integuments that is irritated, we must admit that it feels pain,

and perceives the source of irritation. In short, that it manifests cognizance of the stimulus, by the definite and combined movements that it performs.

But this view of the subject has been objected to by some physiologists, on the ground that as we have distinct evidence of design and adaptation of means to ends in many of the reflex movements of the body, such as contraction of the iris, the associated movements of sneezing, coughing, defecation, etc., so the movements of design manifested by a decapitated animal when it lifts its foot and scratches off any cause of irritation from its back, etc., may be of the same character. But there is a great and manifest distinction between the two cases. In the contraction of the iris, and the associated movements of sneezing, coughing, etc., we have an adaptation in the structure of the parts to the particular function which they perform, or an evidence of design in the Author of nature endowing these parts with the property of contracting on the application of a particular stimulus, and to the attainment of a specific end, just as we observe in the contraction of the heart, etc. But these movements afford no evidence of the adaptation being the design of the animal, as they are merely a uniform sequence or contraction of the parts from a particular irritation; every successive movement being a repetition of the former, and performed without change or variation, these cannot be considered a proof of perceptive power. But when an animal lifts its foot to scratch off any cause of irritation from its back, it does not result from any particular structure of the parts to that particular end, as that is only one of a multitude of movements which it might have performed; thence the evidence which it affords is that of being a perceptive act, or of design on the part of the animal. And this has universally been considered the data by which we determine the character of a perceptive movement in an unmutilated animal. When we, therefore, see a decapitated frog lift its foot in a similar manner, and scratch the part that is irritated, are we not bound to conclude that we have here as distinct evidence of perceptive power as in the former case, and that the movements are identically the same? For if this be not a perceptive movement, it undoubtedly bears all the characteristics of those that have otherwise been considered as such. And then we must admit that in a decapitated animal a definite end is accomplished by indefinite means, which certainly appears to be very remarkable.

There are other remarkable phenomena observed in experiments on the salamander, to which we must refer in corroboration of the views we maintain, and as decidedly opposed to the theory of an excito-motory system. The abettors of that system consider that after the spinal cord is cut off from all influence and communication with the brain, it is only reflex movements that can be produced, and that this is also the case with distinct sections of the cord. When, for example, the spinal cord of a higher order of vertebrated animal is divided below the brachial plexus, the animal is immediately seized with paralysis in its lower extremities; and the reflex movements excited in the part by irritation of the integuments, are of a general and in-

definite character, and consist of alternate contraction and extension of the limbs. But when the spinal cord of a salamander is divided below the brachial plexus, the animal does not suffer paralysis in its lower extremities, but continues to move them as before, exerting distinct power over them, as in the act of locomotion, lifting up one foot and then the other, in accordance with the movements of its fore legs, as it crawls slowly along the surface of the table.

If the excito-motory theory be based on fact—if it be a general law in vertebrated animals that the spinal cord is capable only of reflex action, how does it happen that the animal is able to perform the movements of locomotion after division of the spinal cord?

I took a salamander (*Lacuta aquatica*, Linn.) and divided the vertebræ and spinal cord immediately below the brachial plexus, which, in that animal, as in the frog, is the second pair of spinal nerves, and sends a branch to the larynx; so that the animal retained complete power over the movements of its anterior extremities; and after allowing the animal to remain quiescent a short time, till the effects of the operation had subsided, observed the phenomena.

The animal raised itself upon its fore legs and began to move forward, but did not drag its hind feet like an animal that had suffered paralysis, but supported its body on them as on its fore legs, and exerted them distinctly in the act of locomotion. I could observe no difference between these movements and those which it performed before the division of the cord, except that it now walked with less power and energy. I allowed the animal to remain at rest for a short time, and then slightly touched with the point of a needle the integuments of the right dorsal region, and it raised up its right hind foot and passed its toes across the part. On irritating the integuments of the left side of its abdomen, it raised up its left hind foot again and again to the part. After a short interval, I touched with the point of a needle the upper portion of the left dorsal region immediately below the division of the cord, and it raised up its left hind foot and passed its toes distinctly over the part. I continued the irritation, and the animal repeated the movement, raising up its left hind foot and passing its toes over the part.

Experiment 4.—I took a salamader (*Lacuta aquatica*, Linn.) and divided the vertebræ and spinal cord immediately below the brachial plexus, allowing the animal to retain perfect power over its anterior extremities. I then removed a small portion of the vertebræ from the lower division of the cord, so that the divided ends remained distinct and separate, and observed the phenomena.

After the animal had remained at rest for a short period, it raised itself upon its forelegs and commenced to walk, and exerted its hind legs most distinctly in moving along the table. It raised up one hind leg and threw it forward, while it retained the other in its position, in correspondence with the movements of its fore legs, as it performed locomotion. But it walked more slowly and with less vigor than before the division of the cord, and during this period the divided ends of the cord remained distinct and separate. I touched with the point of a needle a toe of its right hind foot, and it withdrew it and placed

it along the side of its abdomen. I irritated the integuments of its right dorsal region, and it raised up its right hind leg and passed its toes over the part. I touched slightly with the point of a needle the integuments of the left dorsal region, a little below the division of the cord, and the animal raised up its left hind foot quickly to the part. I continued the irritation, and it passed its toes distinctly over the part; but it did not move its posterior extremities till a part of its integuments was irritated, and then it gave the most unequivocal evidence of recognizing the impression.

I have performed the same experiments on many other salamanders with similar results,* and I have, after division of the spinal cord below the origin of the brachial plexus, removed the upper portion of the cord as far as the occiput, destroying all power of motion and sensation in its anterior extremities; and on touching with the point of a needle the integuments supplied with nerves from below the division of the cord, the animal has raised up its hind foot and passed its toes over the part. I have, after this, removed the encephalon, and on applying irritation to the integuments, as the side of its abdomen or dorsal region, the animal has still continued to recognize the stimulus—has raised up its foot and passed its toes distinctly over the part that was irritated. But the movements were weaker, and an interval required to elapse before they could be renewed.

On a lower division of the cord, this power becomes less manifest, and the reflex action more particularly developed.

We are anxious to urge these facts on the consideration of the profession, as our object is to elicit the truth, and determine the specific functions of the spinal cord, irrespective of any particular views. And we think we are entitled to demand an explanation of these facts from the abettors of an excito-motory system, and to ask on what principle they account for the phenomena. The movements must either be reflex or they must not. If the former, then how do they differ so essentially from all reflex movements that have been recorded, as in none of these cases has the power of locomotion been retained on a separate and similar division of the spinal cord. And if they are not reflex, how do the supporters of the theory maintain their views after the manifestation of such phenomena? We, therefore, consider it an established fact in physiology, that in salamanders, after the division of the spinal cord below the brachial plexus or second pair of spinal nerves, the animal manifests recognition of an impression, and distinct power over the movements of its posterior extremities in locomotion.

If we maintain that the spinal cord is constructed on the same type in vertebrated animals, we must arrive at the general conclusion that there is a power resident in the medulla oblongata, or upper portion of the spinal cord, which enables an animal to recognize impressions and perform movements in response to them.

* In a paper which I published in the Edinburgh Medical and Surgical Journal, No. 167, I have given a full account of similar experiments performed on the salamander, and their results.

ART. II.—*Prone and Postural Respiration in Drowning, and other forms of Apnœa or Suspended Respiration.*

[SUCH is the title of a book of two hundred and sixteen pages, by the late Dr. Marshall Hall, recently published in London, having been edited by his son, and reviewed in the leading article of *The British and Foreign Medico-Chirurgical Review*, for April, 1858. The practical portion of the review concerning the success of "Dr. Hall's easy method," will be subjoined, the method itself having been given in a preceding Number of the *N. O. Med. and Surg. Jour.*]

The importance of artificial respiration is so evident, and the readiness of Dr. Hall's method is so apparent, that we cannot but regret that its adoption should be interfered with rather than advanced by assertions such as the following:

"This measure (the warm bath) is perfectly useless, not to say injurious, unless artificial respiration be simultaneously administered; and this administration is incompatible with the posture implied by the use of the warm bath. *To use the warm bath is, therefore, to renounce the only hope, the only remedy, in apnœa.*" (p.34.)

In the sequel, we shall see that, judged of by experience, such is not a warrantable conclusion.

The object of treatment in apnœa is, Dr. Hall continues, first to eliminate the carbonic acid already in the blood, and secondly to check its further formation, and for this purpose artificial respiration is *the* agent. Here follows the most important part of the work before us:

"When the subject is kept in the *supine* position, events occur which render *every* attempt at inducing respiration absolutely nugatory; the tongue may fall backwards, carrying with it the epiglottis, and close the glottis or entrance into the windpipe and air-passages! Fluids already in the mouth or fauces, or regurgitated from the stomach, may not only obstruct the air-passages, but be forced or drawn back into the windpipe, and so add a *new* source of apnœa.

"These obstacles are obviated at once by reversing the position from the supine to the *prone!*" (p.23.)

Experiments, first made by Mr. George Webster, are adduced to show that when the body is placed prone, inspiration and expiration can be readily produced and continued at pleasure by the mere exercise and relaxation of pressure, such alternations being easily accomplished by rotating the body from the prone to the lateral posture. Mr. Fox, Mr. Hunter, and Mr. Bowles, (late of St. George's Hospital), performed further experiments, and, measuring somewhat roughly the quantity of air respired, found it sufficient. Rules for the treatment of persons apparently drowned or asphyxiated, are then given; but as they have been frequently printed, and are now familiar to all, we will not insert them. Dr. Hall then passes on to the consideration of "secondary apnœa," the "consequence of the poisoned state of the blood," which may show itself on the same, the second, or third day.

The portion of the work already epitomised constitutes Part the First, which "is nearly a verbatim reprint of the Essay presented to the Royal Humane Society in January, 1856." The report of the Society thereon is quoted at the commencement of Part the Second. Dr. Hall's rules, it is stated, were sent

> "To each of the Society's medical assistants, ... accompanied by a circular letter, calling on each to give his opinion on the proposed new method of treatment. Ten replies (the Committee reports to the Governors) have been received, including one from Sir Benjamin Brodie, and one from Dr. Christian, M.D., Acting Surgeon to the Society's Receiving House, Hyde Park, and from the preponderating opinions gathered therefrom, the Committee are advised to pause before adopting the new method recommended by Dr. Marshall Hall until it has been proved by the test of successful experience." (p. 46.)

We do not see that the Committee could have acted much more advisedly in the matter; but "the delay is homicidal," said Dr. Hall, "there is no need of a successful experience in a matter so simple; . . . . it is a question for unsophisticated common sense."

Now, at the close of 1857 (two years from the reception of Dr. Hall's Essay), the directions of the Royal Humane Society hanging on the trees in Hyde Park, contain no hints at artificial respiration, nor any description of means, postural or otherwise, for its performance; whereas their fuller directions contained in the "report," advise a clumsy method, which requires apparatus for its performance, and is then often ineffectual. The terms used by Dr. Hall may not have been justified by the course which the Society adopted in 1856; but the continued neglect by that Society of the "ready method" is matter for grave censure. For, allowing that Dr. Hall's views upon the application of the warm-bath were to some degree theoretical, and contrary to the practice of the Royal Humane Society's agents—allowing that these views do require the "test of successful experience," that Society admits the importance of artificial respiration, and is without excuse for its omission now that a ready and safe method for its performance has been discovered.

The new-born infant survives the absence of respiration longer than the adult, because, says Dr. Hall,

> "The excitability of the spinal system, and the irritability of the muscular system, exist in the highest condition, according to a law of animal life which I announced some years ago—viz: that these faculties are, throughout the animal kingdom, *inversely as the stimuli*." (p. 56.)

Dr. Hall then refers to those earlier researches of his which led to the production of two papers before the Royal Society, On the Ratio between Vital Dynamics and Stimuli, which, if they do not stand the searching scrutiny supplied by a more extensive zoology, are yet most interesting and instructive specimens of the mode in which a great mind reduced to at least temporary order and utility a vast array of apparently unconnected facts.

The practical inference drawn from this law, in the work before us, is that efforts at the resuscitation of new-born infants should be more

prolonged than in other cases, inasmuch as there is more hope of their recovery from suspended animation. Of this fact there is no doubt, whatever the true explanation may be. Dr. Hall has suggested the employment of a hot-bath as an *excitant* of respiration, its use being sudden, and alternated with that of another bath at a lower temperature.

In order to ascertain positively that the movements of rotation were sufficient to cause respiration, Dr. Hall devised a little instrument by which the quantity of air introduced could be measured. The gentleman already mentioned as having assisted Dr. Hall, found that twenty cubic inches of air were exchanged by pronation and semi-supination; and that although alternate pressure and relaxation of the chest-walls, when the body was in a supine position, would sometimes effect a certain amount of inspiration, yet in other cases it failed, and that it was always more effectual in the prone.

Another suggestion in Dr. Hall's work is that the inhalation of pure ammonia may be of service in apnœa by combining with carbonic acid, and removing the latter as carbonate of ammonia.

Further, Dr. Hall recommends the employment of postural respiration in narcotic poisoning in the laryngismus of convulsion and epilepsy, or after paroxysms of pertussis, and in the laryngismus of strychnia-poisoning; and after giving some hints upon the prognosis in apnœa, brings forward numerous cases in support of the views advanced. These cases have already appeared in the medical journals, but some analysis of their contents will be of interest.

Twenty-nine cases are recorded, and of these eight are instances of drowning, one of asphyxia from the falling in of earth, one of narcotism, and two of poisoning by chloroform; the remainder are cases of still-birth.

In order to exhibit the value of the cases of drowning, we have extracted their more important particulars, and represented them in the following table:

Age.	Duration of Submersion.	Commencement of Treatment.	Previous Treatment.	Condition when seen.	Result of Marshall Hall method.
2½	.....	½ an hour.	Warmth and friction.	Occasional sob, or gasp with quiver; pupils dilated; no pulse.	In 40 minutes respiration established; recovery
13	15 to 20 minutes.	Immediately.	....	Appeared quite dead.	In 15 minutes respiration; recovery.
Young man.	.....	1 hour.	Warmth; friction; brandy.	Spoke after extrication; but blue, cold, and rigid, when seen: no respiration; no pulse.	In 12 minutes respiration established; recovery.
5	15 to 20 minutes.	A few minutes.	Warm bath; warm blankets.	Livid; pupils dilated; no respiration; no pulse.	No effect after 65 minutes; death.
32	? whether submerged at all.	Soon.	.....	"Dead; in other terms, asphyxiated."	In 60 minutes respiration established; recovery
7	5 minutes.	Just after.	.....	Respiration suspended; action of heart inaudible; cold; livid.	In 30 minutes respiration; recovery.
11	5 or 6 minutes.	Directly.	.....	Appearance, dead; livid; no respiration.	In 2 or 3 minutes some respiration; in 10 minutes regular respiration; recovery.
16	7 minutes.	Immediately,	....	Inanimate; no respiraion; no pulse.	In 5 to 7 minutes respiration; recovery.

Here, then, are eight cases of drowning, the periods of submersion varying from five to twenty minutes. In six cases there were apparent asphyxia; in two, the symptoms were equivocal. In seven cases this method succeeded; in one it failed.

In order to estimate the real value of this method, let us compare the above results with the following, extracted from one of the earliest reports of the Royal Humane Society's proceedings. We have taken the first eight cases of drowning with regard to which anything more than the simple fact of recovery is mentioned in the report for 1783.

Here, then, are eight cases in which the periods of submersion appear, if the statements can be relied upon, to have been protracted from ten to twenty minutes, and yet all recovered, the most promi-

nent element in the treatment being friction, no allusion to artificial respiration occurring, except in one case.

The same report contains a more or less detailed account of sixty-eight cases in which the means recommended by the Royal Humane Society were successful; also thirty-five cases in which life was not restored; but in almost all instances in this second series, the periods of submersion were so long, when known at all, that any chance of recovery was precluded.

Age.	Period of Submersion.	Condition.	Treatment.	Result.
8	10 minntes.	Apparently dead.	Friction and warmth.	Recovery.
8	Some minutes.	Without any appearance of life.	.....	Ditto.
4	20 minutes.	Livid; beyond hope.	Friction.	Ditto.
17	.....	Quite insensible; no respiration.	Ditto.	Ditto.
Young man.	20 minutes.	Hopes vain.	Friction and artificial respiration.	Ditto.
Boy.	10 minutes.	Dead; cold; no respiration; no pulse.	.....	Ditto.
Child.	10 minutes.	Livid.	.....	Ditto.
Gentleman.	10 minutes.	Cold; no pulse; no respiration; appeared totally dead.	.....	Ditto.

During the bathing season of 1856 there were forty-six cases of "drowning" in the Serpentine. The measures employed by the Royal Humane Society's agents were successful in thirty-five, the eleven unsuccessful cases being reported as "beyond recovery." The period of submersion is not mentioned in the report; but of the eleven fatal cases, six were instances of suicide, one "accidentally fell into the water," three were "exhausted while bathing," and one was "found drowned." It is therefore probable that in at least seven of the cases the period of submersion was considerably prolonged.

These facts speak for themselves; and we only add that, while fully appreciating the value of the "Marshall Hall method" of performing artificial respiration, we cannot but think that the cause of science and of true humanity alike demand that "the test of a successful experience" be applied to the method suggested, before it is made to supplant and exclude entirely that which has for many years passed the test, and which already numbers its instances, not by units, but by tens of thousands.

It appears, then, from the facts before us, that each of the two systems is attended by success. The Royal Humane Society directs its attention mainly to the circulation; Dr. Marshall Hall principally to

the respiration; and as both functions are essential to life, it is evident that the restoration of the one does in many instances secure the activity of the other.

Art. III.—*Wines, and their Uses.*

[The following extracts from an article of twenty-one pages in *The British and Foreign Medico-Chirurgical Review*, for April, 1858, are worthy of the consideration of practitioners of medicine who now, more than ever, prescribe wines and other stimulating liquors for curative purposes. A quarter of a century ago, the works of Broussais were actually adopted as text-books in some instances, and were approved by many. The fundamental principles of his system are, "that *all disease are primarily local*," (*Path.* 31); and that "inflammation of the stomach and bowels constitutes all idiopathic fevers," etc. These paved the way for his antiphlogistic theory of therapy, namely: "The debilitants for arresting inflammations, are bleeding, abstinence, and emollient and acidulated drinks; *but the most efficacious of all is blood-letting.*" (513.) This last is generally carried to syncope! Hæmorrhages, scrofula, intermittents, yellow fever, consumption, and so forth, yield to the shedding of blood, but are aggravated by stimulants and diet.

During the prevalence of Broussaisism, (when some physicians thought a few teaspoonfuls of chicken water quite too stimulating and even dangerous in fever) the following anonymous maxim was held good:

"To those whom fevers burn, the smell of generous wine is death and hell."

Wine-wisdom is now a fundamental part of therapy. To "feed a fever," or to wine a fever, is no longer "positively shocking." Great is change.

The blazing star of Broussais, which crimsoned the realms of pathology with its inflammatory hues, quenched at length in the sea of blood whither it guided, guides no more. Mighty is progress.

Seeing that many physiological chemists and medical practitioners agree in theory and practice concerning the value and efficacy of

wines and spirits, it is highly important that these remedial agents should be studied with as much care as any in the materia medica, and the more so because they are more generally adulterated than any other preparations in the whole circle of the pharmacopœa. The wine which Homer calls "precious," was old, strong, unadulterated, and admitted of dilution with twenty parts of water.

—— "Twelve large vessels of *unmingled* wine,
Mellifluous, undecaying, and divine!
Which now, some ages from his race concealed,
The hoary sire in gratitude revealed.
Such was the wine; to quench whose fervent stream
Scarce *twenty measures* from the living stream
To cool one cup sufficed: the goblet crowned
Breathed aromatic fragrancies around."
OD. IX.

To drink undiluted wine was regarded by the Greeks as drinking like the barbarous Scythians; consequently, Dr. Machaon, son of Æsculapius, brother of Podalirius, does appear to have been called to treat any cases of delirium tremens during the Trojan war.

Passing down the stream of time a few thousand years, it may be well to land at Cincinnati, where the "Sparkling Catawba" wine is made from a native grape originally found in North Carolina. This, as a medicinal wine, is probably much underrated. The exhilarating property of this native champagne is similar to that of the imported, while its intoxicating power seems comparatively slight, and its secondary effects less apt to be accompanied with sour stomach, nausea, head-ache, etc. Moreover, the risk of adulteration is, for obvious reasons, very slight. French connoisseurs do not value it highly on account of its acidulous character. (See *Rev. de Deux Mondes.*) Chemistry favors this idea, as the quantity of alkali necessary to neutralize its acidity is large compared with some allied wines of Europe. Nevertheless, experience in the sick-room is the best test. It is at least a pleasant wine. The quality and quantity of its acidulousness may, for many patients, enhance its curative action.

The Dry Catawba, compared with the Sparkling, though much cheaper, is acid, weak in spirit, not very palatable, and probably inferior as a medicinal wine.—B. D.]

1. *Free Acids* are found in all wines and in less variable quantities than perhaps might be expected. How is it, then, that our connoisseurs pronounce some wines "not acid?" How is it that we do not

taste in all sorts this large amount of decided flavoring? Simply because it is masked by sugar more in some than in others. Everybody notices a degree of tartness in even the finest Chatean Yquem Sauterne, yet its acid constituents are less than those of port, where they are undiscoverable, or of champagne, which is pronounced "sweet." This is almost entirely explained by the total absence of sugar from the first, by the presence of about twenty grains per ounce in the second, and of still more in the third. Who would have guessed that Lachryma Christi was in reality nearly as sour as ordinary claret? Yet such it appears to be by the table above drawn out from Gunning's experiments. It is clear that almost any amount of acidity may be concealed from the taster by sugar.

The effect of acids is doubtless to render the gustatory nerves more susceptible of other agreeabler savors. Wine neutralized by soda is sad mawkish stuff, and acidulous fruits are always the most delicious. But it also prevents the sugar from palling the palate, enables more to be borne in the liquor, and thus the peculiar advantages of the saccharine constituents to be enjoyed in larger proportion. There is also every reason to believe that the acid unites with other constituents of the grape, to develope during age those fruity ethers which are the chief attraction to the purchaser.

There is a marked difference in wines as regards the *sort* of acid found in them.

2. *Sugar* has been mentioned above as useful in rendering vegetable flavors more appreciable. Where these flavors are naturally poor and weak, it is essential that a large quantity of sugar should exist in the wine, or it is tasteless. The way in which wines come to be sweet is various. In the more perfect, such as the best Port and Madeira, the ripe grape is so rich in sugar, that the excess checks the fermentation before the whole is converted into alcohol. In others, such as Tokay and Malaga, part of the must is boiled down to form a stock of extra sugar, to be added for the same purpose. In other cases, the sugar is gained by allowing the grapes to hang on the vines till almost converted into raisins. In the many nameless vineyards which furnish the cheaper imitations of the above-named prime wines, cane sugar and potato sugar are added to the genuine juice during fermentation, at the discretion of the manufacturer; or alcohol is added in the shape of brandy, so as to check the further progress of the chemical changes. Of the artificial methods, the two former involve a loss of quantity, and therefore justify an addition of expense; the latter are dilutions of the peculiar constituents of the grape, and if designed for the purposes of deception, are of course fraudulent.

Now, as an almost universal rule, it may be observed that the grapes of warm climates are wanting in higher kinds of flavor, and therefore, that unless they are well ripened, and rich in sugar, the wine produced is of the worst possible quality. This is the case with the inferior Peninsular and Italian growths, which are never fit for exportation. All the better sorts which come into foreign markets—Port, Madeira, Sherry, Lachryma Christi, and the like, are full of sugar. Fortunately, the same warmth of climate which has a deleterious influence over

flavor, is favorable to complete ripening, and therefore (under proper farming) to the formation of sugar in the grape, so that at least a sound if not a delicate wine may be made. On the other hand, where the vine attains its extreme northerly limit, it develops its richest aromas, and on the Rhine and Moselle we find wines which are almost perfumes. But at the same time, the must is so poor in saccharine constituents, that there is only just enough to produce the alcohol necessary to keep the wine. We do not miss the sweetness, for there is enough flavor to be tasty without it. There seems a little doubt whether or no any wines are entirely free from sugar.

3. *Tannin* does not exist in grape-*juice*, but is abundant in the skins, stones, and stalks. Its introduction, therefore, is purely voluntary, and is intended partly to assist in the preservation of the wine, as will be explained further on, and partly also to give a masculine roughness of the flavor which prevents the palling of the palate. The greatest quantity of tannin is contained in the skin of dark purple grapes, and consequently the wines fullest of it are those which are made from such fruit, as Port, Burgundy, and Bordeaux. But, as aforesaid, the quantity of it is purely voluntary on the part of the manufacturer, and there may be made from purple grapes, wines of various shades of red, and even white wines ;* only, it must be remembered, that the tannin will diminish in an equal proportion. As a rule, then, let not customers who dislike a rough wine, insist upon a full red color, for if they do, the merchant must introduce some artificial dye-stuff. Also let them not suppose that roughness is an evidence of adulteration or badness ; if joined to other full qualities, it may be a proof of the anxiety of the farmer to preserve virtues which will increase by age, and so be an index that there is latent good in the wine.

4. There is present in all wines a peculiar characteristic color, a "vinous smell," not due to the alcohol, or to the sugar, or to the acids, for we may separate them and they do not smell of it. At the same time it does not exist in the grape, for the most delicate organs discover there no trace of it. It is familiarly called "bouquet," not because it is like flowers, but because it is diffused from the bottle as from a nosegay. A poetical proverb has compared it to a "sunbeam," so pure, so delicate, so intense, so transient a joy. We look to chemistry to give us a more distinctive name, and to elucidate in some degree the nature of this substance (if, indeed, it is one substance), which is present in all wines, and appears complete nowhere else except in wine.

It is strange, but appears nevertheless to be true, that the substance to which the production of this delicious odor is mainly to be attributed, is itself fœtid. The subject has had more light thrown upon it by the investigation of what are called "foozle oils," [fusel oils] than from any other source. These are well known to all persons familiar with the distillation of spirituous liquors, as unfortunately coming over with the alcohol, and causing much trouble and expense by the necessity for freeing it of them. A small quantity is bearable ; it is

*For example, Champagne is made from a purple grape by separating the skins.

malt or potato foozle oil which gives the peaty taste to whisky ; the the foozle [fusel] oils of grape, musk and grain, flavor brandy ; but nobody could drink either of these unless partially purified. The flavor of spirits derived from the extremely minute quantity of foozle oil in them is so strong, that their presence in adulterated wines can often be thus detected. The place of foozle [fusel] oil (hydrate of amyle) in spirits, which, fœtid in mass, is agreeable, at least to spirit-drinkers, in minute doses, is taken in wine by œnanthic ether, equally fœtid when isolated, yet apparently the main agent in the elaboration of the delicious bouquet.

Let it not be understood that œnanthic ether *is* the bouquet. By no means ; but it contributes to give a *vinous* smell (there is no other more accurate way to describe it) and to *bring out* the bouquet—just as musk itself, undetected, intensifies the odors of eau de Cologne and lavender water, or as garlic is used by crafty cooks to render dishes savory. It differs from all the other ingredients in the products of grape fermentation by being found nowhere else, and is not capable at present of being manufactured in any other way than either from wine or its lees. What, then, is œnanthic ether? It arises from the action of œnanthic acid upon alcohol ; just as the action of butyric acid (sour cheese) upon alcohol produces the fragrant butyric ether, the "pine-apple flavoring" of our confectioners.

It is remarked, that if grape-juice be carefully purified by filtration through charcoal, it will indeed work as actively as usual, and form alcohol, but the resulting wine will not have a particle of bouquet or aroma. This favors the idea that the fatty acids are not the product of fermentation, but are derived from the oils of the skins, stones or stalks—in fact, that what ignorant spectators look upon as dirt in the vat, is necessary to produce the most valued virtue in the wine. Perhaps the reason why Anglo-Saxons are not generally successful as *vignerons* is that they are too cleanly.

5. What is technically termed "body," consists in a great measure of sugar, which we have already discussed ; but besides this, there is also gum, extractive, and a certain quantity of albuminous matter which has survived fermentation. These separately have not much taste, but still both individually, and more when united, have considerable power in modifying the over-marked flavors of some of the other ingredients. Their influence in giving taste is greater as regards the future than the present of the wine. Their medical importance will be spoken of afterwards.

We next come to consider the CHANGES BY AGE which wine undergoes. In such a complex organic mixture chemical rest is impossible, and there is probably no single moment at which wine is not altered for the better or for the worse from the time of its birth in the vat. Like all organic mixtures, it is born to die and decay, but as a first operation of that which afterwards constitutes its death, it may acquire properties which render it more agreeable to the palate, and more beneficial to the health of the consumer. This result is aimed at by judicious *cellaring*.

The first alteration for the better consists in the *loss of water*. This

happens while the wine is in the wood, by evaporation through the cask. Doubtless some alcohol escapes at the same time, but in proportion more water, as is shown by the experiments of Sommering, repeated by Dr. Graham, in which dilute spirit put into bladders became stronger by keeping. Good cellarers are careful to repair this loss by filling up the casks with wine, so that thereby all the constituents of wine, except water, are increased, and the article becomes both stronger and higher-flavored; for the component parts, being more concentrated, act chemically on one another with more energy, and develope the expected flavors more fully. If, however, the deficient space in cask is not refilled, air gets in, much of the alcohol becomes oxidized into acetic acid, and though a fine ether may be thus generated, yet an unnecessary amount of sourness is produced. Thus, for instance, the old plan of sending Madeira backwards and forwards to the East or West Indies rapidly evaporated it, and produced a finely-flavored, but very sour and unwholesome wine; whereas a few more years in a warm cellar and continuous refilling would have elicited the same taste without the acid.

Up to the time then, of being drawn from the cask, wine gets continuously stronger; but once bottled, no increase in alcohol takes place. True, the alcoholic contents of old wine are large; but the fact is, they have been kept long because they were strong: they are not strong because they have been kept long. Sugar could not turn into spirit without an evolution of carbonic acid, which is never found in old wine. Almost any wine may be improved by keeping in the cask, if care be taken to refill it; but only the very best will bear the neglect of this precaution, and even they will not improve so much as they might do under more prudent management. A more important change than concentration, is the gradual *development of the ethers* before described.

Diseases of Wines.—The most destructive of these, the *rot*, is announced by loss of red color, loss of taste and order. At this stage of decay the spirit is intact, and the wine may be distilled into very good brandy. But soon the alcohol begins to be oxidized into acetic acid, and the more destructive disease called *souring* commences. This goes on gradually from above downwards, till the whole is converted into vinegar, and the longer decisive steps are delayed, the less spirit can be saved. The rot and souring are most ruinous occurrences to the wine-merchant, and we cannot therefore be surprised that a variety of plans are extant for arresting them. The most obvious agents to employ for this purpose are those which have the property of stopping decomposition in other cases. Sulphurous and sulphuric acids have been used, as also alum, etc.

The *grease* of wine is the conversion by a kind of fermentation of the sugar into a ropy oily mass consisting of lactic acid, mannite, and vegetable mucus. It has been called "mucous fermentation," and may be produced in the laboratory by keeping sugar and yeast at a temperature of from 80° to 100°. It probably, therefore, is due to some tricks having been played upon the wine by heating it to induce an artificial ripeness, or by too rapid fermentation. The presence of

tannic acid prevents it, and M. François, of Nantes, advises the use of that substance, or of bruised crab-apples, as a cure.

Fungous vegetables may grow in wine. A great incentive to this is excess of tartaric acid. Solutions of tartaric acid in the laboratory are constantly getting mouldy, and the same result not unfrequently takes place in light wine too long casked. When bottled, there is an additional element of decay in the cork, which in an ill-ventilated cellar may often be seen covered outside with a crop of mould. This spreads inwards, and in course of time infects the contents, which are then said to be "*corked.*" There is no more common disease in cellars than this, and excellent wines are often spoiled by it. The remedies are: 1st. If possible, ventilate and dry your cellar. 2d. Recork all the bottles not intended for immediate use. 3d. Cut your corks close off and resin their tops. 4th. As the disease is purely a local one, it often has not extended beyond the surface of the wine in immediate contact with the cork, and hence the rest of the bottle may be saved by pouring off a few spoonfuls.

Bitterness is a disease almost peculiar to Burgundy. It seems to arise during a second fermentation, as carbonic acid is evolved; and the peculiar taste is ascribed to citric ether, which is bitter.

Wines may be used to last any length of time by increasing their alcoholic contents. But there is no advantage in this; all that is wanted is for them to last uninjured till their full flavor is elicited; after which keeping them may make them curious indeed, but will not add to their value. The less alcohol they have in them the better, consistently with their lasting long enough to become perfectly ripe.

All that can be said about the VALUE of wine known by external evidence to be pure, is a deduction from what has gone before relative to the changes by age. The value certainly is not to be reckoned by its alcoholic contents, for no amount of spirit or length of keeping will bring virtues out of wine not capable of exhibiting them. Still less is it to be estimated by mere antiquity; wine, like women, is to be valued for its maturity, not for its age. The most notable *reductio ad absurdum* of the plan of adding an annual price to wine, is the quaint boast of the burghers of Bremen, who, by reckoning at compound interest, and including duties, expenses of cellar, etc., run up the ideal cost of their unique Rosenwein to £54,000 a bottle. It never gets to any heads but those crowned with the laurel or the diadem; the birthday of a Goethe, or such an occasion as the recent Wurtemberg congress of Emperors, alone open the Rosen-keller. But let us profane outsiders flatter ourselves that we do not lose much by the exclusion. Depend upon it, the wine is past its maturity, and its computed price ought to have been *decreasing*, instead of increasing at compound interest for the last hundred years.

The qualities which in a young wine indicate the likelihood of its improving by age, may be inferred from what has gone before—they are, spirit, sweetness, body, and roughness—indicating the presence of alcohol, to preserve it; of sugar, to make it taste; of albumen, extractive, and fatty acids, to act upon its alcohol and develope bouquet; and of tannin, to keep them from decaying too quick.

A rough guess may be made of a wine's latent powers by ripening it prematurely with heat. If it becomes more scented by that process, it will become more scented by age. We cannot, however, encourage our readers in these speculations, unless they have a very well-built cellar, and pay great attention to it. The safest way is to go to a trustworthy merchant, buy wine nearly ripe, and pay a fair price for it. But they will find a good deal of advantage in having some previous knowledge of the subject, in applying their theoretical acquaintance with chemistry to it, and showing their wine merchant that they have done so.

The MEDICAL USES of wines. This is a subject thickly clouded with all sorts of prejudices and prepossessions, as is the discussion of most substances used equally by the sick and the healthy. Persons argue that what is good for themselves, must be good for their patients. We have known a plethoric dietician, who himself loved lobster-salad and champagne in the small hours, advise a starveling dyspeptic to follow his custom of taking no breakfast till noon. So a hearty rough-stomached doctor will declare one diluted alcohol just as good as another; the ascetic, or the reformed rake, will pronounce all equally bad; the gouty will dread all that is thin and acid; the aguish will have a predilection for Port.

It is very possible that prime wines may be made of all kinds, which may be equally and perfectly wholesome; but their rarity will always put them out of the reach of our patients, and what we have practically to think of in naming a wine for use, is at best a second or third-rate article. We must also choose those which are capable of being grown in quantity proportioned to their popularity, or the chances of adulteration are exaggerated. When Madeira was on everybody's table, it could not be recommended to patients, for in nine cases out of ten it was either an inferior sort or a sour imitation. But now that it has gone out of fashion, a wholesome and often perfect wine is to be bought of that kind, and the adulterators expend their ingenuity upon Sherry. What we want is a liquor which is either produced in very large quantities, or is not sufficiently known to the million to be worth imitating.

The medical questions concerning the employment of wine will be put in the clearest light for exhibiting our real knowledge and ignorance, by considering separately the physiological effects on the human frame.

Exhilaration is not merely a minor degree of drunkenness. It may be produced by many things beside alcohol, and which do not inebriate—such as, for example, the essential oils, peppermint, onions, valerian, assofœtida, tea, coffee. Even eating, and the increased circulation of blood, produce the effect to some extent. Alcoholic fluids truly do exhilarate with the greatest certainty and rapidity, but not in direct proportion to the alcohol they contain. A glass of wine will raise the spirits of a healthy person as much as a glass of gin, a glass of fine claret as much as one of strong tavern port; and this is not merely from the pleasure of taste or association, for the same may be observed in fever patients, whose gustatory nerves are blunted by a thick coating of sordes.

The distinction is not only a subjective one, evident to the mind of the recipient, while it is incapable of demonstration to others. There is a real physiological difference in the effects which follow exhilarating and intoxicating doses—a difference which, in its ultimate results, amounts to a complete contrast. The former *in*crease the amount of vital powers rendered available in a given period, and the latter *de*crease them. Can there be a more perfect antithesis?

This is too important a matter to rest solely on the unassisted senses of patient or observer, and it does not do so, for the admirable experiments of Dr. Böcker have submitted it to the proof of chemical analysis. Though the whole series of his investigations into the action of alcoholic stimulants bear directly on the present subject, they are too mutually dependent on one another, and too lengthy for quotation. The general results, however, may be stated as follows:

1. The special action of alcoholic drinks is to arrest destructive assimilation—to stop the over-active processes of life in their effects upon the organism; so that for a certain period during the stay of the alcohol in the system, less urea, less phosphates, less water are excreted by the kidneys, less carbonic acid by the lungs, and less digestion goes on in the alimentary canal, showing that the muscles, bones, nerves, etc., are not getting rid of their effete tissue, but retaining it, and making use of it as far as possible.

2. But at the same time they give rise in the body to a defensive *reäction*, which is prominent, first, immediately after taking the dose, then gives place to the special action, and on this ceasing is again manifested to a greater extent.

3. So that if a suitable quantity be taken, and if both action and reäction are allowed to exhaust themselves before the dose be repeated, more manifestation of life, represented by more excretion and more consequent renewal of the body, takes place in a given time with the alcoholic drink than without. There has been a positive gain in vitality.

4. But, if such a large quantity is taken at once that the reäction is overpowered, or if it is arrested by a continuous repetition of the dose, the manifestation of life is kept down; the body is not renewed, because its effete particles are not removed, and the amount of vitality must certainly be reckoned at a loss.

Nutrition is an indirect effect of wine. There is shown by chemical investigation to be very little substance in it capable of building up the body. The phosphates and albumen are more readily found elsewhere, as Franklin has imprinted on our memories by his comparison of a penny roll and a gallon of beer. But alcohol seems to render the alimentary canal more ready to absorb nutriment. Farmers find this, and always try to put some waste beer or fermenting grains in their pig troughs. Physicians find it, too, and give their patients cod-liver oil in a glass of sherry when they would have it fatten quickly. The effect, however, is probably confined to oleaginous food and the adipose tissue, for the digestion of albuminous matter by the gastric juice is certainly impeded by alcohol.

Hence we gain the following rules concerning the administration

of wine as an aid to nutrition:—1st. That the alcoholic contents are those of principal importance, and that the amount of solid or nutritive matter in the wine makes little difference. 2ndly. That we may hope help from it in increasing adipose tissue, but not muscle. 3rdly. That as its agreement with fatty food is the prime object, we must avoid those wines which are likely to make such food unassimilable, as, for example, by making it rancid; and, therefore, 4thly, That sound wines with a small proportion of acid to their alcohol, and but little body to cause refermentation, should be selected; the types of perfection may be considered the dry Spanish wines, Amontillado and Manzanilla. And 5thly, They should be taken along with the fatty food itself, or immediately after it.

The *arrest of destructive metamorphosis*, or what has been picturesquely called "the moulting of the tissues," is unquestionably the most important of the medical uses of alcoholic liquids. By them we are enabled to stay the progress of interstitial death in low fevers, till the period of the zymotic poison's virulence is passed, and it has either been evacuated or become inert. By them we can check the exhaustion of the body through excessive secretion, as in cases of chronic catarrh, ulcers, abscesses, amputations, etc. By them we can diminish, in ordinary dietetics, the wearing out of the body by the overworked mind, which in this busy metropolis throws so many into the hands of the physician. But in the wielding of this two-edged sword the greatest judgment is requisite, lest we carry the effect too far. The destruction of effete tissues is part of life, and necessarily precedes constructive renewal; if, then, we check it too far, interstitial life is diminished, and the system is overloaded with matter incapable of vitality.

It is better therefore to give alcohol in a diluted form, even when we wish to produce its most decided action, as in typhus fever, for example. And it is better to give it combined, as it is in wine, with other substances of partially corresponding action, than to administer it merely diffused in water, as is sometimes done for economy's sake. Sugar, we know from Dr. Böcker's experiments, has a special effect in limiting the destruction of tissues containing phosphates, tissues of no less importance than the bones and nerves. And it is likely that similar investigations into the physiology of ethers may show some special effects belonging to them. The acids, too, and the extractive in wines, seem to prevent better than water those injurious effects upon the mucous membranes which spirituous liquors exhibit. There is, then, no extravagance in preferring wine to brandy and water in the management of low fevers in hospital and parish practice.

This is not the place to discuss details in the mode and period of administering wine in acute complaints. But one reminder may be deduced fromthe view taken of its physiological action—viz: to allow intervals to elapse, during which its effects may subside, and the system recover for a time its metamorphoses, so that the effete tissues may have a due exit. The night is a convenient time for this in general.

Inebriation is a terrible word to meet with in periodical literature. It opens up a prospect of so many social and political questions, that the reader is apt to close the page in despair. He shall be let off here with a simple remark derived from wayside observation—viz. that in all countries where wine is plentiful and cheap, drunkenness is almost unknown; where it is most expensive, that vice is at its maximum.

Degeneration of tissue, as a consequence of drinking, appears to be a chronic state of that arrest of metamorphosis which has been already discussed as a remedy for disease. The effete tissue remains as an useless burden mixed up with the healthy, and is finally converted into the least vitalized of all the organic constituents of the body, oil or fat. Careful and valuable observations have been made by Dr. Böcker, on the abnormally retained blood-discs in the circulating fluids of habitual spirit-drinkers, and the appearance of the degenerated hearts, livers, and kidneys of these miserable suicides is familiar to us all.

Degeneration arises from the arrest of metamorphosis being too long and continuously kept up. Hence there is little danger of it in acute cases, where the large quantity of alcoholic remedies we find it expedient to administer is necessarily diminished as the disease recedes, and during convalescence is reduced to the ordinary allowance of health. But in chronic cases it is often a matter for serious consideration whether we shall employ an agent capable of doing, along with the good we intend, an evil greater than that originally to be combatted. If the dose of a stimulant be repeated before the arrest of metamorphosis has ceased and the reäction of the system has begun, a second arrest indeed takes place as before; but the postponed reäction is augmented in force each time it is delayed, and when it occurs at last, it is so painfully depressing that it becomes more and more difficult to resist the instinct to put it off, and in the end it is really dangerous to do so suddenly. This is the short history of confirmed tippling; and often we fear it may be traced in its origin to the carelessly worded advice of some medical man. Science or practice has taught him that alcoholic action will alleviate certain morbid phenomena, and he recommends it without due warning. The patient knows no harm in alcohol except drunkenness, and so long as he avoids that vice, thinks he cannot keep up too steadily the agreeable relief he experiences. Alas! much safer for him would be the occasional debauch of a man he despises as a profligate, than his own continuous steady course towards death. A drunken bout brings its own cure, and is usually allowed to be followed by reäction afterwards; but the most alarming symptom in a tippler is that he *cannot* get drunk. Day by day there is a litttle less and a little less life in his system, till at last his degenerated body is fit for burial.

Now, the results above described are, practically speaking, unknown as the consequence of wine; it is spirit drinking that leads to them. There are several reasons for this, independent of the chemical differences of the liquors. Wine is rarely used except at the principal meal, or as a sort of medicine in measured quantity at other hours,

so that the effects have time to pass away before another dose becomes due, and no craving for increased quantity is experienced. In fact, men go on taking daily for quarters of their life the same identical number of glasses, feeling daily the same comfort, and never finding it necessary to increase the quantity. But the spirit bottle is opened when its owner "feels to want it"—nay, it is very often carried about the person under the appropriate name, as regards its deadly results, of a "pocket pistol."

Among the *Derangements of Digestion* arising from wine, it will not be necessary to dwell long upon the immediate consequences of a debauch. It is usual, in army medical returns, to report it as "febris," as indeed there is, truly enough, an ephemeral fever, but, like other fevers, it works its own cure, and civilians are not in the habit of applying to it the same euphemistic nomenclature. But, without being taken in such quantity as to be considered an excess as regards alcohol, wines will sometimes cause a disturbance of digestion, which prevents our sanctioning their use in cases where otherwise we might be willing or anxious to do so. This is always accompanied by the presence of a large quantity of acid in the alimentary canal.

In some instances this excessive production of acid follows equally all sorts of wines, and even spirits. Then it is due to the mucous membrane of the stomach being so morbidly sensitive that it becomes irritable and temporarily inflamed, so that it refuses to secrete its solvent juice, and to perform with sufficient activity the peristaltic movements. Hence the alimentary mass undergoes the acetous and lactic fermentations, instead of being digested. These patients ought to abstain from all alcoholic drinks whatsoever till cured of their morbid condition.

But it makes some difference what sort of acid is contained in the wine. Acetic is to many stomachs much less injurious than tartaric, and it is found that the proportion of these to one another varies very much in the products of fermentation. Thus, in Madeira, nearly one-third of the acid contained is acetic; in port, only one-fourth; in claret, one-fifth; in champagne, one-seventh; and in hock, not one-eighth, whilst the rest is the last digestible, tartaric, or its ally, racemic. Besides these, the tannic must be allowed for, small indeed in quantity, but powerful in operation, as its use in medicine shows. Sugar in wine which is to be taken by itself as a medicine, is often beneficial by making the acid and alcohol less immediately irritating to the mucous membrane; but in that which is to be mixed with food it is very apt to increase the generation of acid in the stomach or cæcum to an injurious extent, generally two or three hours after meals.

Hitherto the genuine and perfect juice of the grape has been spoken of; but it is certain that much of the difficulty which patients experience in digesting wine arises from their use of an impure article. There may be found often a much larger quantity of acid than the taste indicates, concealed by molasses, or perhaps by sugar of lead. Instead of tartaric, there may be malic acid, the consequences of which to the digestion are well known to the eaters of rhubarb tarts

and the drinkers of hard cider. There may be alum, copper, iron, nickel, nay, even arsenic, accidentally added to inferior wine, and nobody knows what else, for purposes of fraud.

The analytic examination concerning the *adulteration of wines*, with which this article closes, is too long for insertion in this Journal, but discloses horrors which are scarcely paralleled even on the banks of the Mississippi!

The writer, in alluding to the many recipes in the "Wine and Spirit Merchants' Own Book," says:

Here we find chalk, marble, etc., used to counteract, not the tartaric acid of youth, but the "sourness" or acetic acid of premature decay. A very different thing; for the tartrate of the earth being insoluble is left in the cask, while the acetate goes down into the stomach. Hence Falstaff was able to detect the "lime in the sack," which he so justly denounces as rascally. Equally bad is the addition of molasses or potato-sugar to cover the acid; indeed, the consequences to the digestion are usually worse, because the acid is left in its native strength. But of what punishment is he worthy who could devise the following infernal recipe?—"To keep wine from turning sour: put in the cask two pounds three ounces of small shot"!!! Ah, Professor Mulder, when your innocence said that lead was no longer used for the adulteration of wine, you did not know the wickedness of an Anglo-Saxon in 1855. But you are not shocked half enough yet. What do you think of this? "In extreme cases, when all the previous receipts have been tried without any satisfactory result"—that is to say, when the wine already contains lead, alum, brandy, chalk, charcoal, chloride of lime, barley, honey, etc., etc.—"take a small pinch of OXALIC acid, and put it in the bottle"!!! Nay, reader, do not be sceptical, there are the words in page 32, in all their normal horror. It is unnecessary to say that the detection of these gross adulterations is as easy as possible.

REVIEWS.

REV. I.—*The New American Cyclopædia: a popular Dictionary of General Knowledge.* Edited by GEORGE RIPLEY and CHARLES A. DANA. Vol. I. A. ARAGUAY. Pp. 752; Contents x. Vol. II. ARAKTSHEEF—BEALE. Pp. 776. Contents viii. Double columns. Royal 8vo. New York: D. Appleton and Company, 346 and 348 Broadway. London: 16 Little Britain. MDCCCLVIII.

THE able editors, Messrs. RIPLEY and DANA, assisted by numerous and competent collaborators, have with great celerity completed two volumes of this valuable Cyclopædia. In his letter* dated May 31, 1858, Mr. Ripley writes that the third volume is nearly ready. The Preface in the first volume is dated in last December. The work which is published exclusively for subscribers in fifteen volumes, will, doubtlessly, be passed through the press as promised, that is, as rapidly as is consistent with mechanical accuracy. This is an undertaking, extensive in its plan and difficult in its execution, in which "many men of many minds" may think profoundly and work hard and successfully without either exhausting their varied subjects or comprehending the All of possible knowledge.

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* OFFICE OF THE NEW AMERICAN CYCLOPÆDIA,
New York, May 31, 1858.

Bennet Dowler, M. D.

Dear Sir,—I beg you to accept my thanks for your acceptable favor of the 17th instant, with a copy accompanying it of the New Orleans Medical and Surgical Journal. I have read your review of Dr. Dunglison with great interest, and find in it many suggestions which will be of service both to my collaborators and myself. You will, doubtless, find in our volumes much to criticise (as imperfection is stamped on the very nature of such a work), but I trust you will also recognize the marks of industry, patience, careful research, editorial vigilance and good faith. I need not say that we should be pleased to profit by any suggestions or comments that may occur to you, and with which you will kindly furnish us.

Your remarks on the term Redhibition are valuable, as it is a title of such local application, that it would be very likely to escape us in making up our vocabulary. I should be happy to receive a brief article on the subject from your pen, and should you think of any other title, of peculiar interest or importance in your quarter of the Union, I should be greatly indebted to you for pointing it out to our attention, and still more for supplying us with materials for its just and intelligent treatment.

I must ask you to find an apology for this long note from a stranger, in the friendly tone which I recognize in your own, and to believe me, with high respect,

Your obedient servant,

GEO. RIPLEY,
Ed. N. A. Cyclopædia.

Bennet Dowler, M. D., New Orleans, La.

Incompleteness more or less inheres in every work devoted to the exposition and analysis of universal science. This work does not aim at originality either in facts or principles on the part of its editors, but draws from every accessible source whatsoever may be available in the systems, discoveries, experiences, and facts of others.

Perhaps it may be allowable to say that even facts grow old, deteriorate, or at least form new combinations in reference to new psychical, social, and physical conditions of humanity. Facts, therefore, must be renewed and retested as well as extended. Science is scarcely a circle but a series of circles, the last of which never coïnciding with the first, its evolutions being completed further on, nigher the great yet unattainable centre of perfect, that is universal truth, whereunto each converges, like the stellar Universe which moves towards a centre infinitely remote, forever unreachable.

The New American Cyclopædia of this generation will be old to the next—old, not useless, the venerable, perhaps obsolete, representative of the mid-century, XIX.

This work is neither a series of treatises, nor one of detached fragments distributed to the utmost possible extent of alphabetical subdivision. In cyclopædias elaborate and exhaustive treatises for special studies and professions have seldom been attempted, never with success. The more a work of this class aims at this end, the less liberty is allowable in distributing a subject into alphabetical minutiæ. It is, however, desirable that brief treatises or monographs should, as much as possible, be concentrated under one head and not subdivided. Detached articles floating on the common stream of literature and science may be to some extent systematically arranged, by means of references, even upon the simple alphabetical principle, being neither the exclusive property of any special science, nor so closely connected with any as to suffer by segration. At the same time, there can be no doubt, among the two thousand articles in these two volumes, that in many instances, *references* to each other as already written, or such as will be written, on many subjects of a parallel, allied or identical character, would aid the student in his inquiries, although there are not in these two volumes probably ten examples of this kind. References, like the cornice which connects the intercolumniations of an immense building, give symmetry, being the more important, because the nomenclature of the sciences is so varied and re-

dundant, that one in looking for information under a technical term, has a nearly equal chance to miss or to find the one, or the several selected by the writer. Thus "*acute diseases*" should refer to chronic; "*albuminuria*" to dropsy; "*affinity*" to "attraction;" "*agriculture*" to "*agricultural chemistry;*" "*alcohol*" to spirits, wines, etc.; "*ague*" to intermittents; *aliment*" to "*adulteration;*" "*age*" to longevity, and "*alum,*" "*alumina,*" "*aluminum,*" to each other, etc. This complemental method is desirable among paralel, similar, or identical subjects, whether theexplanation be under a single title, or partly under several. An exuberant nomenclature, a copious synonymy, together with the fortuitous segrations of alphabetical distribution, resemble the games of chance, at least for beginners in science. The golden thread of filiation, confraternity, and unity, which cannot be preserved unbroken in mere glossology and terminology, should run throughout a cyclopædia from A to Z.

References can yet be given in the remaining volumes to those already published, in almost all important respects, and some corrections may thus be made. For example, the remaining title, INTESTINE, will serve to correct some errors in that of "ALIMENTARY CANAL," wherein it is said that "the small intestines open immediately into the cæcum or *lower portion of the large intestine,* and a third *straight* duct called the rectum, some ten or twelve inches long," etc. The cæcum is not the *lower* portion of the large intestine but its *commencement*, nor is the rectum straight. Again: The important term, BATTURE, is omitted, but this can be noticed under the title Louisiana, or Mississippi river. Batture is the alluvial deposits and accretions upon the shores of the river. The principles of riparian proprietorship to the battures are set forth in various articles of the Civil Code. Riparian rights and laws which are of fundamental importance to Louisianians have given rise to much litigation. The City of New Orleans was, a few years since, a party to a law-suit in which batture property of great value was involved. Once more. *Arundo gigantea*, the colossal wild cane which forms the dense cane brakes of the South, a striking feature, can be introduced under C, if the system of intercurrent reference be adopted.

It may be deemed impertinent, and, perhaps, in a pecuniary point of view utopian, to suggest the necessity of illustrative engravings, of which this work is so far wholly destitute, with the exception of a

few figures under the title *Anthracite.* This desideratum can be supplied at the termination of the work, by adding one or two volumes of plates with explanations. The low price of the work might be hazardous to the publishers, if it were illustrated according to the merits of the text, unless the subscription list be very large. Whether a bibliography, with dates, editions and documentary references should not be subjoined to many important articles without interfering greatly with the brevity or symmetry of the work, is a question concerning which much may be said on both sides. The table of contents at the end of each volume is a convenient improvement. Had each column of the work been separately paged, the references from the contents, would have been facilitated.

In nothing are critics, particularly the friendly ones, more liberal than in the bestowment of their opinion and advice. Swift, the witty Dean, declares that "when friends begin to advise us, they begin to despise us." The sailor who admitted that he and his comrades prayed during a storm, maintained, nevertheless, that their prayers were like their curses; the former meant no good, the latter no harm. Is it not the same too often, in praising and in censuring books? To which of these categories the above-mentioned and the following critical remarks belong, judge ye who read, after having first read the NEW AMERICAN CYCLOPÆDIA. To say that it should be placed in every physician's library, is to use the stereotyped phrase which is applied to almost every medical book. This, if not a medical work is something better than many claiming to be such, abounding as it does in topics directly medical. Letter A, to go no farther, has, among others, the following well written articles:

Abscess, Autoplasty, Atomic theory, Absorbtion, Abstinence, Acclimation, Aconite, Acute diseases, Adipose, Affinity, Albuminuria, Alcohol, Adulteration, Allopathy, Aloes, Ammonia, Alum, Amphibia, Amputation, Anæsthetics, Age, Agricultural chemistry, Anatomy, Aneurism, Angina, Animal, Antagonistic muscles, Animalcules, Anthelmintics, Antimony, Anti-scorbutics, Attraction, Anthropology, Arrowroot, Arsenic, Artery, Articulation, Atony, Atrophy, Asphyxia, Asthma, Auscultation, etc., etc.

The following experiment made to test the topographical accuracy of the Cyclopædia in relation to the geography of Louisiana, created surprise: Every Parish, River, Lake, etc., under A was written down

so far as memory could recall; the Cyclopædia was then consulted, and the list was progressively blotted out when the name was found in the work, whereupon only one title of the whole list remained, namely, Allemands, a lake little known even in New Orleans! Louisiana is a *terra incognita* to most of the inhabitants of its capital. Foreign geography is better known.

This work, in professing to steer clear of political, theological, and philosophical controversies, will, of course, have the advantages incidental to a general pacification: "The discussion of the controverted points of science, philosophy, religion, or politics, does not enter within the compass of its plan." (*Preface.*) With the exception of the two latter, little is to be feared from the antitheses, antinomies, and controversies of science. The excitements, conflicts and shocks originating in the understanding, not in the passions, are beneficial. The collisions of mighty intellects produce corruscations by which truth is elicited. Indeed, the war in the realms of intellect is, and must be, eternal, and withal useful beyond all other war. It tends to advance speculative and practical knowledge, to elaborate "the high argument" of the past, the present and the future. It opposes the *inertia* of mere authority, removes the dogmas of tradition, and ushers in the glory of discovery. It battles with error. Its reasoned faith opposes itself as a barrier to unlimited skepticism, on the one hand, to the despotism of dogmas on the other. Its Copernicus opposes Ptolemy—its geology the dictum of an assumed chronology—its Harvey and Jenner the prejudices of the moment, and brings forth its discoveries too tedious to mention, and almost miraculous in character and utility. It is never stationary, never without controversy. Transcendentalists and experimentalists, and even mathematicians, astronomers, and chemists, and still more physiologists, have controversies to settle with their glittering broad swords of logic. These are holy, bloodless and honorable wars, by which the territories of truth are explored and finally conquered. The evil passions which mingle occasionally in these conflicts are ephemeral compared with the bigotted sectarianism, partisan malevolence, personal animosity, bloody persecution, and hereditary hatred which sway the religious and political worlds, and blacken the pages of history through all time and in all places.

General science whose scope is unrestricted by mere text books, is

of high importance to the physician; it not only develops his intellectual powers, but enlarges the horizon of his knowledge as it regards his special vocation; but it affords calm, rational and enduring pleasures in a profession useful and honorable, indeed, but at the same time an uncertain road to either fame or fortune.

There is an affinity or unity in all the sciences, an everlasting alliance. The profit and recreation in travelling through the kingdoms of Nature, are greater and nobler than can be derived from travelling through political kingdoms.

A Cyclopædia like that mentioned above, rich in scholarship, fresh in researches, judicious in its selections, intelligent in its treatment of subjects, of high, practical, useful and catholic aims, and excellent in execution, is a book which must be peculiarly acceptable to medical men, and the more so, because it treats directly, extensively and ably upon many medical topics.

As already mentioned, a medical man, above all others should travel intellectually—travel beyond the shop. A change of air, new scenery, other lands of knowledge, as revealed in an encyclopædia, afford intellectual health, recreation and opulence.

In the New American Cyclopædia, so far as completed, many articles on the Natural Sciences, Physiology, Hygiene, Medicine, Geology, Geography, Topography, Biography, History, Antiquities, Art, etc., have been intelligently treated without having adopted either a severe technical or a loose popular style.

A fresh work of this kind—a work representing the existing state of general knowlege, neither meagre nor prolix, has been for some years needed by the American student. Without having had leisure as yet to give a critical review of this work, a cursory examination leads the writer of this notice to the same conclusion which has been generally expressed by the medical, theological and lay press, namely, that this Cyclopædia is an impartial, trustworthy and excellent work peculiarly adapted to the times, places, wants and populations for which it is intended.

B. Dowler

REV. II.—*Proceedings of the Academy of Natural Sciences of Philadelphia.* January—March. 1858 Pp. 88. 8vo.

THE Academy of Natural Sciences of Philadelphia, than which a more luminous example of the successful and disinterested cultivation of Science it would be difficult to name, has, for nearly half a century, without governmental patronage, investigated, received, and reflected the lights of objective philosophy, concerning the differentiæ, resemblances, analogies, and characteristics of the flora and fauna, both fossil and living, ethnology, geology, chemistry, etc., of this continent. Its numerous publications, in its *Journals* and *Proceedings*, the most learned in the Republic, are, however, devoted to the purely objective sciences, while the science of life, the laws of physiology, the dynamics of the animal organism and the fundamental principles of subjective science, have been neglected or referred to only incidentally.

The scholar Porron, who even thought in the dead languages, would probably, were he alive, soon become weary over a volume of the Academy's singularly technical descriptions of mammals, birds, reptiles, shells, fishes, insects, plants, minerals, fossils, geological formations, etc. Who but an inveterate entomologist would write, or read and study in Latin, descriptions of all the different known species of insects, one order of which, the beetles, (coleoptera) number thirty or forty thousand recognized species? Although the works of the Academy from their nature are not adapted to the popular taste beyond the narrow circle of rigid naturalists, yet, an enlightened public has not failed to regard them favorably. Many private individuals including some ladies, have contributed munificently in specimens, books and money towards the advancement of the great ends contemplated, and steadily pursued by this institution; while, on the other hand an exemption from the payment of taxes, on the buildings and collections of the Academy, is the only aid received from legislative patronage.

The severe technical descriptions and classifications of the objects presented in the organic and inorganic realms of Nature, adopted by the Academy, are, perhaps, necessary to scientic accuracy, identity, and progress.

To know Nature as she is known by superior minds is no easy task; to know her absolutely as she is in her entirety, variety, and parts, transcends the limits of the most gigantic intellects. "We

know but in part." Nevertheless, history shows that many once apparently inaccessible heights and impenetrable depths of Nature have been successfully explored, revealed, utilized.

When an ardent investigator of a scientific speciality turns to the researches of his predecessors and cotemporaries for full and reliable materials for scientific generalization and information, he will seldom find cause to complain of an exuberance of description, or of excessive minutiæ. He will probably, on the contrary, complain of existing hiatuses and deficiencies of criteria and data illustrative of his favorite study.

The externalities of Nature to which the Academy has hitherto chiefly directed its researches, may, and are about to be conjoined to and animated by the biological, hygienic, histological, and pathological sciences. Recently the new Biological Society of Philadelphia has been merged into the Academy, and its members, who are members of the Academy, have been organized as a subdivision or department, as have several other and different departments of a permanent character, each having separate directors, recorders, treasurers, conservators, rights, etc. The self-government of the departments, and the general government of the whole by the Academy, constitute a complex system the *denouement* of which remains to be seen—a system that would puzzle politicians and statesmen who are accustomed to reconcile or define the boundaries of Federal and State rights and the supremacy of each in this Republic. But scientific men who seek neither power nor emolument, will have few causes for hostility or disagreement.

The following letter explains the future plans of operation in the Biological Department:

"Hall of the Academy of Natural Sciences,
Philadelphia, April, 1858.

"*Dear Sir,*—The Academy of Natural Sciences of Philadelphia have recently instituted a "Biological Department," with special reference to the original investigation of subjects of the highest interest to medical science.

"The papers hitherto published in the Proceedings, though of the highest importance to natural science, strictly so called, have seldom been of such a character as to exercise a bearing upon practical medicine. In future, however, such will not be the case; and by the labors of the members of the Biological Department, it is confidently anticipated that much matter valuable to the physician will be given through this medium.

"The subjects to be specially considered by the Biological Department will relate to Anatomy and Histology, Physiology, Pathology, Organic Chemistry, Mierology, Etiology and Hygiene, and Embryology and Teratology. On these branches rests the superstructure of medical science, and in the same ratio with which they are cultivated may we expect progress in practical medicine.

"I am, sir, very respectfully,

Your, obedient servant,

"H. HARTSHORNE, M. D., *Recorder*."

Scientific not less than industrial pursuits advance towards perfection with increasing celerity, by means of division of labor. This division which the Academy recognises in fact, is neither arbitrary nor accidental, but is founded upon differences and peculiarities of mental organization, genius, capacity, energy, taste. Astronomical does not imply surgical talent, nor mineralogical obstetrical, nor therapeutical, entomological, botanical or geological ability. Homer, Harvey, Hunter, Humboldt; Galen, Galileo, Goëthe, Jenner; Linnæus, Lavosier, Shakspeare, Laplace; Columbus, Kant, Cuvier; Newton, Washington, Franklin; Bacon, Burns, Bichât, all and each obeyed "a God-given best."

The moral effect of the study of the Natural Sciences which abound in lessons of wise and beneficent designs and plans, elevates the mind above the sensual passions and produces faith in the existence and government of the All-knowing, the All-wise, the All-good. "This high argument" in favor of physical and physiological investigation, coïncides with the physical and sanitary well-being of society. Almost all scientific discoveries have been, or will doubtless be utilized and converted into capital. Discover first. The application will certainly come soon or late.

B. DOWLER.

REV. III.—*The Annual Report of the Managers of the State Lunatic Asylum of New York.* (Senate Document. 1858.)

THIS document, which abounds in the usual statistical tables that few ever read, possesses merits of a higher order not always met with in similar publications, namely, practical observations concerning the

hygiene of asylums—observations which, for the most part, apply equally to every dwelling, in regard to water, warming, ventilation and sanitary economy. Speculative writers on hygiene, and on the ætiology of disease, who deduce their theories from assumptions concerning wholly unknown agents, or from the ordinary or unusual meteorological conditions which happen to coïncide with the outbreaking of epidemics and endemics, so far from having contributed to the advancement of these sciences, too often obstruct their progress and interfere with the dictates of common sense by inaugurating hypotheses which obtain credit and influence the more readily because it is sometimes equally difficult to prove or disprove them.

Without enlarging on this topic, it is intended to offer some extracts from the above document as worthy of the consideration of sanitarians, though they may possess more truth than novelty.

In alluding to the recent desolating conflagration in the State Lunatic Asylum, the Managers of the Institution make the following remarks which have a momentous significance in regard to all public buildings, churches, lecture rooms, theatres, court houses, jails, etc., which should be made as far as possible incombustible:

"The calamitous event of which we have thus given an account, illustrates the importance of greater care than is usual in our country, in the construction of public buildings, and especially of those which are designed for the occupation of large numbers of persons, who are rendered helpless by mental or bodily disease. Such buildings in Europe are now generally made fire-proof. In erecting them here, it ought at least to be required that all interior walls should be of brick, and of very substantial character, and wherever it is possible, that they should extend to the roof; that stairs should be of iron leading through brick stairways, and that all connections between different parts of the buildings should be so arranged and guarded as to prevent the spread of fire from one part to another. It is hoped that the time is not far distant when public opinion will justify the expense of making such structures entirely fire-proof, not only as a matter of safety but as demanded by a wise economy.

"An occasion is also furnished for repeating what has often been said in reports from this institution, and by superintendents of asylums throughout the United States, that no such buildings should be erected without a certainty of an abundant supply of water. The water for this asylum, (except for table and culinary purposes,) is forced by a pump from one of the levels of the Chenango canal. That source is too distant to be reached by engines, and in case of fire, the principal dependance must be upon the tanks in the attics, which of course, are difficult of access for such a purpose, and to which engine hose cannot be attached. It was only after the firemen

were able to extend the hose to the Erie Canal, a distance of about half a mile, that the fire in the roof and attic of the wing was successfully combatted. A resort to so distant a source could not have been had in the night or in case of a wind, in time to have been of any avail. All such disadvantages should be avoided in selecting a site for an asylum or hospital. In addition to all proper interior arrangements for the extinguishment of fire, there should be a near source of supply, and sufficiency for throwing large quantities of water upon the roofs and the exterior openings.

"Having in view the unfortunate origin of this fire, (without, however, expressing or implying any opinion respecting the mental condition of Spiers,) we desire once more to call the attention of the Legislature to the subject of further provision for the custody and treatment of insane convicts and lunatics of the criminal class. It will be seen by reference to the report of the superintendent, that during a period of eight years, sixty-seven convicts were sent to the asylum from the State prisons, of which number thirty-nine were convicted of crimes of violence; and that seven of these were notorious housebreakers, and feigned disease to escape punishment. It also appears that during the last fifteen years eighty-seven criminals and dangerous lunatics have been sent to the institution by the order of judges and courts; and of these twenty-two had committed murder, and fourteen had been guilty of arson; (of course they were not deemed to be legally guilty of those crimes, having been saved from trial or acquitted on the ground of insanity). These facts show in a striking light the dangers to which such an institution as this is constantly exposed. When it is remembered that these convicts and criminals are sent here, *to be treated for disease, and if possible to be cured*, and that shackles and rigid confinement are never permitted, and cannot be resorted to without changing the character of the institution from an asylum to a prison, there seems to us cause for congratulation and gratitude that serious calamities by fire and otherwise have not heretofore overtaken us. During the period of eight years, from 1846 to 1854, sixty-seven convicts were transferred to the asylum. During the past fifteen years eighty-six 'criminal and dangerous lunatics' have been sent to the asylum on orders of judges and justices.

"Happily, provision has lately been made, by law, for the proper care of insane convicts, in a building now in the course of erection at the Auburn State Prison. We unite with the superintendent in the expression of a hope that 'this is a step in the direction of an entire separation of the various criminal classes from the ordinary insane.'"

There is another impending, perhaps not remote danger very common, at least in New Orleans, and very deserving of the attention of the Grand Jury, namely, the tortuous, narrow, and imperfect means of exit from churches, lecture and school-rooms, courts, theatres, etc. In case of fires or other causes of panic, a long time would elapse be-

fore a large assembly could possibly escape from these edifices, owing to the paucity and restriction of doors, corridors and stairways.

The New York State Asylum is richly supplied with Medical officers; its resident staff consists of Drs. GRAY, VANDEUSEN, CLEAVELAND and TOURTELLOT, who have treated during the current year, 696 patients (360 males 336 females) in this institution; 95 of these latter have recovered and have been discharged; 25 discharged, improved; 85 unimproved; 10 not insane; 32 have died.

The forms of mental disease which obtained among the fatal cases are thus enumerated: acute mania, sub-acute mania, chronic mania, melancholia, dementia, general paralysis. Twelve died of exhaustion from mental maladies; 89 of consumption; 2 of apoplexy; 3 of general paralysis, etc.

"It appears also, by another table, that 5,065 patients have been received since the opening of the asylum on the 16th of January, 1843; that 4,614 have been discharged, of which number 2,112 had recovered, 768 were improved, and 1,095 were unimproved; that 34 were not insane, and that 605 have died."

Of the origin of the fires in the Asylum on the 14th and 18th of July last, the Managers say:

"During the progress of the fire in the barn, the superintendent received evidence that it was set by an inmate of the asylum, named William Spiers, whom he caused to be immediately arrested. The evidence was conclusive, and Spiers confessed that he set fire to both the barn and the center building. The fire in the barn was kindled in the loft, and that in the asylum was kindled in the large foul air flue leading from the north wings, where it passed through the main attic over the ceiling of the chapel to the cupola. There was a door entrance to the flue, through which he passed, and set fire to a temporary wooden box within, which was intended to connect the ventilation of the chapel with that flue. The testimony taken by the judge who committed Spiers, does not enable us to say with certainty by what means he gained access to the attic. It is probable that in some way he obtained possession of the keys. Spiers was admitted to the asylum as a patient in January, 1850. He was sent from the city of New York by order of a court of oyer and terminer, in which he was arraigned on a charge of arson. He escaped trial upon the plea of insanity. He remained a patient until February, 1856, when he was discharged by order of a justice of the supreme court. During almost the whole of this long period he was industrious, cheerful and useful, and continually improved under moral and intellectual training and instruction, and gave no evidence of being in any respect a dangerous person. After his discharge (with the exception of the month of September, 1856, when he was absent on a visit, as is supposed, to a rel-

ative) he remained in the employment of the asylum, working the greater part of the time in a dining room, and in the printing office. He was also employed as a nurse.

He was examined before Mr. Justice Bacon, of the supreme court, on Monday the 20th of July, and was by him committed for trial. He was indicted, and the district attorney was ready to try him at the court of oyer and terminer, in October last, but the trial was postponed. In the meantime we deem it our duty to refrain from expressing any opinion upon the question of the sanity of Spiers."

Doctors, legislators, administrators of hospitals and asylums, school directors and sanitarians in the South, and elsewhere, give ear to the following suggestive hints concerning the sanitary influences and uses of the free God-given air of the atmosphere, without an abundance of which, medicinal drugs, or the costliest perfumes,

> "Sabean odors from the spicy shore
> Of Araby the blest."

can neither preserve health, nor prolong life.

"In former reports we have referred at some length to the important sanitary influence of the apparatus for warming and ventilating the institution, which has now been in operation nearly five years. By means of the fan introduced into the female department in 1853, we are enabled to insure to that division a large and continuous supply of fresh air, over the quanity and temperature of which we have perfect control. During the past year this fan has been in constant motion, night and day, and the manifest improvement in the health of the patients in that department, and the continued and uniform decrease in the rate of mortality, as compared with the male division, must be in some measure attributed to the effects of its operation.

"The steam coils for warming the department, are all in their place, the inlet and outlet flues, the distributing chambers, and foul air ducts are constructed, and all that remains to be done to secure the same advantages in this division, is the erection of another fan. This contemplated improvement, it is earnestly hoped will be carried into effect some time during the coming year.

"The experience of another year serves to confirm the opinion heretofore expressed, that the only apparatus adapted to the wants of an institution like this, is one which unites the operations of warming and ventilation, and which effects the expulsion of foul air, as rapidly as may be desired, and to the same extent and simultaneously with the admission of fresh air. The perfect, free, and equable distribution of fresh air, warm or cold, under any circumstances, and in all seasons, is absolutely essential to any large hospital, and we are convinced can be secured in no other way.

"The confident expectations of its projectors in regard to the quiet and efficient working of a fan in carrying into such a pile of buildings, through such devious channels, and over such an extent of surface,

so large a body of air, have been as fully realized as have been the advantages which were expected in reference to the health and comfort of the household. Since the construction of the fan in our female department, (the first, as far as we are aware, used for the ventilation of an hospital,) Mr. Nason has entered still more fully into the study of the subject, and in the form of some of the later ones, thinks that he has made important improvements. Though the present fan is noiseless in its action, is run at very moderate cost, and thoroughly ventilates the part of the building to which it is applied, still it would be well to ascertain whether the advantages claimed for the new form of fan, (several of which are now used in other asylums, and another nearly ready for operation in the new Hall of Representatives at Washington,) are really possessed by it.

"Experience has shown that we require thirty thousand cubic feet of air per minute in the winter, and from forty to fifty thousand per minute the remainder of the year. As we know the quantity of air delivered at each revolution, and can register, by means of an instrument designed for the purpose, the number of revolutions, we are assured of the entire accuracy of our computations. This amount may seem large for the use of only about two hundred and sixty persons, but it is found to be no more than sufficient for the complete and constant renewal of the atmosphere requisite for their health and comfort. Indeed, during the warmer days and nights of summer, we have thought it advisable to increase even this quantity. In the operation of such a machine, the expense of a few thousand additional feet of air per minute is very trifling.

"We have thus again introduced this subject, not only on account of its value to the public, but because of the too general ignorance of the value of pure air, both to the sick and the well, and of the ease with which it can be supplied to all buildings occupied by large numbers of persons. We trust that the day is not far distant when this system of ventilation will be introduced, not only into all hospitals wherein the sick are treated, but also into our public schools and seminaries, where the laws of health, above all other places, should be carefully regarded, but where they are almost universally set at defiance."

The poor and the rich often suffer for want of free ventilation, especially in cities. The poor, often from a hard necessity, overcrowd their habitations in order to economize space and diminish their rental dues, while the rich keep their houses closed by day, fearing the dust, or the embrowning effects of a straggling sunbeam, and at night from an unfounded fear of the deleterious effects of night-air in their bed chambers!

In conclusion, it may be stated, that from the New York State Lunatic Asylum, at Albany, is issued *The American Journal of Insanity*, a periodical highly esteemed both at home and abroad.

B. Dowler.

Anatomical Chair of the Medical Department of the University of Louisiana.

The chair of Anatomy in the University of Louisiana made vacant by the resignation of Dr. J. C. Nott, has been tendered to and accepted by T. G. Richardson, M. D., Professor of Anatomy in the Pennsylvania Medical College, at Philadelphia, late of the University of Louisville, Kentucky, one of the editors of *The North American Medico-Chirurgical Review*, and the author of a large and valuable work entitled, "*Elements of Human Anatomy, General, Descriptive and Practical.*" (See a notice of this work in the *N. O. Med. and Surg. Jour.*) The Regents of the University of Louisiana, in making this appointment, have, it is believed, been guided solely by the scientific interests of the Institution, now the great centre of medical education in the South. To the chair of Anatomy, which is one of fundamental importance, Dr. Richardson brings a reputation for learning, ability and experience which augur well for the advancement of an institution whose pupils, now numerous, will doubtlessly continue to increase.

Prof. Richardson won his reputation as a practitioner, teacher, editor and author, while in the Valley of the Mississippi, whence he removed to Philadelphia about two years ago; he will, in New Orleans, be very accessible to students from the West and South having a predilection for his lectures and demonstrations in a city unrivalled for anatomical facilities and hospital advantages.

Yellow Fever of 1858.

The first death from yellow fever this year occurred upon June 20th. Drs. Delery and Morrison attended this case, one in its first stage, the other to its close. Each of these gentlemen have furnished us with their notes which we take great pleasure in presenting to our readers.

Case of Yellow Fever: Reported for the *N. O. Med. and Surg. Jour.* Catharine Maxwell, aged eighteen years, a native of County Roscommon, Ireland, was admitted into the *Maison de Santé*, on Friday, June 18, 1858, with the following symptoms: Severe frontal headache, violent pain in the loins and calves of the legs; skin hot and dry; pulse frequent; tongue moist, and covered with a white fur; eyes red and watery; bowels constipated; great tenderness over the epigastrium, and morbid sensibility of the entire surface of the body.

The history of her case, as far as I could learn, is this: She came from Ireland to New Orleans last January. Had been employed in the residence of a lady residing on Barrack street, between Dauphine and Burgundy. Stated that she had not visited any persons residing near the levee or shipping in that or any other part of the city; that she had not visited any person sick, to her knowledge. She first felt unwell late on Tuesday night, 15th; next day was worse, and sent for Dr. Deléry. The Dr. visited her until Friday, the 18th, when calling, he was informed that the patient had been removed by her friends to an hospital.

The following physicians visited her at this hospital, and with one accord pronounced her case to be yellow fever: Drs. Armand Mercier and Chaillé, of the Circus Street Hospital; Drs. Foster and Bickham, of the Charity Hospital; Drs. Wederstrandt, Hunt, Jones, Faget, Deléry, Alf. Mercier, Chastant and Trudeau.

Friday night, June 18, she first vomited a dark fluid, which was pronounced black vomit. Saturday morning continued still to vomit, and also passed some of the same dark matter by stool; could not retain even a little ice water, but rejected it and every thing else given. She also had a desire, at times very great, to pass urine, but could not. An examination showed suppression of this secretion. Towards evening she dozed a little, but during the night the nausea and vomiting returned. The matter vomited was the same. Sunday, June 20th, her condition was the same; about 12, M., Sunday, she vomited a small quantity of black vomit, and at half-past six, P. M., died comatose. The vomited matter presented, under the microscope, blood globules altered in shape and size, epithelium, amorphous granular matter, and starch. It was decidedly acid.

The friends of the patient objected to a post-mortem examination; but the history of the case as detailed, with the strong tendency to hemorrhage of the mucous membranes, and the skin spotted with petechial mosquito bites, together with the presence of so many of the most characteristic symptoms of this disease, permitted no one who saw her to doubt for a moment the nature of the disease. There was but *one* opinion which an autopsy could neither have strengthened nor invalidated.

JOHN MORRISON, M. D.

Maison de Santé, June, 21, 1858.

The same Case of Yellow Fever, reported by Dr. Deléry, and translated by Dr. Chaillé; New Orleans, June 21, 1858.—Last Wednesday, the 16th instant, I was called to see a patient at my mother-in-law's, on Barrack street, between Burgundy and Dauphine streets. It was nine o'clock in the evening when I arrived. My mother-in-law told me that her white servant had been attacked with fever about four o'clock in the morning. It was feared in the house that it was a case of variola or scarlatina. The servant was lodged in a room adjoining that of the mistress of the house. I examined the patient by the light of the candle, and was struck by her physiognomy, as if I had encountered in the streets some one whom I had long lost sight of, and did not know was in town. I recognized the stamp of yellow fever. I communicated my suspicions to my mother-in-law, not wishing to make a positive assertion, surrounded as I was by negative facts only. In fact, there had been as yet no case of yellow fever, reported in town; the patient resided in a part of the city generally healthy, and inhabited by old Creole families, and which had never been, so far as I know, the nidus of any epidemic. The details concerning this patient are as follows:

Catherine Maxwell, a young girl about eighteen years of age, a native of Ireland, arrived in New Orleans, last January. She has light brown hair, a fine, white skin, marked with freckles, and has always enjoyed excellent health. For the past three months she has been employed by my mother-in-law, and worked at washing and ironing only. In consequence of a sore finger she had done nothing for eight days past, unless she read in her room. Tuesday, the 15th, she had gone to bed in good health, feeling no pain in any part of her body. At four o'clock in the morning, she was awaked by a violent headache, with pain also in her loins but not so intense. The face was of a crimson red, the eyes strongly injected, the pulse one hundred and twenty. The skin was covered with an abundant perspiration, which, nevertheless, offered a sharp acrid heat, disagreeable to touch. The tongue was white, thick, curved upon itself, with red dots upon the extremity. The gums were red, without a white border. The somnolent state was sufficiently well marked. There had been no evacuations for five days. I prescribed a saline purgative to be taken early the next morning.

Thursday, the 17th; I returned to the patient at 10, A. M., but found

that her sister had had her transported to Tremé street, between Ursuline and St. Philip. I hastened there, and found Catherine in a well-ventilated chamber, extended upon a couch, in prey to a violent cephalalgia, and extremely fatigued by the removal just made. Her general condition was the same. There was a white line running along the superior border of the inferior gums, none upon the superior. Prescription: salts in solution, at 2, P. M. No evacuations. At 7, P. M., the salts were repeated, with an ounce of senna leaves in decoction. Five copious evacuations followed, and the patient urinated while upon the stool. No change in her general condition. The white line invading more and more the lower gum. Prescription: lemonade slightly purgative to drink.

Friday, 18th; cephalalgia was much diminished, the white line was invading the entire lower gum, no vomiting, nor desire to vomit; slight drowsiness, pulse full, but very depressible. Drs. Faget and Alf. Mercier who saw the case with me on the 17th, also recognized the characteristic marks of yellow fever.

Friday evening at my visit, I learned that Catherine had been removed to the *Maison de Santé*, where I was enabled with the assistance of the attending physician to follow out the different phases of a disease, which had, so to speak, broken out under my eyes. From this time, I became simply an observer.

Saturday, 19th, prostration extreme, the face which was very red begins to become pale, and through or rather behind the palor, can already be seen a slight yellowish tinge. The pulse is less full, and excessively depressible. Copious black vomiting, suppression of urine-less cephalalgia.

Saturday evening. Same condition, no urine, extreme debility.

Sunday, 30th; pulse 96; sighing respiration, 36 in a minute; tongue divested of all saburral accumulations, and rather scarlatinous, coated about the middle with black matter; gums red and swollen, as though scorbutic. Icteric tinge manifest upon the upper part of the chest. Her sister told me that the black vomit had been frequently and copiously ejected during the night. Catherine succumbed on the afternoon of this day.

Reflections.—We have here a case of confirmed yellow fever, (the first if I am not deceived) which has suddenly appeared in a perfectly well ventilated room, in a house kept scrupulously clean, in a quarter

inhabited by the old families of the city, habitually exempt from yellow fever, where there is neither misery, nor other unfavorable hygienic conditions. The victim is a young girl, living in the city for the past six months, without having quitted it; robust, having enjoyed, until the present time, perfect health; properly nourished from the table of the mistress of the house; for the past eight days confined to her room, engaged during this time in no kind of work; not exposed to the heat of the sun, and having committed no kind of excess whatever. On the other hand, the officers stationed at the different quarantines have reported no ship arriving from an infected port, or carrying on board anything which resembles yellow fever. This case, even if it should remain isolated, does it not prove that the yellow fever may originate among us? Had it been permited me to have any doubts before, it appears to me that this case would decide the question and terminate my doubts, so clear and precise are the facts, divested of everything equivocal, and free from even the shadow of a doubt.

Now presuming that we are really menaced with an epidemic, can we foretell, with this case in view, what will be the character of this epidemic? Reasoning from analogy, we may venture to predict the adynamic form, which prevailed in Catherine's case. Now, the first case which precedes an epidemic, is an algebraical formula of the cases which ought to follow. Farther this is the character which has marked the diseases of the past month and more. A large number of physicians, as well as myself, have seen for some time past a quasi epidemic of atonic diarrhœa without irritation, or colic, and seemingly attributable to a diminution in the tonicity of organs.

C. Delery, D. M. P.

Although the case of Catherine Maxwell is, without doubt, the first well authenticated and indisputable one of the season, there is yet another now lying in the Charity Hospital, which is so very suspicious, that out of eight physicians who have pronounced an opinion to me, five have considered it yellow fever. As even the first suspicious cases are interesting, as forming part of the history of this disease, which may eventually throw some light on the grand *quaestio vexata*, we have taken the following notes.

Edmund Cook, aged about twenty-five years, a ship carpenter; came to New Orleans from Ireland several years ago. The summer of 1854

was his first summer in the city, that of 1856 his second; the summer of 1855 was passed on the Gulf shore, that of 1857 in Boston. The past two months he has been living on the job-boat Olivia, working, eating and sleeping on board. This boat has had no regular wharf, one day lying at one, another day at another. On Sunday, June 13th, the Olivia left the foot of Enghien street and proceeded down the river. Cook was attacked when about thirty miles below the city with severe headache, aching pains throughout the body generally, lassitude, deep seated chilly feelings, and what he forcibly designates as "heavy stretchings." Succumbing to this indisposition, he laid down on the exposed deck of the boat in the dew. The Olivia proceeded to the quarantine, some fifteen miles *below* where Cook was first attacked. He returned to the city on Monday on his boat which towed up a brig from the station; arriving in the city on the same day, he remained on board two days, suffering with a fever without remission. Prior to this trip, the patient had not been lower down the river than the Barracks (the Quarantine Station is some forty miles below these,) for a year or more. On the 16th he was admitted into the pay wards of the Charity Hospital. The preceding information is derived from the patient himself.

After his admission into the Hospital his fever continued until Friday, (fifth day of his sickness,) when it gradually subsided, and the eyes which were highly injected on his admission have gradually become more and more jaundiced, as also the whole cutaneous surface, until the time when first seen by myself, June 21st, 1858. At that time there was also an evident tendency to hemorrhage of the mucous membranes; the gums were swollen and vascular, and considerable thin blood was oozing from an excoriated surface near the corner of the lips.

This patient is doing well and has presented none of the more marked, that is, most fatal symptoms of yellow fever; but the continued fever, followed by remission on the fifth day, and subsequently by jaundice and a hemorrhagic tendency, are sufficient to render this case deserving of attention.

CHAILLÉ.

Professor Richardson.

Since the publication of our senior Editor's notice of Dr. Richardson's appointment to the chair of Anatomy in the University of Louisiana,

we have been permitted to peruse a letter to Dr. J. C. Nott, from a physician who is justly and preëminently distinguished, not only as an operator, but also as a surgical author and lecturer. While our regret for the resignation of Dr. Nott is not diminished thereby, it yet induces us to look forward with pleasant anticipation to the advent of his successor. A few extracts from this letter can not but prove interesting to the friends of this institution and of southern medical literature:

"I know of no man in all the country so well qualified to succeed you at New Orleans as Dr. T. G. Richardson, Professor of Anatomy in the Pennsylvania College of Philadelphia, and one of the editors of *The North American Medico-Chirurgical Review.* I know Dr. R. well. He was my private pupil, and was for eight, perhaps nine years, Demonstrator of Anatomy in the University of Louisville, and for a number of years one of the Surgeons of the Louisville Hospital. He is a fluent teacher, is popular with his class and colleagues; and is known, withal, as the author of a work on Anatomy, a new edition of which he is now preparing for the press. As a man he is without faults; he is making his mark, and what more could I say of him or for him?"

Professor Richardson can feel but little more gratified than ourselves, not in receiving even deserved compliments in an age when *printed* praise is so cheap, but in having them thus conferred fresh from the heart of a generous and appreciating friend, who did not destine them for publication; and whose approbation is the attainment of that which is so pleasing to every man guided by a worthy ambition, not the applause of the *profani vulgi*, but the "*laudari a laudato viro.*"

CHAILLÉ.

Death of Professor John K. Mitchell, M. D., of Jefferson Medical College, Philadelphia.

Man springs out of nothing, crosses Time, and disappears forever in the bosom of God; he is seen but for a moment staggering on the verge of the two abysses, and then he is lost.—TOQUEVILLE.

IN the organic world, waste and repair, organisation and disorganisation, birth and death tread on each other, march and counter-march,

mingle, blend, dissolve, and anon disappear "like the baseless fabric of a vision," while the pendulum of the inorganic world swings to and fro! ever! ever! in an eternal series, changeless and new as at the morning of the creation. "But man giveth up the ghost, and where is he?"

Ephemeral is life. It begins as it ends amid sighs. It is a promenade to the cemetery. It is man alone who loves life and knows its value; yet his is the melancholy privilege denied to brutes, of foreknowing the speedy and certain doom which is reserved for him—death, oblivion, nothingness. Even in the charming walks of science, the more he knows the more he feels the vanity and insufficiency of the utmost attainments of human knowledge.

Thus the pleasures of knowledge are mingled with pains:

> "The hearts that are soonest awake to the flowers,
> Are always the first to be pierced by the thorns."

Man's capacities for a never-ending improvement, the inadequacy of the present life for his happiness, and his irrepressible desires for immortality indicate, if they do not demonstrate, that for him there is a better world.

> "The soul uneasy and confined from home,
> Rests and expatiates in a life to come."

"To me," said Goëthe, "the eternal existence of my soul is proved from my need of activity; if I work incessantly till my death, Nature is pledged to give me another form of being when the present can no longer sustain my spirit."

Reader! in such a faith, accompanied with a cheerful discharge of duty, there is a moral sublimity which superior intelligences must admire.

> "Here will I hold."

John K. Mitchell, an orb of science, has disappeared from the medical heavens, but the lost pleiad has left a track of light along his pathway which other pen must delineate. Behold his bier! the mourners! Draw near his companions who bewail his loss. "Wait the great teacher, Death and God adore."*

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* These reflections were intended to precede an obituary notice copied from the *N. A. Med. Chir. Rev.* which has been crowded out. B. D.

Mortality Statistics of New Orleans, compiled from the Weekly Reports politely furnished by Dr. Baldwin, Secretary of the Board of Health. Population of New Orleans estimated at from 150,000 to 200,000.

Time.	*Total Deaths.*	*Children under 2 yrs.*	*Under 20.*	*U. States.*
January	352	....	170	204
February	362	....	175	250
March	424	153	223	296
April (5 weeks)	576	221	327	325
May	576	223	311	426
June (2 weeks)	266	136	168	195

Principal Diseases.	*January.*	*Febr.*	*March.*	*April.*	*May.*	*June (2 wks.)*
Still born	32	28	21	35	20	10
Tris. Nascent	22	10	16	10	13	7
Cholera Infantum	4	4	1	13	19	14
Infant Convulsions	19	26	39	41	34	26
Infant. Marasmus	...	...	11	28	17	11
Teething	...	...	5	16	23	12
Croup	13	8	10	16	13	6
Scarlatina	11	7	6	11	11	0
Rubeola	0	0	3	0	1	1
Variola	14	12	15	17	11	5
Diarrhœa and Dysentery	14	14	14	34	44	28
Gas. Enteritis	0	2	6	11	9	9
Inflammation of Liver	4	4	3	5	8	1
Inflammation of Lungs	13	22	28	27	16	3
Consumption	41	43	61	89	74	24
Apoplexy	8	9	9	8	10	3
Congestion of Brain	3	6	6	9	6	5
Fever Typhoid	6	12	11	12	13	2
" Miasmatic	7	2	3	9	10	6
" Yellow	1	0	0	0	0	0

The first death by Yellow Fever occurred on June 20th, a report of the case will be found elsewhere. CHAILLÉ.

MONTHLY SUMMARY—METEOROLOGICAL REGISTER.—*From the Medical Purveying Office, U. S. Army, N. O.* New Orleans, La., Lat. 29 deg. 57 min. 30 sec. N.; Long. 90 deg. W. Altitude of Barometer above the level of the sea, 35 feet.

1858.	BAROMETER.			THERM. ATTACHED.			THERMOMETER.		
MONTHS.	Max.	Min'm	Mean.	Max.	Min'm	Mean.	Max.	Min'm	Mean.
April	7 A. M. 27th. 30.406	2 P. M. 12th 29.794	30.079	2 P. M. 20th. 78	7 A. M. 14th. 62	70.62	2 P. M. 20th. 82	7 A. M. 14th. 56	71.50
May	7 A. M. 13th. 30.276	2 P. M. 10th. 29.860	30.121	2 P. M. 30 and 31. 85	2 P. M. 11th. 70	77.90	2 P. M. 30th. 85	7 A. M. 11th. 65	77.05
June 15	7 A. M. 8th. 30.300	9 P. M. 15th. 30.044		Several, 86	7 A. M. 15th. 75		2 P. M. 3rd. 86	7 A. M. 14th. 72	

1858.	HYGROMETER.			PREVAILING WINDS.	RAIN.	
MONTHS.	Max.	Min'm	Mean		Days.	Quantity.
April	2 P. M. 6th. 77	7 A. M. 27th. 51	67.19	South East, S. West and N. W.	3	2.26 inch.
May	9 P. M. 28 and 29. 80	7 A. M. 11th. 63	73.09	South East, South and East.	7	3.45 inch.
June 15	9 P. M. 1 and 2. 80	Several. 69		South East, South and North.	7	1.89 inch.

THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL
FOR SEPTEMBER, 1858.

ORIGINAL COMMUNICATIONS.

ART. I.—*Mortification of both Lower Extremities, caused by Coagula in the Common Iliacs—Amputation of both Limbs. Post-mortem Examination:* By J. C. NOTT, M. D., of Mobile.

THE subject of this case was a male negro, about forty years old, and the history of it previously to my first visit, is so imperfect as to leave much to be desired ; I have, however, been able to develop facts enough to show its unique character, and pathological importance.

It seems that about two years ago he was attacked with syphilis, and treated for primary and secondary symptoms, by two physicians, at various times, during twelve months ; after which he was pronounced well, and employed by the Government agent engaged in building a fort at Dauphin Island, thirty miles below Mobile. He labored there for about twelve months, complaining occasionally, but never lost more than a few days in a month, until the last month, during which he was off duty all the time, but I cannot learn with any accuracy, what was the train of symptoms. During his residence at Dauphin Island, his work required him to be much in the water.

He was brought up to Mobile about first April, and placed under the care of Dr. Woodcock, who informs me that his general health was much delapidated, and his principal source of complaint was a violent rheumatic pain in the right ankle joint.

After treating him some fifteen days, it was discovered that the toes of this foot were becoming gangrenous, and at this stage the Doctor requested me to see him in consultation. I found him very feeble, pulse rapid, great distress of countenance, breathing hurried,

loss of appetite, etc. He was placed on generous diet, porter, etc., and at the end of two weeks, a line of demarkation, between the gangrenous and sound parts, was established about the middle of the foot. I amputated the leg below the knee. On relaxing the tourniquet there was a moderate discharge of venous, *but not a drop of arterial blood*. Surprised at this, I examined the femoral artery as far up as it could be traced, and found it entirely without pulsation. On examining the opposite limb, its main artery was discovered to be in the same condition. *No pulsation whatever could be detected below Poupart's ligament in either limb.* The circulation in the upper part of body, radial and carotid arteries, etc., was preternaturally strong.

The stump was dressed and left untouched for three days, when the dressings were removed. Considerable union had taken place, and the stump looked well, except a little pallid about the cut surface. In a few days more, however, it became a little sloughy, but the gangrene remained superficial. A slight pulsation could now be discovered just below Poupart's ligament, but almost imperceptible, and it never increased beyond this.

In the meantime, the opposite foot had become very cold, tumid, dark, and assumed the appearance of incipient dry gangrene. In a few days, however, the cuticle began to separate, sloughing commenced and progressed rapidly. About 25th of May, a well marked line of demarkation was established some three inches below the knee. On the 29th, I amputated this limb by simply dividing the bodes and soft parts at the line of demarkation—not thinking it prudent, in his condition, to shock the system by a regular amputation through living tissues. He had for two or three weeks been becoming more and more dropsical, and died two days after this second amputation.

From all the circumstances, I could arrive at no other conclusion, than that the circulation had been arrested either in the iliacs or aorta near its bifurcation.

Post-mortem. Commencing at the femoral arteries, I found in them slender strings of coagula, but not sufficient to obstruct the circulation, and nothing to explain the mortification below.

On opening the abdomen, and following up the external iliacs, I found on each side fibrinous coagula blocking up the common iliacs at their bifurcations. The one on the right side was about the size of a nutmeg, and was composed of a nucleus surrounded by layers,

which had doubtless been deposited upon its exterior ; the nucleus was in a state of degeneration, and the whole mass was infiltrated with pus. The artery had become dilated around the mass, after the manner of an aneurism, and on the anterior part the internal and middle coats of the artery were absorbed, and the blood was retained by the cellular coat alone. The artery, above and below the clot, was clear and healthy. On the opposite side, the appearances were somewhat different. There was no enlargement of any part of the artery—no concentric layers, but a simple clot extending from the lower end of the common, through the whole length of the external iliac, closing the artery completely and adhering firmly to the coats of the artery. In both limbs the blood found its way imperfectly through the internal iliacs. The examination of these arteries was not carried below the termination of the femoral in the popliteal, nor farther up in the aorta than about three inches above its bifurcation.

There was considerable effusion of serum into the chest, and the pericardium was distended with it. The heart was very much hypertrophied, the left cavities being greatly dilated, and thickened in their walls. The aortic valves were much thickened, and covered with small warty excresences; beyond this there was not much appearance of endocarditis ; there were some opaque patches in left ventricle and several large, firmly adhering fibrinous coagula were found in both the left cavities. There were no tubercles in the lungs or elsewhere; the left lung showed evident signs of previous pneumonia, being hepatized in several parts.

The question now comes up, as to what caused the coagula in the two external iliacs, and consequent mortification of both lower extremities ? This question opens a wide and mooted field which it is not my purpose here to explore. My object mainly is to record a few curious facts, which may be used by those who have made pathology more a speciality than I have.

That coagula of blood will form in inflamed veins, or those in which the circulation has been arrested by ligature or otherwise, is well established ; but two very opposite theories prevail with regard to the formation of *fibrinous* coagula in arteries. Where endocarditis exists, as it did in the case just related, there is a well known tendency, to the deposition of fibrine on the inflamed membrane, and also to the formation of fibrine-clots within the cavities of the heart thus affected,

and these coagula are more or less adherent to the endocardium. Now, the plugging up of the arteries, as above described, would be explained by some in the following manner: A coagulum, (they say,) which has been formed *in the heart*, becomes detached—is washed down in the current of blood through the aorta, and finally becomes arrested by passing into some artery too small for its passage—the artery is thus plugged up by the mass of fibrine, the circulation arrested in it, and the consequences described, follow: rheumatism, syphilis, gout and other causes for endo-and pericarditis have been assigned, and there can be no question that rheumatism is its precursor in, perhaps, not less than fifty per cent. of the cases.

Other pathologists contend that these coagula may be formed in the arteries themselves, at points distant from the heart in consequence of a diseased state of the blood, or from the local effects of those diseases which we have enumerated, as causes of endocarditis. Those who are disposed to investigate this important point, will find much to interest in the works of Rokitansky, Paget, Jones and Sieveking, Kirkes, Bouillaud, Watson and others.

Similar coagula have been found in arteries of the brain, in the pulmonary and other organs, giving rise to softening of the brain, pulmonary apoplexy, and other lesions.

The tendency of the blood to coagulate in the veins during the puerperal state, (as seen in plugging of the pulmonary artery, in the femoral vein in cases of phlegmasia dolens, etc.,) presents another important phase of this subject. An interesting little extract connected with this point, will be found in the April Number, 1858, of the American Medical Journal, by Dr. Hewett, of London.

I regret that circumstances compelled me to make an exceedingly hurried post-mortem examination of the case above related, as it has many interesting points, and the subject deserves far more attention, than is generally given to it by the profession at large. It is to be regretted, too, that I could not obtain a more perfect history of the case, in order to trace, if possible, the connection between the condition of the arteries described, and the disease of the heart, rheumatism, syphilis, etc.

ART. II.—*Case* of Retention of the Fœtus for three weeks after Rupture of the Membranes and the Escape of the Liquor Amnii:* By M. TROY, M. D.

ON May 11th, 1858, I was called six miles in the country to see Ellen, a mulattress house servant, aged about twenty-four years, who considered herself to be seven months advanced, in her fourth pregnancy, and threatened with miscarriage. Her previous history presents some points of interest, which may have a bearing on the present case.

She was married and became pregnant for the first time in 1854, and when about seven months gone, fell out of bed one night in her sleep, and struck her abdomen violently against the leg of a table. The next day pains like those of labor came on, and it was with much difficulty that I could control them, and prevent a miscarriage. She, however, carried the child to the full term, and it is still living.

In her next pregnancy, the same kind of pains came on, at about the same period of gestation, and she actually did miscarry. The child was not viable. But, as I did not see her at this time, I do not know any other facts connected with this abortion.

She became pregnant the third time, and was again strongly threatened with miscarriage, but nevertheless carried the child to the full term, and it lived a few months. There seems thus to have been with her a tendency to abortion, at about the same period of gestation in all her pregnancies.

I may remark in passing, that unlike many negro women, she was not only very fond of children, but very anxious to have them, and looked upon a miscarriage as a very great misfortune.

When I saw her on the 11th of May, she had been suffering with irregular pains, for a week or more, and the usual remedies for them had been employed without effect.

She had been confined to the recumbent posture; opiates had been given both by the mouth and by enemata; sinapisms had been applied to her back, etc. In spite of all these, the pains had steadily increased in strength, frequency, and regularity, until now they came on every twenty or thirty minutes, and were very strong; her pulse

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* BENNET DOWLER, M. D.—*Dear Sir:* Thinking the following case may be of sufficient rarity to justify its publication, I forward it to you, without comment, to be used as you think proper.

Very truly yours,

Cawhaba, Ala., June 21st., 1858. M. TROY.

was quiek and weak. She was excessively nervous, restless and low spirited—being firmly convineed that the result would be fatal to herself or to the ehild.

Upon examination the os uteri was found not dilated but soft and easily dilatable—the finger entering it to the first joint with great readiness; the cervix was not obliterated, and the membranes could not be felt. There seemed to be very little effect produced upon the os by the pains, though the body of the uterus as felt through the parietes of the abdomen, and became very hard and tense during their continuanee. There were frequent desires to pass water, and some difficulty in micturition. I gave her:

Tr. Hyos.
Spt. Nit. Eth. aa. f. ʒi.
Tr. Camph. f. ʒss. M.

To be taken at once (6 o'clock, P. M.).

This prescription had the effect of temporarily allaying the uterine pains, and upon their recurrence, six hours afterwards, it was repeated. I remained all night, and saw her on the morning of the 12th. She had had only two or three pains through the night, but otherwise was no better. In fact the despondency and restlessness had increased, and the urine had to be drawn off by a catheter. There was no perceptible change in the os uteri. She said she felt that her pains were coming on again.

I gave her another dose of the hyosyamus mixture and directing a teaspoonful of laudanum to be given by enema, in case the pains became severe, left her. About 12 o'clock I was again hastily sent for, and reached her about 2, P. M. I found that the pains had rapidly increased since morning, and were attended with such excessive nervous excitement that convulsions seemed imminent at every one of them, till about noon, when, during a violent pain, there was a sudden gush of blood from the vagina which was immediately followed by the escape of a quantity of the liquor amnii. The hæmorrhage ceased almost immediately, but the liquor amnii still continued to dribble away with every pain, when I arrived. I found a great change in her general condition.

Every practitioner of much experience in midwifery, must have met with cases where the nervous or hysterical symptoms have been distressing and even alarming, during the first stage of labor—while

the os uteri is dilating, but have at once disappeared upon the change in the character of the pains, which usually follows the rupture of the membranes.

Thus it was with Ellen. She became quieter immediately, and slept in the intervals between the pains, which, also soon began to be longer. So when I arrived, she said she "felt like a new person,"—was in good spirits, and bore her pains, when they came, with fortitude. She was drowsy from the narcotics she had taken, for her mistress being greatly alarmed, had given her two laudanum enemata, instead of one, besides a considerable quantity of McManus' elixir of opium by the mouth.

Upon examination I found the os uteri dilated to the size of a quarter of a dollar, with the head of the child presenting. I could distinctly feel the hairy scalp, with nothing whatever, between it and the finger. Her pains however came on more and more slowly which I ascribed to the opium, and predicted a speedy delivery, with great confidence, as soon as its effects wore off. Further interference with the case seeming improper, and she remaining quiet, I left her again in the hands of a midwife, having remained with her as long as I could, unless the necessity had been very great. I left directions with her mistress, who is a very intelligent lady, for administering chloroform in case convulsions should occur, or be again strongly threatened, which, however, I did not now apprehend. I saw her no more till the 3d of June, three weeks and one day after my last visit. On this day she was delivered of a living male child. Her labor was quick, and I was not present till after it was over. She said there had been a little dribbling of water, through the labor, but no sudden gush, as there had been in all her previous confinements. After the 12th May, her pains came on at longer and longer intervals, till finally they left her altogether. She was afraid to leave her bed for nearly a fortnight, but eventually got up, and for the last week had been going about her business almost as usual, and in fact was up when the real labor pains came on.

Both mother and child are so far doing very well.

ART. III.—*Researches into the Types of Disease and Types of Therapy:* By BENNET DOWLER, M. D.

UNITE *these two methods* [*the experimental and the speculative*], *act like a good physical philosopher, who, in his laboratory,* THINKS AND EXPERIMENTS, EXPERIMENTS AND THINKS, *and makes use at once of his senses and of his reason. Begin with the method* A PRIORI *and give to it by way of counter poise the method* A POSTERIORI. *I consider the identity of these two methods as the only torch by whose light we may find our way.*—[COUSIN'S HIST. PHILOS.

No experiments can contradict truths derived from the observation of Noture. The great difficulty under which we labor in our experiments is the immense sacrifice of time and exertion required to imitate the conditions under which we observe the phenomena manifest themselves in Nature.—LIEBIG.

HAS the type of febrile diseases undergone a fundamental alteration during the last quarter of a century? Was the type formerly inflammatory? Is it now non-inflammatory? Was it sthenic? Is it now asthenic? It is not intended in this place to attempt a demonstration of the positive or negative answer to these questions; but it may be proper to offer presumptive evidence upon this subject—evidence derived not altogether from actual pathology, but chiefly from doctrinal and practical points of view, the validity of which may or may not be questioned.

Yellow fever will, perhaps, serve as well as any other disease for the purpose of illustration, with occasional glances at other maladies. Has the type of yellow fever changed in New Orleans, to go no further from home? Now, in order to test this question, an appeal may be made to the opinions and practice of physicians, dead and living, during the last twenty or thirty years. Were the physicians of that period agreed in ascribing this malady to localized or general inflammation—to gastro-enteritis or to general sthenic action? Leaving out of view the existing type whether that belong to sthenia or asthenia, it cannot be denied that in New Orleans, at least, physicians of equal respectability and skill were formerly divided in opinion and practice upon these questions. Had this malady been, on the one hand, unequivocally characterized by inflammation, or on the other, by debility and adynamia, would this fundamental antithesis of opinion and practice have existed to any great extent among physicians?

During the period in which many physicians considered yellow fever a gastro-enteritis, sthenic or inflammatory, curable only by repeated blood-lettings carried to fainting, including cuppings and leechings, others treated it either by the expectant method, or by quinine, porter, camphor, opium, and stimulants. Each party claimed superiority as to the successful treatment of the disease by modes really or apparently different. Now it is not reasonable to suppose that all

these therapeutic methods were equally good, yet they indicate, at least, that neither was so satisfactory as to be beyond question. A transition state followed. Blood-letting and active purgation declined, as did excessive stimulation, enormous doses of quinine, etc. The resources of Nature ; timely, simple, and moderate medication replaced excessive and perturbating remedial agents. Extremists began to approximate, to review their experiences and "reason together."

Did not the type of opinion rather than the fundamental type of disease change ? Gradually the rigid antiphlogistic treatment was, with few exceptions, abandoned.

There is no necessary connection between the mildness or the malignity of a disease and its fundamental type in respect to its inflammatory or adynamic character. Thus, if yellow fever be an inflammatory disease, it may be, nay, it really is, in some cases, mild, or aggravated in degree, though the same in nature or type. Again in puerperal fever (whether that malady be an idiopathic fever, or what is more probable, a puerperal peritonitis) the intensity of the disease in no wise changes its type, though it may modify its therapy. The pure phlegmasiæ exist in every degree without a fundamental change of type.

The aphorism, that extremes meet, is exemplified in therapy, the great finality of medical science. Who could have anticipated that Samuel Thompson, one of the most illiterate men that ever appeared in the medical world, and Professor Bennett, of Edinburgh, one of the most learned, should virtually meet on the same antiphlogistic platform, and agree in denouncing bloodletting even in the most acute phlegmasiæ, and inflammatory fevers ? A quarter of a century since, while Broussaisism was in the ascendant in the United States, Thompson, who had a million of adherents in this country, maintained "that fever is a friend," and he bound all with a solemn agreement, who practised his patented system, "not to let blood as was common with physicians." He denounced the whole faculty, of whom he said, that they "took the same method to cure a sick man as to kill a beast, that is, bloodletting."

At the present time, Professor Bennett represents a class of practitioners who virtually adopt the same theory in diseases of the most acute and inflammatory class. With them inflammation is, as Thompson would say, a friend—a natural method, to be interfered with

neither by blood-letting nor other antiphlogistic procedures. Thompson, however, gave lobelia-emetics to remove what he called the canker from the stomach and bowels. He then raised, to use his own words, as great a heat as he could by steam externally, and by No. 6, or red pepper and alcoholic liquors internally. Some of our modern philosophers feed and stimulate fevers. Where is the difference? Thompson was no skeptic, nor was his emetic weed a mere *placebo*.

A quarter of a century ago, books on liver complaint, and prescriptions for that almost universal malady abounded. A bilious type was assumed for many diseases. Large doses of calomel, powerful, prolonged and repeated purgations characterized the theory and practice, all being based on the antiphlogistic principle, whether as auxiliary to, or independent of, blood-letting. This treatment was carried to an excess for which the epoch was remarkable, and paved the way for that reäction which ever follows unwarranted extremism, be the type of disease what it may.

Admitting provisionally that the type of disease has not changed, it may be asked, how is the exisiting change in doctrine and practice to be explained and accounted for? In the first place, this change is not altogether fundamental among intelligent practitioners, who limit without wholly rejecting blood-letting and similar remedial agents, using, yet not abusing them.

The existing restriction imposed on antiphlogistic treatment is due, it may fairly be assumed, to the progress of science. As modern research has been most active, progressive, and successful in almost every branch of the healing art, it would be against all analogy to suppose that therapeutics was either stationary or retrograding all the while, seeing that it is beyond all other branches the most labored, the most important, and has been subjected to numerical and other methods adapted to test and appreciate its claims and achievements. Many unquestioned improvements and successes might be enumerated, even in therapy, affording presumptions at least, in favor of some other points which are still dissented from by individuals.

The great increase of the physical comforts, as to food, clothing, firing, ventilation, lodging, and other hygienic measures of modern times which may be supposed to prevent that deterioration of constitution and general health, which the opposite condition of things entailed a century ago (and much later), when the fevers, especially

typhus, were most probably of an adynamic type, and carried off hundreds of thousands among the poor, the ill-housed, half-clad, and starving.

The invasion of epidemic cholera, doubtlessly tended strongly to convince practitioners that excessive watery purgation, however induced, exhausted the vital forces and speedily ended in death. Here was a vast and all convincing experiment performed by nature, by which millions were struck down. The origin and the progress of choleraic discharges, though not identical with those produced by active and exhausting cathartics, nevertheless threw a strong light upon their dangerous character, and hence, induced greater caution in their use.

Previous to the invasion of epidemic cholera, the doctrine of Broussais, always adverse to the active cathartic treatment of fevers, had prepared the way, not for the entire adoption of his views, but for a better appreciation of the uses and abuses of cathartic medicines. "Hamilton on Purgatives," did not spare the British alimentary canal, but at a later period, the learned Professor Cooke, who wielded a powerful pen, wrote much, was doubtlessly conscientious, for more than a decennium anterior to the invasion of cholera, operated most extensively upon American intestines with ounce or even larger doses of calomel mingled plentifully with scammony, jalap and so forth, and drew after him an unexampled number of the faculty, including an almost incredible mass of the population of this Republic, especially in the valley of the Mississippi. Hamilton's purgatives "paled their ineffectual fires" before Cooke's. Cholera and menorrhagia, dysentery, fevers, and innumerable maladies were treated with enormous doses, the history of which will be read with astonishment by the next generation.

Of the many able physicians now living, who adopted Dr. Cooke's views, never fully recognized as valid in New Orleans, there is not probably one who has not repented and reformed, not because the type of morbidity has essentially changed, but because their minds have been changed by the progress of science, by a more enlarged experience, by the logic of events, by a juster interpretation of nature in both health and disease, and by a thorough conviction that, in many instances, neither the cause nor the cure of disease has been ascertained and fixed beyond the possibility of mistake. No therapeutic

generalization has yet been discovered of universal application not even in the acute phlegmasiæ.

The fundamental rule of therapeutics is, *do no harm;* if reason and experience indicate a mode of medication, be it antiphlogistic, stimulant or mixed, adopt and carry it out steadily, cautiously, hopefully, faithfully, unless the logic of events requires alteration.

Bleeding, purgation, blistering, mercury, tonics, diet, stimulants, and opiates are not necessarily incompatible. They may be beneficial or detrimental in sthenic or asthenic diseases according to the stages, and individual conditions of the patient. A sthenic affection of the brain or lungs may require blood-letting, purgatives and mercury in one stage, and tonics, stimulants, opiates, and diet in another, all tending to reduce the original diseased action or its ill effects, all counter-acting the natural morbidity in the one case, and all coïnciding with the curative action of nature in the other. The *modus operandi* of medicines is not *per se* sufficiently known to enable the practitioner to fix an absolute standard for either sthenia or asthenia without a continuous reference to the carefully observed developments of clinical experience, and the contemporaneous character and import of events, in time, place, condition, season, climate, race, age.

While more or less bias in favor of the present state of medical science, may enter into a comparative estimate with the past, yet the evidence in favor of the superiority of the present, is overwhelming, as might be shown by the simple enumeration of many recent discoveries. Nevertheless, in therapeutics, in which some important discoveries have been made, the number actually available in clinical practice is small compared with the discoveries and progress in physiology, diagnosis, microscopy, physiological and pathological chemistry, pathological anatomy, humoralistic pathology, medical chemistry, pharmacy, and operative surgery. But it may be justly said that the efficacy and the inefficacy of medicinal agents, the danger of entire non-medication, and of excessive medication, the destructive and healing powers of nature, are now better appreciated than formerly. The excessive confidence once reposed in very active medication is lessoned, or replaced with extreme caution, and, on many occasions, with reserve and timidity, the pretensions of art being resigned in favor of Nature, the *vis medicatrix naturæ*, of which more will appear in the sequel.

Upon reviewing the whole ground, the following conclusions may be regarded as probable: The sthenic type of disease during the first quarter of the present century and at the commencement of the second, was not so general nor so strongly marked as to justify the excessive blood-letting, purging, and other antiphlogistic measures which were generally practised. The non-medicationists of that period and of the present erred, and still err, in adopting the opposite extreme. The great body of enlightened physicians now occupy a middle ground between these extremists, inclining, perhaps, to the opinion that diseases are less inflammatory or sthenic than formerly, without adopting the opinion that all diseases are asthenic or adynamic, the antiphlogistic as well as the supporting treatment being adapted to individual cases. The former, however, is rarely, if ever, required to the exaggerated extent practised from twenty to thirty years ago.

But it has been, and is still asserted by some of the most prominent medical teachers and authors of the present day, that the type of disease has undergone no change, and that the decay of the antiphlogistic treatment is due to the progress of science. Thus, if yellow fever always was, and still is, a sthenic malady for the cure of which bloood-leeting must be carried to fainting and be repeated until the malady shall yield or terminate in death, how shall the general disuse of this remedial measure at present be accounted for? If that faith exists its works have nearly disappeared. If, on the other hand, the faculty have laid aside this treatment because they have found it ineffectual or injurious, the disease in the mean time having been of the same type, how is this to be explained without making a candid confession of previous ignorance, incompetency, and mal-treatment? Is mankind addicted to bearing testimony against itself?

Now, admitting what history teaches, namely, the weakness of human nature, a readiness to excuse one's self for errors and faults, (the first man excused himself by blaming his wife), nothing could be more plausible than to accuse the yellow fever of 1837 and '39, and especially of 1841–2–3, with sthenia, and that of 1853 with asthenia, seeing antiphlogistics would suit many physicians in the one case, and tonics, etc., in the other. Without making such a charge against any one, it may be here said in all charity, that a temptation to adopt such a mode of exoneration rather than the confession of error, incom-

peteney, and mal-practiee, exists. Henee the wisdom of the prayer—"Lead us not into temptation."

The faculty of New Orleans who have witnessed the epidemic yellow fever of 1837, and every subsequent invasion up to the present time, have had, beyond all others opportunities for investigating and determining the type of that disease, and the eomparative effieaey of the various methods of its treatment. The recorded experienees previous to 1833 and '37, not exeepting a few reports of the Physieo-Medieal Soeiety, are meagre and unsatisfaetory. The memories of preeeding epidemies, dim and ever fading among the most retentive, are now no longer available, being extinguished by death.

The faculty, who for the last twenty years have battled with New Orleans epidemics, represent many nationalities, have a medieal elientele of various nations and races, and have praetised the different methods of treatment whieh had been previously reeommended, or suehas appeared to promise more favorable results—whether the simple ptisans of mulattresses, or aetive mereurial catharties—rigid non-medieation, or twenty to sixty grain doses of quinine—porter, or blood-lettings, limited only by swooning, followed by cupping, leeeh-ing—foreed perspirations, in elosed rooms, under blankets, or eold spongings, ieed drinks, and free ventilation with open windows, the lightest possible eoverings—stryehnine, or sweet spirits of nitre—warm tea or cold hydropathy—a strong infusion of eoffee, or ten grains of tartar emetie hourly, ete.

These and many other remedial measures, whether viewed as specifies, or as adapted to remove speeial symptoms and complieations, were put in requisition either by the faculty, or by non-professional persons, eharitable assoeiations, mulattresses,* ete., and eannot fail

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*The following letter which appeared in a Northern Journal during the epidemic of 1841, is not altogether a caricature:

THE YELLOW FEVER.—The yellow fever, although it has visited us twenty times in the last thirty years, is by no means treated in a uniform way by the doctors. One bleeds—another says to bleed is to kill, a third gives a decoction of green coffee; a fourth calomel; a fifth quinine; a sixth the Sydenham potion; a seventh purges with salts; an eighth laughs at all purgatives, and claps blisters on the feet and stomach; a ninth covers you with blankets; a tenth congeals you in ice; and some do nothing at all but nurse the sick man, letting nature take her own course. Of all these treatments, it is hard to say which is the most *un*successful. What kills one, cures another; and the work of the destroying angel goes on regularly and proportionate, and, as if, by way of satire, the malady has fallen upon the doctors themselves. Two promising young physicians, Lafon and Delavigne, died a few days ago of the epidemic. The latter belonged to the American doctors; they bled him, etc., etc., following what is called the American practice, every effort was used to save him. When he died, the French physicians shrugged up their shoulders and sneered at their American brethren; but down went Lafon, and in spite of "sharp-pointed soup," or *bouillon pointu*, as they call a certain mode of giving a cool dose to the bowels, in spite of blisters and lemonade, etc., etc., off he went, to the utter confusion of the French practice. With this total want of success on all sides, you see what must be the dreadful state of anxiety, fright and uncertainty in which those who are unacclimated are constantly kept. Strange, it seems to me, that after so many years of experi-

to afford the philosophical observer data for serious reflection, and some probable conclusions.

The faculty, always the most competent judges, have been at length very generally convinced from abundant experience, that in not a few cases the fatal tendency of yellow fever cannot be averted by any known remedy. They look with distrust on all modes of treatment which claim to be universally successful.

Distrusting polypharmacy and perturbating remedies carried to extremes—it may be repeated, they find in nearly all modes of treatment valuable resources which may be relied on in particular cases, either as curing, or as enhancing the chances of recovery when skillfully applied.

The apparent antagonism of types of disease, and of modes of treatment, is, after all, for practical purposes, to a great degree, conditional and contingent. Thus, as already stated, an acute pneumonia, pleurisy, or peritonitis which may require blood-letting in the early stage, may afterwards equally require stimulants and tonics to obviate debility, and simultaneously opiates and mercury, to remove exudations, effusions, indurations, or other effects of the antecedent stage.

Although it is not intended to extend this paper by data copiously recorded in manuscript, illustrative of the progress of medication in the yellow fever of New Orleans, the following note made a few years ago will show the decline in regard to heroic doses :

During the prevalence of the yellow fever *anno* **** I copied from the books of the Hospital, the prescriptions in six wards from September 1st, to the 13th inclusive. I omitted simple drinks as ice-water and lemonade. I estimated the average daily number of yellow fever patients at sixty, and other patients at thirty. In thirteen days these patients used, of calomel, twenty-five grains ; blue mass, ninety-six grains ; salts, one dose ; castor oil, four doses ; enemata, twenty-eight ; blisters, nine ; general blood-letting, four, and local, three ; cold to the head, one. In one ward sulphate of morphia was given oftener than any other medicine, but the number of doses is not stated. Hence, in six wards the daily consumption of calomel was less than one grain.

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ment, and of experience, nothing definite or settled is arrived at in the treatment of this infernal sickness. As to myself, I fear it not. I was born yellow fever proof; for I write from my native city.

It is now settled that yellow fever is not caused by miasmatic exhalations, nor by excessive heat, for it is extremely fatal during a season of daily rains, which constantly cool and purify the atmosphere. A fresh north wind is now blowing. What is the cause of the epidemic no doctor can tell.

In another large ward filled chiefly with yellow fever patients, the prescriptions for one day comprehended, one porteree ; six ice-water ; four gum-water iced and lemonade ; two blisters ; one sinapism ; six ounces brown mixture ; two grains of tart. emet., and half a drachm of syrup of morphia. In another similar ward, in the same month, the prescriptions for two days may be thus summed up : foot baths, two ; barley water with elixir of vitriol, four ; quinine juleps, four ; enemata, three ; cold to the head, one ; quinine, ten grains ; camphor liniment, one ; blisters, two ; castor oil three ; lemonade six ; gum-water 6. In some other wards, the swooning-blood-lettings *(les saignées syncopales)* were practised as fully as if M. Bouillaud had been the medical attendant. Many physicians, including M. Thomas, considered the epidemic of this year, highly inflammatory, requiring the long continuance of an energetic antiphlogistic treatment. *(Traité Prat. de la Fièvre Jaune. Paris.)*

Several medical men (during this epidemic), who practised repeated syncopal blood-lettings *("saignées syncopales réitérées")* lost only four in the hundred, others one in fifteen, acccording to *their* published statements !

A writer on the yellow fever of New Orleans, opposed to cathartics in this disease, who "never saw an instance fit for tonics, nor hæmorrhages except when mercury had been taken," and, who, by bleeding, "in the horizontal position," was enabled to give his statistics in the following pleasing words—"out of my seventy-five cases only six died." Nevertheless, Dr. La Roche in his learned and colossal work on yellow fever, finds that in the United States, the mortality statistics of this disease give nearly one death to every three patients, instead of one in twenty-five, one in fifteen, or one in twelve.

Another medical gentleman during a later epidemic, reported that he had had seventy-seven cases, and cured all but three—another that he had treated one hundred and lost four. A respectable apothecary, however, (since dead), declared that he knew of many, including one entire family, that died under the treatment of this gentleman at the same time and in this same epidemic. This treatment began now to assume a new name, that of "*spoliative* blood-letting," instead of *jugu-ler*. Of this latter, the late M. Magendie, in his published lectures, hinted that it is not the disease, but the patient that is *jugulé*.

The late Dr. Drake, in summing up yellow fever statistics, found

that for sixteen years ending in 1843, the mortality in the Charity Hospital's "accurate registry" amounted "*to one out of two*," and that "all the available data give an average mortality over forty per cent." Private practice, is for obvious reasons far more successful.

These brilliant cures were not witnessed in the public hospitals; and instead of dwelling upon them, it may be better to return to the line of investigation with which this paper set out.

It is not Thompson, but certain eccentric skeptics among the otherwise learned, who contend that inflammations and fevers cannot be in any instance cut short or arrested; that these maladies must run a definite course uncontrolled, as regularly as the seasons, without hindrance from art, as if nature is altogether competent to practise physic, but is the worst of quacks in setting a broken bone, reducing a luxation, extracting a stone, or a tooth, reducing a hernia, closing a hair lip, and in performing cæsarian and most of other curative operations. That nature's morbid processes are essentially healthful and curative, and that the healing art can do nothing but aid, abet, and accelerate the march of the former, are assumptions too absurd to be admitted, being not only contrary to experience and analogy, but self-contradictory, claiming, yet in no wise receiving unequivocal support from recent microscopic investigations. The latter how important so ever they may ultimately prove in pathology, are not sufficiently matured for a basis of therapy; or they may be admitted without invalidating the question of sthenic or asthenic treatment. Nor can it be justly affirmed that the great advances recently made in diagnosis prove or disprove the possibility of controlling inflammations, fevers, etc. Diagnosis, though of the utmost importance, does not necessarily carry with it the method of cure in phthisis, cancer, cholera, heart-disease, etc.

The diagnosis of cholera is easy. But it does not follow that the cure can be deduced from this diagnostic knowledge The precision with which consumption is now diagnosticated is among the most remarkable advances in modern pathology, but the mortality statistics of the present time will show that the cure of this disease has not progressed in a ratio comparable with that of its exact diagnosis. Hence the fallacy of theorizing in therapeutics solely on this basis. Intermittent always existed, and is easily diagnosticated, but this latter did not contribute an iota towards the discovery of the Peruvian bark.

This statistical argument, however decisive in itself, is virtually neutralized by the significant fact that every mode of treatment from the heroic to the infinitesimal claims its protection and takes shelter under its autocratic sanction. Figures cannot lie. In the celebrated medical school of Edinburgh, Dr. Henderson, Professor of General Pathology, finds figures proving the superiority of his homœopathic treatment, and in the same school, Dr. Bennett, Professor of the Institutes of Medicine, teaches that his figures in favor of no treatment, at least no antiphlogistic treatment, are still more favorable, and that the figures in even pneumonia for instance, "*satisfy him that blood-letting lengthens rather than shortens the progress of the case. The real disorder is prolonged, and rendered proportionally more fatal by that practice.*" Now M. Bouillaud has tables showing that in numerous cases French pneumonia, in which blood-letting averages for each patient four pounds and nine or ten ounces (French), yields with great promptness, giving the minimum of mortality, when the disease is thus *jugulated*. In some hard cases, ten pounds were required to jugulate or strangle this disease. *(Philos. Méd.)* This is nearly as much as I have known to be taken in individual cases of yellow fever in New Orleans. Prof. Bouillaud's practice was fully tried, and even his neologies adopted, in this latitude, and that too, by able physicians, a number of whom, the first surgeons of the world must own as their equals, when put to the test. This subject will be referred to again.

But to return to the Edinburgh Professor :

Dr. Bennett's test disease, pneumonia, is a fair type of inflammation as it exists in the parenchymatous viscera. Here he plants his theory, appealing to statistics to show the pernicious effects of antiphlogistic treatment and the truth of his views. In order to accept his therapy of the phlegmasiæ, it is necessary to discredit the almost unanimous testimony of the past and the present. The presumption is, therefore, altogether adverse to his isolated opinion. But this is not all. His medical compatriots, and more especially Dr. Bell, deny the authenticity of his special statistics. In this state of the controversy and uncertainty the preponderating evidence is by no means in his favor. A small portion of Dr. Bell's paper invalidating Dr. Bennett's statistics and logic has already appeared in the New Orleans Med. and Surg. Jour. ; its length prevented its insertion entire.

Hippocrates after enumerating the symptoms of acute pleurisy and peripneumonia, lays down this rule of treatment, namely, "venesection; the quantity of blood drawn to be proportioned to the constitution, the season, the age and the color of the patient; if the pain be acute the bleeding should be boldly pushed to syncope; afterward, an injection is to be administered." Has modern microscopy led to a sounder rule? Experience has limited its application by the circumstances indicated by Hippocrates; the syncope is an accident, but not the object aimed at, which is relief. Centuries later, Celsus said with due circumspection that for this disease, the vigorous should be bled—the feeble cupped: *(Oportet, si satis validæ vires sunt, sanguinem mittere: si minores, cucurbitulas, etc.)* (l.iv.c.vii.)

M. Bouillaud lays great stress upon the diagnosis of disease as the certain key to the cure: "If there be an axiom in Medicine," says M. Bouillaud, "it is, that no disease exists without a seat. The determination of the seat of diseases is one of the most beautiful conquests of modern medicine." *(Essay on Med. Philos.* 259.) This determination of a special and invariable seat of yellow fever remains to be revealed. Early in this century some of the most prominent British writers, for instance, Dr. James Johnson, considered the seat of yellow fever to be in the liver, the disease being virtually a hepatitis, while in Paris it was called a gastritis. Now, assuming provisionally yellow fever to be a local inflammation of the stomach and bowels, or of the liver, its mode of cure by blood-lettings pushed to syncope is not a necessary consequence of this pathological condition, nor is the absolute interdiction of purgatives thereby proven to be necessary, as the expulsion of fæcal and morbid excretions, not to mention their antiphlogistic actions in other respects, may be beneficial. Experience alone is competent to decide in these cases. Analogy, nay, direct evidence, shows that a treatment which reduces the strength and vital forces without diminishing the disease, is injurious.

On the supposition that blood-letting carried to syncope cuts short, or controls the violence of fevers and inflammations, there is great danger in its continued repetition, seeing that it may prostrate the inherent vital forces, without which the natural cure, so to speak, must be precarious, for without a certain *quantum* of this inherent vital power, tonics, stimulants and diet, must fail to insure recovery how much so ever the original disease or its accidental complications may be subdued.

Nevertheless, the natural method of curing a malady is in many cases wholly unreliable, unless art should interfere to remove or check injurious agents, morbid conditions, and unfavorable circumstances so that Nature may be the better able to cure the disease by recovering her normal reign, abdicating always in favor of old age. Therapy is virtually based on a duality, namely, morbidity and health —a duel in which the doctor may often heal wounds not necessarily mortal.

A philosopher after much research, devoutly concluded it was a remarkable proof of the goodness of Providence that great rivers always ran by great towns! The Pantheistic notion that the whole universe, nature, matter, disease, etc., is God, may be distrusted in clinical medicine. Nature in disease is very often an arrant quack of the most dangerous kind, a blind, unintelligent necessity, without medical education, to be opposed with force and arms. The founders of christianity performed miracles to oppose Nature in disease Doctors of these latter times not being thus endowed, fight or control her, not the Almighty, with physic, scalpels, splints, forceps, chloroform, vaccination, and so forth.

Broussais' doctrine, which was almost exclusively of the inflammatory type, had scarcely begun to wane in New Orleans, when the work of the distinguished Professor Bouillaud, on Medical Philosophy appeared. *(Essai sur la Philosophie Médicale et sur les Généralités de la Clinique Médicale.* Paris. 1836.*)* His type of disease was also inflammatory and his remedial type (which, it is said, he still adheres to) was blood letting carried to an extreme without parallel. The English language is too poor to express his sanguinary practice: Swooning and fainting are innocent babes compared to his *coup sur coup, toute puissante, juguler*, etc. The result of this *Thugging practice*, which he calls the new method, are given in his book, fortified with a profusion of arithmetical tables to establish its almost uniform success in pneumonia, pleurisy, heart inflammations, inflammations of the stomach, bowels, and other abdominal organs; also in rheumatism, anginose affections, erysipelas,* and fevers.

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*From April, 1834 to March, 1836, M. B. treated fifty-three cases of erysipelas of the face, having cured all with astonishing facility and rapidity by general and local blood-letting, employed *coup sur coup*, (p. 415,) amounting in several cases to four or five pounds;—"*la maladie etait reellement jugulee.*"

Van Helmont, (born 1577 died 1644) who rejected blood letting which he maintained exhausted the *archæus* or vital spirit, lost his life from an acute pleurisy, which, as Guy Patin, a contemporary doctor, said, might have been saved by venesection. This celebrated Guy Patin practised "spolia-

In typhoid fever, M. B. claims to have cured all but twenty-two out of one hundred and sixty-eight cases, nearly one in eight, instead of only one in three according to MM. Chomel and Louis' statistics. These M. B. calls "the results of his method—the new formula." Be this as it may, the treatment of yellow fever in New Orleans by serial sanguineous emissions limited only by swooning—*coup sur coup, juguler,* ended in more than distrust, notwithstanding the ability, skill, zeal and benevolence of its advocates ; some of these are now dead, others have returned to their native skies, and many it is believed, after an abundant experience and matured reflection, have modified or dissented from their previous opinions.

"I was," said Bordeu, "a dogmatist at twenty ; an observer at thirty, an empiric at forty, and now at fifty, I no longer have any system." When I was young, said a physician, I had fifty remedies for every disease ; now I find fifty diseases for which I have no remedy. True most venerable centenarian ! But have you not seen veritable discoveries, almost innumerable, particulary in surgery ? Did vaccination. quinine, iodine, anæsthetics, and other modern achievments enter into your most youthful anticipations ? Nevertheless an enlightenced experience tends to reduce or rather to exclude from practice a vast majority of the drugs enumerated in the dispensatory.

The diagnostic question of sthenia and asthenia (now that diagnosis is viewed as the *io triumphe* of modern medicine) would seem to be one upon which diversity of opinion should no longer exist ; but in practice it is not so, judging by the actualities of therapeutic standards. It will be seen in this paper that many physicians of equal ability in New Orleans, (where yellow fever is probably best understood) have in every epidemic for a quarter of a century, differed in opinion and therapy on this question. It is the same in other cities. For example, in Paris, two of the most eminent medical men, the best representatives of these two fundamental types of therapy, M. Bouillaud (whose extreme antiphlogistic treatment has been already adverted to) and the late M. Magendie, who adopted the most opposite practice, will serve to prove the truth of this proposition.

A few years ago M. Magendie, in his lectures in the College of France, summed up his conclusions as follows :

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tive blood-letting" equal to Rush, if not Bouillaud himself, having bled his son, in a fever, twenty times—a lad aged seven, for a pleurisy. thirteen times—the king's physician, sixty-four times for rheumatism "There is not," says he, "a woman in Paris who does not believe in its efficacy even for her infant when affected with small pox, measels, convulsions, teething," etc.

He said, while disavowing all belief in homœopathy, that a "a physician would cure a patient with globules, if the patient had faith in them, better than with the most appropriate medicines, if he distrusted their action. The benefit of blood-letting is due to the imagination or moral effect. For more than ten years I have not found it necessary to have recourse to a copious bleeding ; in other words, I have endeavored to act on the mind of the patient rather than on the circulation." In the *Hôtel Dieu*, M. Magendie treated pneumonia and other acute diseases with hygienic means, as temperature, diet, etc. Here then, are two among the greatest teachers, writers, diagnosticians, practitioners, and experimenters of the school of Paris, who present therapeutic, if not diagnostic, antagonisms unparalleled among eminent men in New Orleans. The rule which some have proposed, that is, trial blood-letting as a test of type for sthenia or asthenia is fallacious. I have witnessed cases wherein I could not discover any of the usual phenomena of inflammatory action, in which six or seven pounds of blood had been taken in from two to three days without a fatal result.*

As the recent counter-march from the antiphlogistic camp to that of asthenia and semi-medication is due neither to the British nor American schools, but to that of Paris, or rather that of a party in this capital of which M. Magendie was autocrat (whose funeral eulogium has recently been pronounced), it may not be irrelevant to the scope of this paper to recall to mind some of his fundamental views, which will be condensed from his work *on the Blood*. Whether these views be true or false the reader will determine for himself. Truth cannot be crushed out by the great name of Magendie, if he shall be found in error.

"When I commenced my medical career, imbued with the prejudices of the schools, like my brethren I paid my tribute to scholastic dogmatism ; that is, I believed in inflammation, irritation, etc., as so many articles of faith." 107. In repeating some experiments of Sir B. Brodie on the ligation of the common bile duct in which all the animals died of peritonitis, M. attempted by copious blood-letting before experimentation to prevent this, but found that it had the contrary effect, increasing the inflammation, etc., which also, induced him

* In 1809, Dr. Lucas, of Virginia, bled Capt. Niblett, in less than a month 57 pounds troy, besides cuppings, leechings and the use of setons, for a pneumonia.—*Med. Repos.*

to condemn bleeding before surgical operations. 107–8. He declares that by bleeding animals (dogs) from time to time he finally kills them with inflammation, produced by the blood-lettings, as engorgement, œdema, pneumonia, and "the entire train of what people call inflammatory phenomena." The serosity of the blood increases, fibrin diminishing in these cases. 19. "What on earth is the meaning of irritation? What sense is there in applying the word inflammation to our organs? Do our tissues really take fire? I confess I know of no example of such a phenomenon. 32. The fundamental point in the theory of the blood is, that in order to support life it must be coagulable; if it loses that property existence is threatened. This is what occurs in the greater the number of epidemics. 28. Viscosity of the blood is necessary to its free and healthful circulation. Thin blood or water will not enter where viscous will. 35. Blood-letting changes the relative proportion of serum and clot, changing the quality of each, with a consequent change in the organs causing the very diseases which venesection is intended to cure. Uncoagulable blood cannot traverse the capillary vessels. 40–1. He refers to "rheumatism which yields to bleeding, tartar emetic, to every imaginable kind of treatment—yields above all to rest, and diluents. In my hospital practice I never have recourse to the lancet, to tartar emetic or to leeches, yet all I have treated have recovered." 177. He opposes tartar emetic in thoracic affections as given by Laënnec, maintaining that it attacks the blood chemically, decomposing some of its elements, producing in the lungs the diseases which it is given to cure.

"In serous apoplexy, acute hydrocephalus, etc., venesection diminishes the proportion of the fibrin, of coloring matter of the blood, increases that of serosity, facilitates exhalation, and, therefore, seems to me a most likely plan to aggravate the violence of the symptoms." 110.

"I hesitate not to declare, no matter how sorely I shall wound our vanity thereby, that so great is our ignorance of the real nature of the physiological disorders called diseases, that it would, perhaps, be better to do nothing, and resign the complaints we are called on to treat, to the resources of nature, than to act, as we are too often compelled to do, without knowing the why or wherefore of our conduct, and at the obvious risk of hastening the end of our patient." 88.

Amid the difficulties and doubts which still reign in pathology and

therapeutics, the inquirer should not adopt as ultimate truths the prevailing illusory phrases, "*vis medicatrix*," "God in disease," "the natural method," "expectant method," "*les juguler*," "self-limited diseases," etc.; instead of these, let him substitute further experiments, observations, and deduction. Thankful for what science has already achieved, let him labor, hope, wait.

If the epidemics of New Orleans have taught its medical faculty that no medication yet known is beneficial, this is a discovery of high import to the well being of society, since it will soon or late obtain influence over lay-medication, the type of which is often very active, consisting of emetics, purgatives, etc., and consequently, injurious. If the former modes of treatment have been too active and perturbating, now is the time to reform them—to restrain the interference of art so as not to thwart nature ; if nature can be assisted with physic and hygienics, be it so ; prescribe the formula. This anthithesis between nature and art in therapy, is as fanciful as it would be to assume an opposition in agriculture, manufactures, cooking, etc. Intelligence and skill harmonize these two agencies and give mutual support.

If sheltering themselves under the authority of Sir John Forbes, physician to the Queen, Professor Bennett, the late M. Magendie and other physicians still more skeptical in the art of healing, and altogether opposed to medication, the faculty should renounce the practice, but not the study of medicine, much evil and probably some good would be the result. History, daily experience, the hopes and fears of mankind prove that remedies, good or bad, will continue to be given for the removal of suffering and for averting the danger of impending death. As almost every individual would have a different theory, method, remedy, or dose, this universal empiricism would afford the true physician new data for study. Some of the most remarkable cures I have witnessed, have been accidentally made by persons ignorant of physic.

If the signs of the times be not illusory, medical opinion and practice are receding from the reliable land-marks of the past. The antithesis of the healing power of Nature and Art, a *petitio principii* at the best, is gaining adherents and assuming consistency under various disguises and scientific pretentions warranted neither by experience nor medical statistics. Thus, Prof. Bennett teaches his class the fundamental doctrine "*not to attempt to cut short even pneumonia*,"

etc. Some medical men who advocate a positive therapy, qualify their support with so much reserve and lurking skepticism as to place themselves under a neutral flag, or among friendly enemies more dangerous than the openly hostile homœopathists, hydropathists, spiritualists, and self-styled reformers of the physico-medicalist and Thompsonian sects.*

Legitimate Medicine has in all ages encountered assaults by enemies from without and by dissentions from within its pale, but at no time have adverse circumstances or illusory opinions been more prominent during the last fifty years than at present, notwithstanding the scientific progress made in all this period. During the last few months the changes of opinion originating within the profession have been greater perhaps than ever occurred in so short a period—changes the most disastrous or fortunate—the most disloyal and false, or disinterested, truthful and beneficial to society. If medical treatment be a snare, a cheat, and mischeivous ; if nature be competent to cure without the assistance of art, and further, if art, how skilful so ever may its interferences, tends only to enhance the dangers of the sick, serving but to prolong their maladies, the physician's "occupation 's gone." Medical colleges, journals, and books should sing hosannas to morbid Nature, and hurl maledictions against her enemies and the enemies of the afflicted, that is, the doctors.

An author who has unfortunately written a book, or essay in which he has "made up and expressed an opinion" upon a method of treatment, is apt to be biased in its favor. "When Dr. Sangrado was told that all his patients died, and that he must, therefore, alter his system of bleeding and warm water, he replied—'Impossible ! for I have written a book upon it.'"

"O, that mine enemy would write a book !" said Job. If he meant a book on the practice of medicine written to sustain an ill-founded and exclusive method of treatment, this prayer impends no good but great danger to the enemy aforesaid, as he exposes himself without cover to the fire of all who differ from him, and worse yet, soon or

* An epitaph on the tomb of the old Ambrose Paré, now much quoted and true enough in a certain pious sense, reads thus: "*Je les pansay et Dieu les guarit.*" "Empirics may boast that they cure, and doctors of divinity may sustain them, but the physician knows that it is God who healeth all our diseases." Whereupon *The Southern Medical Reformer* (Thompsonian) for July, says: "Such a letting down! What! The Doctors cured no diseases? What, then, are they good for? If they cure none, don't they sometimes make them worse? Have the thousands of constitutions that the Doctors have ruined confessedly, been sacrificed to a vagary of the imagination?" Since the age of miracles has passed away, "God healeth our diseases," as he maketh our cotton, sugar, railroads steamboats, telegraphs ; or cutteth the stone out of the bladder, etc.

late he may fear annihilation from the batteries of experimental research, or from some newer dogma equally confounded.

It was said of Hamilton's book on Purgatives, that it quadrupled the sale of purgatives in England. When it was translated into French, more than thirty years ago, M. Tavernier in his review of it, exclaims, "Ah! if French physicians could adopt the practice of Dr. Hamilton, what a number of people will consider it as the regeneration of medicine in this country! With what exultation will the whole class of pharmacopolists view the termination of a calamitous revolution, by which their shops have been left deserted and their drugs allowed to rot! If purgatives should come into vogue, what will become of the poor leeches?" Americans, however, will not allow apothecaries to starve for the glory of a theory. If the faculty renounce drugs and adopt the so-called "natural method," the people and the apothecaries will not. They will take physic as well as vote, and quack advertisements which already occupy a large space in the newspapers, will increase in proportion to the decline of prescriptions by the faculty. Abstractions die, but druggism is immortal.

It does not follow because neither pneumonia nor yellow fever, etc., can be *thugged* by "spoliative blood-letting," nor by "the abortive treatment," that treatment can in no wise directly contribute to the arrest or the favorable modification of the disease. Suppose, for example, that you ride twenty miles among the hills of Western Virginia, to see a robust farmer suffering from inflammation of the lungs, or pleura, and, after bleeding him, you hear him say, the pain is gone, and then, perhaps you see him turn over and go into a sound sleep. In a few days after he gets well, with or without other treatment. Are you to conclude with some closeted microscopist whose facts and theories concerning the cells, their anatomy, physiology, combinations, growth, life, and decline are truly interesting, yet do not afford a basis for medication, nor prove the expediency of doing or not doing any thing whereby "*the exudation of the liquor sanguinis*" in inflammation may be modified—I say, are you to conclude, that you have in the malady aforesaid been guilty of mal-practice in furnishing aid and comfort to the enemy? Are you to adopt a histological *non sequitur* instead of falling back on clinical experience and medical statistics?

Mother-wit is a guarantee that the unnatural defection in the ranks

of Medicine, now prevalent, can only be of temporary duration. Whatever changes may occur in the type of diseases the very skeptics whom "too much learning hath made mad," will consent to be purged, blistered and bled, or stimulated in the trying hour, just like their more loyal brethren who still practise physic under the belief that it is an honest business, and withal beneficial in removing obstructions or morbid conditions, so that the healing powers of nature may the more readily rehabilitate the healthful functions of the economy.

These questions of type and of therapy, or rather suggestions in relation to medical facts and the opinions deducible from them, including the modes of treatment found to be beneficial or otherwise, are submitted to readers, who are earnestly requested to investigate these subjects and give their results to the world in this Journal. Discussion may elicit the truth, and if men cannot be convinced and converted, they may be induced to think and to reason. "I question," says Mr. Surgeon Johnston, the author of *Nuces Philosophiæ*, "whether any man was ever convinced by the arguments of another. All that a reasoner can do is to set his readers *a-thinking* in the right direction. He gives them a clue, and then they either dress up his arguments in their own language, and please themselves with believing them their *own*, or else they do really discover new arguments of their own on the same side by which they are convinced. And there is reason for this. For the same natural law which makes it offensive to a man to be beaten with a stick, makes it offensive to be beaten with an argument. There is a distinction but no difference."

Home facts and home opinions are needed. What has been done—what is now doing abroad, should not be overlooked or neglected. But the rural practitioner in the South will not always find in the clinical reports from foreign lands, from crowded pauper hospitals, under different skies, among physically deteriorated and crowded populations, precise and available information to guide him in the treatment of patients of different climates and races, scattered over a territory as large, perhaps, as some of the German States. A home practice or a home book is much needed, a periodical book of record. Such the readers can make this Journal. A faithful history of medical practice contributed to its pages from all parts of the expanded South, would be of mutual advantage for mutual instruction, and

would greatly advance the art of healing, especially in rural localities, to which the practice in the filthy purlieus of crowded towns and hospitals, may not be always strictly applicable. Such contributions, though small and sometimes imperfect, viewed singly, become, in the aggregate, of inestimable value, and would soon rival in utility, if not in splendor, more pretentious volumes. Home essays, imbued with home practice and thoughts, adapted to home populations and places, are due to yourselves, and will command the respect of non-resident medical compatriots and foreign cultivators of science. An American school! Yes, a Southern school with its four millions of negroes, and peculiar climate, diseases, and topography, is a desideratum.

Is there not a necessity for inquiring whether there be not a rural as well as an urban school of clinical medicine? The city physician, usually the bookmaker or monographist, sees in hospitals and among the poor who greatly preponderate over the affluent, a comparatively debilitated population, badly housed, badly nourished, badly aërated, and often badly intoxicated with bad liquor? Is the city type of therapy adapted to the vigorous populations, white and black, in the country, where agricultural pursuits constitute the employment of nearly all, where food is fresh, wholesome, and abundant, where the muscular forces are highly developed, where the blood is aërated with pure breezes from plain and hill, and where, it may be presumed, an acute malady treated with more or less energy, will not be followed by a collapse of the vital forces? Let the country physicians teach, that is, write, as well as the physicians of the overgrown cities of Paris, London, New York, Philadelphia, New Orleans.

The tendency of medical opinion in favor of the importance of hospitals as schools of clinical education is great, increasing, and, in this point of view, ought not to be diminished. But there are so many unfavorable mental influences and hygienic circumstances which modify hospital practice, that the student who accepts as model types of disease and therapy what he observes in such institutions, may underrate the prevalence of sthenic maladies and the efficacy of antiphlogistic measures. Under the best practicable, mental, hygienic, medical, surgical, and obstetrical treatment, where from five hundred to one thousand sick persons are crowded into one building situated among filthy streets, the chance of being cured is small compared with private practice in urban, and still more in rural situations. Should the

popular mind ever arrive at a thorough appreciation of this fact, a revolution will probably take place in regard to the increase of large charity hospitals, notwithstanding their advantages as schools of instruction and their economy in diminishing expenses. A growing belief, whether ill-founded or not, that most diseases of a febrile character are contagious, is a prelude to the expediency of the isolating of the sick.

Art. IV.—*Researches into the Natural History of Cholera:* By Bennet Dowler, M. D. (Continued from p. 394.)

The obscurity which reigns in the pathological anatomy of cholera as tested by the ordinary standards of a limited number of maladies, renders it a study of unusual importance, seeing that it represents a class of diseases having the most intense symptomatic morbidity, apparently without leaving in the dead body a corresponding amount of disorganization. It is not intended to enter formally into the pathological anatomy of this disease, which would be incompatible with the original purpose of these fragmentary and brief papers scattered over the pages of this Journal at long intervals. A faithful account of the facts observed and recorded during every epidemic from the first one in this continent (in 1833), to the present time, may, it is hoped, interest some readers who have not witnessed an epidemic, and even others who have, seeing that the analytic investigation and the true interpretation of recorded facts are of the utmost importance.

Before proceeding to post-mortem histories, it may be allowable to offer a few general remarks. From the universality, rapidity, and brevity, with which cholera attacks and pervades the organism and its fundamental functions, strongly marked lesions and local disorganizations could scarcely be expected even in the mortal cases. Death from apoplexy and croup for example, may be equally or more sudden, but their immediate seats are more limited, and their lesions more obvious. Cholera attacks almost the entire organism, solid and fluid, arrests the neutrive action and absorption, deranging the ex-

cretions and secretions, the capillary and the general circulation, innervation, muscular contractility, calorification, respiration, vital chemistry, and the functional dynamics of the whole economy. While the involuntary muscles concerned in the respiratory and circulatory systems are deranged or nearly paralyzed, those of the limbs and abdomen are spasmodically excited. The physiological equilibrium or health-status is disturbed at all points at once, including the physical, chemical, and vital forces. It is easy to imagine, what indeed seems to take place, that even a slight disturbance in the dynamics of the vital chemistry of the body, an inequilibrium in the forces of attraction among the compounds necessary to health, or an increased affinity among one set, a diminished affinity among another set of elements, might be expected to lead to recompositions and decompositions incompatible with life. The pathological import of these chemico-dynamical alterations may not be, at present, fully known, nor appreciable with any exactitude, but they exist most evidently, and will probably be detected and explained with a reasonable degree of patient investigation. Diminished calorification alone, must according to all known analogy, cause great changes. The merely physical change witnessed in the shrinkage of the limbs, fingers, and general surface during the progress of algid cholera is a phenomenon of peculiar significance, not to say most extraordinary in symptomotology, being without a parallelism even in the deathward progress of congelation. This change is unique, inasmuch as it is accompanied with the subjective sensation of heat, while the objective condition of the body including the breath and tongue, as tested by the thermometer, is from 20° to 30° below the normal standard, as may be seen in numerous cases which I have published in this journal. In the present state of our knowledge or rather ignorance of the *vera causa* of cholera, it matters little whether algidity be the absolute cause or the conditioned effect of this malady.

In cholera several morbid allerations must be studied before death. Thus the lesion of the entire skin—(if it be allowable so to speak), the lesion of color, the lesion of non-vascularity or non-capillary circulation, the lesion of temperature, the lesion of structure—I say lesion of structure, for if a similar withering, sodden corrugation or contraction were to take place in the brain, spinal cord, splanchnic nerves, cavas, abdominal aorta, pleuræ peritoneum, arachnoid, heart, bowels, etc., would

it not be called a real lesion? Can anything be more unreasonable than to reject all lesions of structure occurring before death and not discoverable after, especially where these are most open to inspection? Unfortunately some organs are so remote from view, so little within the reach of examination, that their actual condition during life can only be subsequently assumed from post-mortem appearances. Suppose that a spasm of the aorta, heart, small intestine, glottis, or stomach were to prove fatal, but after death relax and assume its natural appearance, the lesion might elude observation; but if had been all the while open to the view it would be a folly to doubt for the time being that it was a real lesion because it had disappeared without leaving rupture, ulcer, or gangrene. Why is morbid anatomy to be confined to the centre? Is not the surface equally important? Moreover, by what law of pathology is morbid anatomy to be based on inspections two or three days, as is usual, after death? Why should changes obvious to all, be disregarded, be rejected before death. The lesions in cholera begin with the disease. The lesion of secretion, called ricewater-discharge, is as much a lesion as hæmorrhage from the lungs or the coughing up of pus; as much as apoplexy, hydrocephalus, or abdominal dropsy, etc. Each external vein that is collapsed, each nonpulsating artery, each portion of the skin, each cramped muscle, each drop of blood, offers a lesion to those who have eyes to see. The very term *morbid* anatomy is perverted by neglecting to include the changes before death, especially in those cases of alteration, which are open to view, and which are liable to modification by the agony, by the complete extinction of vitality, by chemical agencies, and by post-mortem alterations. Hence the lesions in cholera are more general than local. The secretions and excretions, the fluids and the solids, the temperature and the blood, the tissues, organs, and the functions of the centre and circumference of the body are very extensively affected, yet uniform alterations limited to special seats and having definite, salient anatomical characters, are comparatively wanting, or rather the appreciation and knowledge of these remain to be discovered, classified and fixed upon a solid basis.

The cold sodden, shrunken, and contracted state of the comparatively firm tissues of the entire circumference of the body, together with the collapse of the blood vessels and the almost entire loss of the superficial circulation, must, from the nature of the case, cause in

the spongy vascular viscera, central congestion even though none whatever be discoverable after death. It is reasonable to conclude that this congestion alone, no matter how induced, is sufficient to cause, or at least to contribute to the fatal result. That coldness, shrinkage, and congestion are greater before than after death, I have shown by numerous observations. After death the body often becomes warm, and plump; the capillary circulation fills up the external veins; the blueness of the skin sometimes is replaced with a dusky red, and still oftener the color becomes more natural than at the close of life; the clammy sweat or exudation from the skin also disappears. Hence cholera, to a certain extent, reverses the ordinary march of pathological anatomy, the living lesions overtopping the post-mortem.

The physical, chemical, and physiological effect resulting from the enormous abstraction of animal heat occurring in cholera, cannot fail to be an element of paramount importance, in what point of view soever it may be regarded. As a merely mechanical agent, the increase or diminution of heat must influence the gasses, liquids, and solids of the body, expanding or condensing the entire mass and changing its external relations to the atmospheric pressure and other agents. A remarkable example of the absence of the healthful tonicity chiefly dependent on the natural heat, is seen in the hard horny surfaces of the palmer aspect of laborers' fingers, which, instead of being round and plump, become concave or cupped, as if indented by a positive exterior force.

The lungs, as formerly stated, may be regarded as organs of depuration in which carbon is excreted, and a new aliment, so to speak, is taken in and digested, that is, respiration at the same time decarbonizes and oxidates the blood. Without inquiring into the cause of the deranged respiration in choleriques, the fact is indisputable, namely, that this function is performed in an imperfect manner. Whether the inspirations be quick or slow, the air returns in expiration almost as cold as it entered the trachea, being sometimes apparently colder. The chest has little motion or expansion. The breathing instead of fully distending the lungs seems confined chiefly to the air passages. These organs like the skin are probably collapsed, as they are often found to be, upon dissection. It may be possible that the first link in the ætiology of cholera begins in the lungs and in imperfect respi-

ration, imperfect oxydation of the blood, an imperfect absorption of oxygen gas, an imperfect elimination of the carbonic acid poison. Now it appears to be generally conceded that the essential cause of cholera is an aërial or gaseous agent ; whether that agent originate in the Indian or Arctic ocean, in fermenting swamps or amid arid sands ; being atmospheric, its first point of contact would probably be in the lungs, and through them, the intestinal and cutaneous capillaries, and the whole circulation. Whatever doubt may hang over the primordial or essential cause of cholera, there can be none as to its marked secondary effects upon the blood which becomes progressively poisoned by carbonic acid. The cyanosed discoloration of the skin is like that produced from execution by hanging, organic heart disease, etc., though sometimes more intense, whence the characteristic name, cholera asphyxia. As misfortunes seldom come singly, but in a crowd, so it happens in this case. Copious exhausting discharges from the bowels by which the blood is deprived of its serum, alter its normal proportions, rendering it unsuited for transmission through the capillaries of the lungs, thereby cutting off the means of its aëration or purification from the poisonous carbonic acid, which, in health, the lungs eliminate constantly.

There is in many cases a marked difference between the blood in the capillaries and that of the large vessels of choleriques. The blood in the former acquires after a few minutes exposure to the air, a roseate hue as seen in the muscles, membranes, etc. The blood taken from the large vessels sometimes remains black as at first, for one and two days—perhaps much longer.

The following experiment though an accidental one, is not for that reason the less instructive. In performing a post-mortem blood-letting from the jugular of a cholerique, the blood was received on a large delf plate, one portion of which contained a stratum of powdered lime adhering to its concave surface ; that part of the blood which fell upon the stratum of lime became of a bright scarlet color, while the residue was dark, and so continued as long as observed, that is 24 hours. Here the lime appears to have parted the carbon from the blood, so that the latter assumed an intense arterial hue.*

The limits to which this article must be restricted forbid, at pres-

* Prof. Liebig says that a man expires or discharges from his lungs 13.9 ounces of carbon, and consumes 37oz. of oxygen, in 24 hours. (*Anim. Chem.*)

ent, any further discussion upon generalities. Post-mortem examinations will, therefore, be resumed at once.

Jan., 1849 ; Noon ; air 56°. Mrs. M. M., aged twenty-six, resident one month, was taken with cholera twenty-eight hours before death. In the meantime, her youngest child, still at the breast, aged eighteen months, sickened and died of the same disease. At the present moment, her only remaining child, a boy aged about three years, is pulseless, cold, and dying of cholera. [Died next day.] The mother who was stout, vigorous, rather fat, suffered severely from vomiting, purging, cramps, coldness, etc.; her senses being natural except a little wandering during the night.

She was treated with quinine, opium, sugar of lead, and three blisters, one on each arm, and a large one on the abdomen. The blisters had removed the cuticle, but the skin was not reddened thereby.

Dissection one hour after death : body flexible ; muscular contractility active ; face and hands blue and mottled. Hands, etc., at first shrivelled, gradually became plump.

The subconjunctival tissue corresponding to the tunica albuginea presented a beautiful crescentic coloration, its convexity towards the malar-bone, where was observed a purplish red ecchymosed patch somewhat like a bruise called black and blue ; muscles natural, adipose tissue abundant.

The left mamma, from which alone she had nourished her child, was enormously distended with natural colored milk ; on pressing the breast streams jetted freely ; on making an incision into the highest portion of this gland, the milk spontaneously ran out, nearly streaming, for several minutes ; the whole organ was gorged with milk. There was not a drop of blood discharged from the incision, but much milk of a natural appearance in every respect. This gland three hours after death, gave 85½°, the entire body having been all the time exposed without cover to the air at 56°. The axilla for an hour after death continued to rise, reaching 100°; vagina in an hour and a half 102°—in two hours 101¾°, at which time the epigastrium, liver, and other central regions of the abdomen were 100° each ; the left chest 95° ; three hours after death pelvic region 97°, axilla 91°, centre of the thigh 97°, centre of the calf of the leg 77°, centre of the left mamma 85½°, the upper third of the arm in the centre 85½°, left chest near the spine 86¼°, the heart 96°, epigastrium 93°, vagina 99°.

No serosity, but much venous vascularity of the brain ; fauces, tongue, larynx, pharynx, trachea, salivary and bronchial glands natural ; thyroid red, slightly tumid. The serous membrane of the chest and abdomen lined with a greasy, yet adhesive exudation, rendering it difficult to hold a knife soiled with it ; no serosity in the chest, abdomen or pericardium ; the whole venous system from the right side of the heart, including the coronory veins, the pelmonary arteries, the portal, mesenteric, omental, pelvic veins, the cavas, etc., enormously engorged with blood which was thick, greasy, adhesive, dark ; the right side of the heart and cavas contained soft clots ; these vessels discharged about four pounds. Stomach empty and contracted to two-finger's width at the pyloric and three at the cardiac end.

Serosity with mucus was found chiefly in the jejunum with a few streaks of black vomit ; also many red patches in the ileum, which was most vividly injected. Thickish rice-water fluid in the large intestine ; portions of the latter contracted.

There was a peculiarity, probably congenital, in the descending colon, one portion of which was doubled back on the other, being adherent externally for three inches, forming an acute angle, and a narrowing of the calibre. No feculent matter or odor.

The uterus flaccid, natural in size, bluish red ; a tenacious cord of mucus or fibrinous matter passed out of the os tincæ into the vagina three inches long ; the uterine cavity lined with an adherent stratum of dark red blood ; ovaries gorged ; bladder contracted and empty ; kidneys, spleen, pancreas, ganglionic nerves, natural ; liver tumid, purplish, gorged ; the gall bladder contained about 2ʒ of bile very dark in mass, but yellow when diluted or diffused.

The blood in this cadaver was dark, forming an adhesive non-elastic clot with little serosity. The muscles and adipose tissues though described as natural scarcely came up to that standard, both were a little darkish. The vagina and lower end of the rectum were of a dark red, almost blue. It is worthy of remark that the secretion of milk had been active. The left breast was eight or ten times larger than the right, and was excessively distended with fluid milk. The ovaries contained a number of corpora lutea or whitish yellow sacks from the size of a pea to double that size. These were not always globular, but some were oblong and contained limpid serosity. The skin of the lower abdomen was extensively marked with the scar-like white stripes usually the indicants of pregnancies.

The curious punctiform crescentic injection of the subserous tissue of the eyes above mentioned, has probably sometimes escaped my attention in other examinations. I find the following note upon this delicate and neglected, if not novel appearance: Jan. 6, 1848: I observed to-day four choleriques dead ten hours on an average. All had a crescent-shaped, punctiform subserous injection or discoloration concealed by the under eyelid unless the latter be depressed so as to bring it into view.

Dec. 20, 1848; air of the room 79° to 81°. E. B. C., born in Kentucky, aged 34; had the usual symptoms of cholera, as vomiting, purging and severe cramps; was cold, blue shrivelled, collapsed, etc. Treated with opiates, paregoric, etc. Half an hour after death; limbs and body flexible; mottled and blue about the face; eyes injected; abdomen level; contractility powerful, for two hours lifting repeatedly a hammer, (1½ lbs.) tied in palm, carrying it to the breast. Post-mortem rigidity quickly set in after the last flexion of the arm, forming at the part percussed a large solid mass fixing the fibres at the maximum of their contraction.

Axilla 3 m. 97°; 2 m. 99°; rectum in 15 m. 108°; in 3 hours after 106°, The body became plumper, the skin more natural. Muscles natural; a little paler than health. Fat, dryish or sticky.

BRAIN, though elevated, bled two to three ounces; serosity as much in the arachnoidal sack and ventricles, with increased vascularity of the pia mater; substance of the brain healthy; arachnoid milky, increased in tenacity; adherent upon the hemispheres to the arachoid of the dura mater.

Neck, fauces, tongue, gullet, salivary, lymphatic glands, trachea, somewhat pale.

CHEST.—The right side of the heart and pulmonary arteries and all the great veins congested with blood; on removing the heart about five pounds of black adhesive blood accumulated in a few minutes in the chest, clotting slowly, forming a peculiar kind of clot, not brittle as is usual, but adhesive or sticky. The lungs much collapsed and bloodless; liquor pericardii abundant.

ABDOMEN.—The stomach contained nearly a pound of black vomit; its mucous tissue thickened or tumefied; intensely injected or rather of dark red, the arborizations not strongly marked. The same appearances prevailed in the lower part of the ileum. The intestines

contained whitish and yellow serosity, the large intestine considerably contracted ; the omenta, mesentery, and subserous tissues loaded with blood ; the serous membrane had glutinous matter not very perceptible to the eye, but adhering to the hands. The liver somewhat enlarged mottled with a cork color ; bile about ℥iss, ropy ; common duct pervious. Spleen joined to the omenta and other adjacent organs by strong old adhesions ; round shaped ; of moderate size ; its serous coat white, cartilaginous and thickened ; within this, occupying one-third of the organ, was a white, firm, cartilaginous sack, one-sixth of an inch thick, containing a semi-solid matter like the cineritious part of the brain, though firmer ; not being vascular. This was different from ordinary tubercular matter ; no tubercles in other organs. Bladder empty, contracted, natural ; kidneys, mesenteric glands, pancreas, etc., healthy.

Dec. 20, 1848; 11, A. M.; air 79° to 80°. J. R., aged twenty-five, born in Ireland; had yesterday all the symptoms of confirmed cholera, vomiting, purging, cramps, coldness, shrivelling, etc.

Dissection one hour after death ; face, legs and neck mottled, blue ; eyes intensely injected and ecchymosed; corneæ dry. The veins in an hour and a half after death, filled, particularly the jugulars ; axilla 5 m. 96½°, 5 m. 97½° ; rectum 10 m. 101½° ; 10 m. 102¼° ; abdomen level ; muscles natural ; neck rigid ; contractility moderate for an hour.

Brain natural, serosity about one ounce ; pia mater vascular. Trachea and chest natural except an old adhesion of the left lung. No liquor pericardii ; right side of the heart and pulmonary arteries greatly distended with blood. The blood was adhesive, several pounds of which accumulated in the cavities during the dissection. Neck, fauces, glands, trachea, gullet, natural.

Stomach pale ; contained black vomit ; the small intestines contained much serosity, whitish, variegated with long flaky threads and masses of black vomit. The liver somewhat enlarged, pale on a part of its convex surface ; bile dark yellow ; flowing through the duct on pressure. Spleen enlarged five or six times, blue externally ; bladder empty and contracted, colon contracted ; the lower portion of the ileum injected in its mucous coat. Other organs natural.

Jan. 10; M. A., aged 33, resident one month; diarrhœa yesterday; vomiting, purging, and cramps, to-day.

Jan. 11; noon. Discharges and cramps have ceased; severe pain in the abdomen, which pressure increases particularly in the vicinity of the stomach and liver; features dark, sharp, sad, and cadaverous; skin a darkish blue, collapsed, veinless, pulseless; abdomen retracted; tongue moist, pale, and slightly tumid; thirst inconsiderable; respiration imperfect, noiseless, twenty-six; little urine; palms 69°; bend of the arm 75°; axilla 91°; calf 77°; popliteal 81½°; tongue 76½°. Takes brandy and s. morph.

Died, at 7, P. M.; exposed to about 50°, in the dead-house during the night.

DISSECTION fifteen hours after death: rigidity; muscular and adipose tissues well developed and natural; skin blueish; eyes injected; corneæ dry and glassy.

Brain, natural.

Spinal cord: The theca contained about 1½ oz. of serosity; the substance of the cord slightly injected; exterior to the theca a slight hæmorrhagic infiltration of the tissues. (Perhaps this latter appearance is cadaveric.)

The lymphatic glands of the neck, chest, and abdomen considerably hypertrophied; some red, others pale.

The organs of the mouth and neck natural, including the salivary glands.

Lungs natural though discolored with black blood; liquor pericardii abundant; heart distended on the right side with black, soft clots; both sides of the heart contained also, grayish fibrinous polypi. Serous membranes sticky to the touch.

Solar plexus and ganglionic system natural; kidneys, pancreas, ureters, gall-bladder and ducts, bile and omenta, natural. The urinary bladder contracted contained a teaspoonful of liquid slightly acid; all the medical observers, at least half a dozen, agreed that it was exactly like the rice water of choleraïc stools. The spleen healthy in texture but atrophied to one fourth the usual size.

The mucous membrane of more than half of the stomach softened, particularly along the greater curvature, increasing towards the cardia, with punctiform injection mottled with white, yellow, and roseate hues. This organ was coated with a dense, yellowish mucosity amounting to several ounces, very adherent, almost like a false membrane; the pyloric end of the stomach, rugous and slate colored, the

duodenum contained thick mucosity, bile, and a milky fluid ; the Brunnerian glands near the pyloric end of the duodenum were hypertrophied.

Rice water or milky liquid, reddening litmus, prevailed in great abundance throughout the small intestine, with a slight tinge of yellow in places. The large intestine contained about twenty-five ounces of blood and mucus as thick as molasses, of a dark reddish chocolate hue, and thoroughly mixed. The ileum, little injected was generally very diaphinous, but everywhere studded with the solitary glands (glandulæ solitariæ), salient, firm, white; many hundreds of these even nearly as large as grains of barley; the Peyerian glands, (glandulæ agminatæ) were hypertrophied chiefly in the middle portion of the small intestine, the clusters varying from a few lines to five inches in length, and from three to six lines in width, elevated, granular, firm, yet brittle, and but little injected, sometimes roseate and sometimes pale and anæmic. The jejunum and the upper part of the ileum had isolated spots of punctiform injection or red dottings in the mucous tissue, the valvulæ conniventes, the villosities, follicles. In the large intestine the mucous coat has abundance of dark red dottings, arborizations, sanguineous gelatiniform infiltrations of venous blood, with thickening and softening; also hypertrophy of the solitary glands, and universal vascularity. The mesenteric glands, hypertrophied, mostly pale; a few red; estimated at two hundred in mass; some of these were larger than a peach kernel. The cava and its branches enlarged and distended with black adhesive blood, much being clotted. Other organs natural.

These few histories or copied notes made on the spot to use a striking idiom, are not offered to the reader as select or extraordinary, which would be a vicious mode of scientific research. Nevertheless, it so happens on reviewing them, that they appear to me somewhat unique in relation to the appearance of black-vomit in the digestive apparatus of choleraïc cadavera. Although it is not intended to offer remarks upon the pathological import of the lesions disclosed in these autopsies, it may be proper to inform the reader that the years 1848–49 brought with them many cases of yellow fever in New Orleans ; indeed the latter was an epidemic year. Hence it is reasonable to conclude that the cause of yellow fever, whatsoever that may be, blended itself with that of cholera, so far at

least as to produce the black vomit matter above described. I cannot say, however, whether the entire series of post-mortem histories not published, for these years, will or will not afford similar results.

I may here further remark without going into a lengthened argument, that the assertion which has been too confidently made by some celebrated pathological anatomists that all diseases mentioned in the Nosology, have special, determinate, and invariable anatomical lesions, must not, without further proof, be taken for granted. It does not follow that the thousand *names* in the artificial classifications of diseases represent an equal number of fundamental differences in their nature, whether symptomatic or anatomical. Their natural unities not less than their differentiæ must be borne in mind. So far as human research and reason can determine, their phenomenal history shows that they often either blend or possess much in common. At least the diversity of *name* does not cause diversity in the nature of things. The thing does not accommodate itself to the name. Thus exclusive of ætiological and anatomical bases, De Sauvages founded on mere symptomotology, ten classes, forty orders, three hundred genera, "and an almost innumerable host of species." Vogel, however, in his improvement on this system, nearly doubled the genera, etc. Pathological anatomy cannot anchor in such a bottomless maelstrom.

Pathological anatomy cannot as yet accept M. Cruveilhier's definition, though fortified by his great name as a worker and thinker: AXIÔME FONDAMENTAL EN ANATOMIE PATHOLOGIQUE, SAVOIR: *que toute altération organique qu'on ne rencontre pas constamment à la suite d'une maladie, ne peut pas être considérée comme faisant partie esentielle de cette maladie.* The difference between an uniform alteration and none is immense.

Dr. Watson in his valuable work on the Practice of Physic says, in his lectures on Cholera: "The examination of the dead bodies threw *no light, that I know of*, upon the nature of this frightful disease." Let the compilers of every book on Practice, examine fifty or one hundred cadavera, victims of cholera, devoting four hours to, and making a special history of, each, and then let him review the whole, in order to see whether there is *one* in which "*no light* is thrown upon the nature of this frightful disease." Less dogmatism and more dissection! Less resignation, that is less despair and more labor, more logic. Of the five modern systems of Practice commencing with Hos-

ack's Thomas, which can claim as a basis, even a dozen years' personal experiences in the dead house? Matured years cut out of the middle-life time? Years enlightened by previous blunders as well as by mature, and extensive reasoned experimentalism? The *vis inertia* of systems is for the laborious reader and investigator, counteracted by medical journalism, and still more effectually by personal researches.

ART. V.—*Perforation and Rupture of the Stomach and Œsophagus in Yellow Fever, (including the concurrent Post-Mortem Appearances in the Cadavera):* By BENNET DOWLER, M. D.

IN the xiii volume of this Journal, 614, I have given the history of a case of *Spontaneous Ruptare of the Stomach*, causing speedy death, in a mulatto (a foundry workman) just after taking a hearty breakfast. The body was disinterred. I made the post-mortem examination before the Coroner and jury of inquest, and found a rupture of the stomach, four inches in extent. The stomach contained about half a pint of semi-fluid diet, about the same quantity having been effused into the peritoneal cavity on the left side of the spine, near the diaphragm, resting in clot of blood.

In this case the evidence in favor of preëxisting disease was wanting. The man had just eaten a hearty breakfast, of the usual food, including a good deal of coffee. If antecedent disease had existed so as to have altered the normal cohesion of the stomach, distension, especially in connection with a violent contraction of the muscular tissue of the stomach and abdomen, would afford an explanation. Violent and sudden contractions, it is well known, will rupture muscular organs, independently of preëxisting disease. Distension is a condition favoring rupture of the uterus, bladder, heart, etc.

The ætiology and diagnosis of rupture and perforation of the digestive apparatus, are often obscure. These lesions, rarely spontaneous, are probably, for the most part, secondary results, preceded by softening and ulceration, acute and chronic. Their marches vary in duration, being sometimes almost immediately fatal; sometimes death is delayed until fæcal and other effusions cause peritoneal imflammation.

Without dwelling on the importance of these lesions, and their relations to morbid changes caused by certain acrid poisons, often of grave import in medico-legal investigations, it is proposed to devote this paper to autopsic evidences of the perforation and rupture of the œsophagus and stomach, which I have found in yellow fever cadavera.

These lesions are not presented as characteristic of, or peculiar to, yellow fever subjects. I am not aware that they have been ever mentioned as occurring in this disease ; nor can I now give the ratio of their occurrence, not having leisure to examine only a small part (134) of my post-mortem histories, from which these cases are taken.

J. C., Frenchman, aged 25, last from France; resident six months, sickness five days.

Towards the close of life, he became delirious, raved, screamed; was tied in bed; ejected black vomit. Expired August 31st, 1843.

DISSECTION, two hours after death. Body hot; limbs rigid; fingers flexed; the muscles natural in color, large, knotted in the limbs, as if drawn up into lumps by cramps; skin, eyes, and fat, yellow; abdomen depressed.

HEAD —From the highest, not less than the lowest portions of the scalp, blood flowed for two hours, clotting well, and amounting to two or three lbs.; the arachnoid highly tenacious; pia mater a net-work of turgescent vessels, its meshes infiltrated with blood; injection of the cerebral substance. Two considerable clots in the posterior horns of the lateral ventricles. Brain firm.

MOUTH.—Amagdylæ red.

NECK.—Gullet had the lower third of its epithelium removed, its mucous tissue red, raw, and near the cardiac, black, where, also, a perforation of the tube was found, from which about twelve ounces of black vomit had been discharged, partly into the left pleural sack, partly into the mediastinal tissue, and, into the external œsophageal and trachial cellular tissues.

CHEST.—Mucous tissues red. A few ecchymosed dottings under the linings of the right auricle of the heart.

ABDOMEN.—The stomach contained some black vomit, and near the œsophageal perforation its mucous coat somewhat reddened, tumefied, softened. The jejunum contained both chylous matter and intensely colored black vomit, which latter was adherent to the intestinal surface. The ileum considerably contracted, had five or six elliptical

Peyerian plates, about one inch wide and three long, with several circular, smaller clusters. The patches consisted of separate pustular or papillary elevations obliterating the intestinal folds, *(valvulæ conniventes)*, with more or less tumefaction of the mucous tissue, but no ulcerations. The dessiminated glands of the large intestine were more or less tumefied. The mesenteric glands, spleen, kidneys, pancreas, bladder, peritoneum, etc., natural. No appearances of fæcation. Liver of an uniform pale yellow ; its parenchyma somewhat brittle and bloodless; its great vessels apparently enlarged; its gall bladder which was collapsed, contained a few drops of green liquid. Other organs natural.

D. T., born in Vermont, aged twenty-five, last from Boston, resident eight months; sick two weeks, at first with intermittent, as was supposed.

July 30th, 1843; delirious; difficulty in swallowing; no vomiting; ol. ric. cups, foot-bath, and enemata ordered.

Aug. 3d, 9, P. M. died. Learn that for two days before death he could not swallow.

DISSECTION, twelve hours after death:

BODY, above the ordinary size; adipose tissue moderately developed; muscles natural; skin and eyes yellow; abdomen distended.

HEAD.—(Opened last.) Serosity four ounces; increased tenacity of the arachnoid and pia mater; vascular turgescency of the latter; cerebral substance natural.

CHEST.—The mucous tissue of the trachea and left bronchia, pink red; left lung loaded with blood, dense, indurated, non-crepitant, augmented three or four times in weight, of a dark red color, with some scarlet spots, (red hepatization); the pleuræ thickened, having febrinous, semi-organized shreds, adherent; the parenchyma had less cohesion than is usual in hepatization; the base of the lung sank rapidly in water; these lesions augumented from the apex to the base. The right lung natural.

The heart much distended with blood on the right side, its ventricle, auricle, and adjoining cavas were of a deep pink color, and somewhat tumefied. Maceration for one hour did not discharge the color. The left side of the diaphragm, especially the left crura, much injected. Black vomit and oil effused into the chest.

ABDOMEN.—The cardiac portion of the stomach ruptured as large

as a dollar; an extensive surface, around the ragged orific, had lost all the coats except the white tendinous-like serous coat, which was denuded; the residue of the stomach, free from thickening, vascularity and redness. Through the perforation, (which included the lower crural portion of the diaphragm), black vomit, mixed with oil was freely effused in the pleural cavity, along the spine.

The intestines, in some portions, contracted, contained both black and white fluids. The large intestine contained masses of almost perfectly white, and nearly solid fæces. The liver yellowish. The urinary bladder enormously distended. Other organs natural.

T. L., born in France, last from Havre, a sailor, resident in New Orleans one month, sick with yellow fever four days. Aug. 24th, 1842; is stupid, though very sensible to pain along the spine. When he is moved he cries aloud. He was not observed to throw up black vomit. He became delirious, and died at 9½ o'clock, P. M., Aug. 26th, after an illness of six days.

DISSECTION, nineteen hours after death:

BODY, cold; skin, eyes and fat yellow; abdomen, concave; muscular; of medium size; muscles, natural.

HEAD, not examined.

CHEST.—An accumulation of black vomit, in the left cavity of the chest; an ulceration of the diaphragm, (mentioned below).

ABDOMEN.—The stomach was ruptured, several inches in extent, next the cardia, the parts ulcerated. The diaphragm ulcerated, and more or less torn in appearance, the opening being irregular, the tendinous expansions shreddy and denuded of their muscles. The rupture was at the left crus of the diaphragm, near the spine, extending upwards towards the cartilages of the ribs. About four ounces of intensely colored black vomit, had entered the left plural cavity of the chest; a like quantity was lying behind the stomach in the abdomen, on the same side of the spine. The stomach and diaphragm, though, not adherent at the time of dissection, had probably been so previously, as indicated by their opposing surfaces which were raw, ragged, red, and covered with glutinous exudations. The mucous tissue of the stomach, generally of a dirty, pale hue, which near the rent, gradually became redder and redder, until it assumed a peach blossom color; its texture thinned, softened, pulpy; and, near the rupture or perforation, was shreddy like rotten tow, or stringy matter in ill-condi-

tioned abscesses, extending over a space as large as the hand. This spot was traversed by many naked blood-vessels, forming beautiful arborizations, the extreme branches of which, terminated in minute leaflets, of a color, generally, like black vomit, some being red. Here, the mucous tissue was gone, and beside the great rent, many small perforations of all the coats, were descernible. The remaining coats were attenuated generally, with great loss of tenacity, especially at, and near the perforations. The duodenum red, its coats, brittle. The small intestines, in their upper half, contained a chylous or white paste, the lower half abundance of healty fæces, moderately firm; the large intestine contained enormous quantities of very solid though natural fæces. All the intestinal coats, natural. The liver enlarged, (one third,) was firm, and of a deep, reddish brown; gall-bladder, contained a bloody serosity. Other organs natural.

ART. VI.—*Lecture on the Pulse*:* By the late PROFESSOR CHAPMAN, of Philadelphia; from the manuscript report of the late Dr. A. HESTER.

THE pulse had long been considered as affording an indication of disease, and as justly entitled to our attention, and demanding our fulest confidence, but is evident, it could not afford a just diagnosis or prognosis prior to the discovery of the circulation. Hence it was cursorily noticed by Hippocrates, who meant little more by it than the throbbing of an inflamed part. By Celsus, who flourished some centuries afterwards, some advance was made towards a more correct knowledge of the pulse, though so little was known, with respect to it, that he denominated it *fallacissima.*

To Galen, the credit is due of having impressed on practitioners, the necessity of attending to this indication, and to him we owe the first correct instruction. His treaties on the pulse, is voluminous, and contains all the minute divisions, which, even to the present day, is familiar to us.

But this very minuteness of division, is what constitutes the chief defect of his work, and it seems rather to embarrass, and perplex,

* Delivered in the University of Pennsylvania, Nov. 1835.

than to guide us in our investigations. As may be supposed this subject has not been neglected in modern times. It has engaged the attention of the ablest men, but as usual in matters of importance, there have been no small differences.

Some writers maintain, that the pulse is the only safe criterion of the condition, and fluctuation of disease. Others are disposed to predicate its utility, as being an auxiliary or subservient, and should never be trusted to the exclusion of other numerous signs. The truth, may, perhaps, lie *in media*.

I shall first treat of the natural pulse: In a healthy condition the pulse is soft, open and vigorous; it is free from all sense of resistance, and the return of the strokes are at equal intervals. At birth, conformably to the best calculations, it beats at first one hundred and forty in a minute; at the end of the first year one hundred and twenty; at the expiration of the second year, one hundred; at the third year it averages ninety. From this period it becomes slower, until the age of twelve years, when it assumes the adult standard of about seventy-five strokes in a minute. After the meridian of life it is somewhat slower, and at old age, still less frequent, being not much more than forty. It is then also, fuller and stronger; being somewhat intermittent occasionally. These two last changes should be borne in mind, in practice, because in young men with such a pulse, you should *bleed freely*. If we do not bear in mind, that it is thus irregular and intermittent, we shall often experience unnecessary solicitude.

2dly. The second cause which influences the *pulse* is *sex*. The pulse of women is quicker than that of men, and the difference by some has been computed to amount to ten strokes a minute. The greater mobility and irritability of the female organization, would readily convince us that such might be the case. But from my own experience, I should decide that the difference was not so great.

It is worthy of recollection, that certain states of the female, have great influence in the pulse. Thus we always find it more active and full during *pregnancy*, than in an opposite state of the system, and it is also much in the same condition, just before the period of menstruation.

3dly. The pulse is varied according to the temperament. It is more active and quick in the sanguineous, than in the cold and phlegmatic. Persons of a sanguineous temperament, have the same sort of mobility, that is incident to females.

4thly. It is influenced by *stature.* This has been ascertained by actual experiment. By B. Robinson, it is said that the pulse in a man of six feet high, is ten strokes slower in a minute, than in one of ordinary height. In a dwarf the pulse is from ninety to one hundred in a minute, when in giants, it ranges from fifty to sixty.

5thly. The pulse is modified by the position of the body. It is quicker, when in an erect posture; much slower in a recumbent posture of the body, and intermediate, between the two, when sitting. In a man lying on his back, it is sixty-three or sixty-four; sitting sixty-eight, and standing seventy-five, in a minute. This is stated on authority of Robinson. If we were not aware of these circumstances, we should be often liable to be deceived in positions of the body.

6thly. The pulse is not a little influenced by sleep; this abstracts much of the stimuli, which act upon, and excite the body and mind. This difference amounts to several throbs in a minute. The fact is denied by some, and by Haller, among the rest. It is now, however, admitted when the pulse is quicker in sleep, that we must attribute it to the chamber or bed clothes, or to the mind of the patient disturbed by unpleasant dreams, etc.

7thly. Darkness, and light have great influence on the pulse, which is slower in dark, and more vigorous in the light. When we wish to subdue vigor of the circulation, we should keep the shutters closed. The circulation is affected much by motion. The pulse of a person in walking four miles in haste, is augmented in velocity to one hundred and thirty, and on running still more.

8thly. Diet also influences the pulse; much depends on the nature of the food and drink. Animal food increases it much more than vegetable food, and wine more than aliment, and ardent spirits most of all.

9thly. The pulse is influenced by medicine. Cold depresses the pulse; but much depends on the application; dashing on cold water raises it, etc.

10thly. The condition of the mind influences the pulse. It is greatly excited by intense thought, and solicitude of the passions. Anger, joy, hope, accelerate it, and increase its force and fulness. Fear, and grief have an opposite effect. Hence it is important to observe silence in a sick room, etc.

11th. The time of day exerts an influence. Cullen says it is more

frequent at noon, and in the evening, similar in this respect to *hectic* fever. A more general opinion is that it is slower in the morning, increases a little towards the middle of the day, then diminishes in frequency, and in the evening is again augmented. It has been asserted on the contrary, that the pulse gradually quickens in the morning, and gradually diminishes towards night. This circumstance is ascribed to the diurnal motion of the body, in consequence of which every function is less perfectly performed at the close of the day--particularly those of respiration, digestion, and the circulation. Beside these, according to chemistry, there is less air respired at night, than in the morning, which is proved by the fact that less carbonic acid gas is produced. Cullen asserts that digestion is but imperfectly carried on at night, which he proved by the fact that all persons suffer more from supper than any other meal. The trainers and pugilists, in England, always deny supper to persons under their direction, as they mostly become debilitated, rather than invigorated by it.

THE MORBID PULSE.

We are now to consider the pulse in a morbid condition. You well know that the vascular system is a whole, or unit made up of parts, chiefly connected by sympathy, as it extends over the whole body, penetrating the most secret recesses of the organized structure. It is exposed to every morbid impression both internal and external; hence it participates with diseased action in the vessels, or in other tissues of the body, and communicates to us the information we desire from examination of the pulse. The blood vessels thus impressed in most diseases, the pulse is altered by every febrile action, and so much in some cases as to be wholly different from what it is in a healthy action. Some maintain that there are two hundred pulses, but the distinctions are so slight and difficult to recollect that an enumeration of all, would serve rather to embarrass, than to guide us in the treatment, and far more in delivering our diagnosis. The same objection may be made against the division of diseases, as against the differences of the pulse, namely, that it cannot be communicated in a lecture, but must be acquired by great diligence, application, and observation.

The first kind of pulse, according to Galen, (who was copied by Dr. Rush), is the synocha. It is full, frequent, and tense, with some hardness and contraction of the volume. It occurs in the synochal

fever of Cullen, (especially rheumatism and phrensy), and in the phlegmasiæ.

The second variety is called synochus, and is full, round, vigorous and frequent. It differs from the preceding, by being soft, more open, and of larger volume. This is to be met with in diseases of moderate inflammatory action, particularly in the ordinary autumnal fevers.

The third, is the synochula; it is quite tense and sometimes vibrating, always corded to the touch, imparting the sensation of an elastic tube. It differs from the preceding in being quicker, harder, and of a smaller diameter. We find this in the protracted stage of phlegmasiæ, particularly in gout and rheumatism, and in pulmonary consumption.

The fourth, is the *typhoid* pulse. This is quick, has some degree of tenseness, and small contracted volume. It occurs in debilitated states of the system, especially when accompanied with high irritation, as evinced in hectic fevers.

The fifth, is the *typhous* which is very small, sometimes trembling, and compressible. It is found in those diseases which are distinguished by little vigor, as the jail and hospital fevers.

The preceding are the pulses most commonly found in the practice of medicine, and may be called with propriety the five primary or original pulses. But they are often blended or modified in several ways, not the least frequent to be met with is the *soap bubble.* It is voluminous, full, soft and round. This is an insidious pulse; it calls apparently for venesection, and other evacuants, when, in fact, the indications are exactly opposite. It often occurs in pestilential fevers, and is particularly observed in our winter epidemics. The loss of a quantity of blood, in such cases, sinks the pulse, immediately, and with it the vitality.

The most important variation of pulse remains to be stated. It takes place in an oppressed condition of the system, and sometimes imitates the pulse of exhaustion, so that the two may be readily confounded. It is highly important to distinguish between them, which may be done by the following condition: The oppressed pulse occurs only in the first stage of the disease. 2. It is found only in the malignant fevers, and others of great violence. 3. It is always to be met with in an inflamed condition of the alimentary canal, and some-

times in the same affection of the brain. 4. It is commonly slower than the pulse of debility, and sometimes when the oppression is great does not beat more than half the usual number of strokes, being as slow as thirty or forty in a minute.

ART. VII.—*Lectures on Fever*, by the late PROFESSOR CHAPMAN: (From the MS. report of the late DR. A. HESTER.)

[AT the time when these lectures were delivered, Prof. Chapman was at the zenith of his reputation. He had then been for a quarter of a century a popular professor in the most celebrated school in America, the University of Pennsylvania, from which he retired in 1850, three years before death.

The synopsis of a portion of his lectures, which forms this paper, is highly interesting, reflecting, as it does, his matured opinions anterior to the transition period, to which the present belongs. Broussais' Pathological works, translated by Drs. I. Hays, and R. E. Griffith, had then been several years before the American profession. Gastro-enteritic inflammation and the theory of sympathy were the pillars of Broussaisism. These were the favorite theories of Dr. Chapman's pathology and therapy, as the following paper will prove.

The succession in American Medicine was virtually Cullen, Rush, Chapman. With these dynasties systematic medicine ended. The Professors of the *theory* and practice of medicine ceased to teach rigid general theories. They occupied the same platform with the student. They observed. They experimented. They deduced such principles or opinions as appeared to be warranted by facts, for the explanation of which, they could propound no theory of universal application, and they left a large mass of facts wholly subject to an enlightened empiricism awaiting the eventualities of further investigation and generalization. There are no more Cullens, Rushes, Chapmans.

While the professor of the present day teaches the practical methods of observation, experiment and study, the prestige of his mere personal opinion is seldom accepted as final authority from which there

can be no appeal. The *argumentum ad hominem* has greatly declined since the time when Professor Paracelsus, who, on being appointed to the Chair of Physic and Surgery, at Basle, professed to be infallible, and attempted to establish this high claim on this wise: "Know, ye doctors, that my hat knows more than you; that my beard has more experience than your academies. Greeks, Latins, Arabs, Frenchmen, Italians, Jews, Christians, Mahommedans, you must follow me; I shall not follow you, for I am your monarch, and sovreignty belongs to me, etc. What are you about, then, physicians and doctors? Don't you see rightly? Have you carbuncles instead of eyes? Your prince, Galen, is in hell; and if you knew what he has written me thence, you would make the sign of the cross with a fox's tail. Your Avicenna is at the entrance of purgatory. I have disputed with him concerning potable gold. O hypocrites, who will not listen to the voice of a physician instructed in the works of God! After my death, my disciples will discover your impostures; they will make known your dirty drugs, with which you have poisoned the princes and nobles of Christendom." Although M. Renouard, in his elaborate History of Medicine, (lately translated by Prof. Comegys,) assigns to Professor Paracelsus a very low place among medical philosophers, yet "M. Malgaigne does not hesitate to proclaim him the precursor of Bacon, the great reformer of philosophy," quoting as a proof of this, the following very significant theory of nature, from a passage in the "*Great Surgery*" "of this precursor": "*Experience, and that which is found familiar and in accordance with nature, and which produces like actions, is the cause of truth and certainty,*" etc.

At the time when the following lectures were delivered, the authority of Broussais had declined in his native land. Dr. Meuhry, who, in 1836, published a work on the State of Medicine in France, etc., says:

"How great the authority of Broussais once was, is well known; it is now on the wane. When one sees him take his seat in the large hall of the school of medicine, in the red cap of the faculty, and his blue spectacles; how he directs his eyes to one spot, chafes more and more as he proceeds with the theme of irritation, and calls to his auditors, the number of whom fills only the first bench: 'Why then, young men, is there no one among you with good sense enough to oppose this doctrine of general symptoms, so that science may march on—march!' he sees at once that the spell is broken. In the midst of his lecture new hearers enter the doors above and below,

until the amphitheatre is filled. They are attracted, however, not by the eloquence of Broussais, but by their desire to obtain places for the lecture on internal pathology, which Andral delivers an hour later.

"The following statistic table will confirm the assertion of the diminished success of the Broussaisan doctrines. In 1823, France still exported leeches to the number of more than a million. After this she exhausted not only her own supply, but also that of England, Germany, and Hungary, and has even drawn them from Moldavia and Wallachia.

	Import.	*Export.*
1820	. . . .	1,157,920
1823	320,000	1,188,855
1827	33,634,494	196,950
1833	41,654,300	868,650
1834	21,885,465	868,650

"One who considers what would have been the history of French medicine without the appearance of Broussais' system will probably be led to the conclusion, that all the means existed before his time of bringing it to that condition in which it is at the present moment. Broussais imparted to the pathology of the alimentary canal, and to inflammation, an excessive importance, which has been diminished gradually, or is now in the progress of being so, to that just measure to which the regular progress of science would in time have brought them." p.40

Physiological pathology or physiological medicine is a luminous doctrine generally accepted by the profession as well as by Broussais, but unfortunately it neither affords light enough to guide the practitioner at the bed-side, nor rational deductions or explanations in pathology and the modes of medication in all cases. Thus if neither the physiological pathology nor the pathological anatomy of intermittent fever was known, and if the physiological action of cinchona, whether it be an excitant, or a sedative, was also unknown, still its curative power is in no wise invalidated by these negations. It is the phenomenal succession of events, and their constancy, and not the laws of casuality whereby they happen, which, for the most part, constitutes the utmost attainment of clinical experience, and often guides the practitioner whose theory may be erroneous to a practice which is correct, whether he believes that fever is localized or diffused, seated in the solids or in the fluids, in the mucous membrane, stomach, nerves or brain, etc.

Notwithstanding the positivism with which Broussais repudiates general symptoms, and asserts certain fundamental principles of his

so-called "physiological medicine," which are warranted neither by facts nor logic, his pathological works (now out of print), are of the utmost value to the practitioner.—B. D.]

University of Pennsylvania, Nov. 17, 1835.

Dr. Chapman, *On the Cause of Fever.*

In the beginning of his lecture, Dr. Chapman asks this question: Does fever have a local or general cause? He remarked that all fevers are secondary, or sympathetic. Fevers follow as the consequence of the primordial impression made upon the system, whether this impression be made upon the sensitive or sanguiferous system, the effect is ultimately the same, only varying in point of time, if first on the nervous system, the fever is not so rapid in its development. The pulse does not at first indicate that derangement of circulation, but, eventually, the impression made upon the nervous system, is propagated to the sanguiferous, and then follow, high pulse, strong vascular excitement, phlogosis, and in short, in quick succession, all the concomitants of a fibrific cause. In the first case, the patient feels debilitated, and prostrated, until the morbific impression is imparted to the vascular system; then excitement follows.

Fordyce remarks that although the cause of fever may be abstracted, still the febrile excitement will not be lowered, and *vice versa*, the reapplication of the cause is not attended with a corresponding increase of the symptoms.

To attack the fever in its inchoative state, is to multiply the chances of its arrest. Instead of awaiting the suggestion of the *vis medicatrix naturæ*, without relying upon the resources of our art, I would strike the flag of our profession, and quit cheating the public, and abandon the practice of so base a fraud.

In the works of the older writers, we frequently find them speaking of natural spontaneous crisis, terminating with a general relief from all the symptoms on the third, fifth, seventh, ninth, eleventh days and so on. But this is doubtless attributable to the temperate and salubrious climes of the countries in which those diseases of which they wrote, prevailed. These spontaneous crises, cannot be hoped for in a climate, variable like ours. The natural course of fever, in this country, is too often interrupted by the physician. Nature operates precisely as a judicious physician should do, and he

should be deemed skilful only so far, as he aids the sanative efforts of this mighty conservative principle of nature.*

November, 19th, 1835.

DR. CHAPMAN, *On Fever.*

Fever in all cases has a local seat, in some of the organs of the œconomy. The morbific agent being in the atmosphere, is commingled with the saliva, and operates directly on the stomach. It is not introduced into the system through the aërial tubes of the lungs, because if this were the case, the first febrile disturbance, as the febrific agent on its first application to so delicate a tissue, would be violent; but the first manifestation of diseased action is generally in the mucous membrane of the stomach; hence nausea, oppression, etc. Individuals, who expose themselves with an empty stomach to these deleterious agents circulating in the atmosphere are prone to take on the diseased impression.

Torpor is induced in the cutaneous vessels by changes of the atmosphere, and as the power is lost in the superficial vessels, it is concentrated in the internal larger ones, and this power or action which is driven from the subcutaneous vessels, falls on the mucous and serous membranes. In the former we have gastro-enteritis, dysentery, diarrhœa, etc. In the latter, pleuritis, sometimes, hepatitis, etc. Rheumatism proceeds from local impressions.

Local irritations through the sympathies, produce fever by both direct and indirect sympathy. Those agents in being applied to the stomach are direct. The indirect are those which affect the skin, brain, and ganglionic system. The mucous membrane of the stomach is most exposed. All our autumnal fevers are *gastric.* The liver is secondarily affected, and then we have regular bilious fever. All fevers begin from derangement of the stomach. Local irritation gives origin to fever, and it may be in any of the tissues. This local irritation multiplies its connections through the ganglionic system, and ultimately involves the general œconomy: First the chylopoietic

* Dr. Hester gives at the close of this lecture, Prof. Eberle's views, viz: 1. Fever is a general disease, the sanguiferous system being essentially, and fundamentaly disordered.

2. The morbid vascular excitement of fever is located essentially in the capillary system of blood-vessels, consisting in irritation, and not in mere increased or decreased action.

3. This irritated excitement may be the result of morbific causes acting directly on the internal surface of the sanguiferous system, or of irritating impressions conveyed sympathetically to the system from a primary source of irritation.

4. The first link in the chain of morbid actions which occur in the development of fevers always commence in the nerves.

5. The remote or exciting causes of fever rarely produce local inflammation anterior to the development of the general febrile reaction.

viscera, then the brain. Each link of the chain of action may be multiplied. Gastric fever sometimes becomes cerebral.

The solids and fluids act ultimately on each other, because one cannot exist without the other.

November 23d, 1835.

Dr. Chapman, *On Intermittents.*

Intermittents and remittents are pathologically alike. The paroxysms of *hectic* are irregular; after a certain time generally two paroxysms of hectic occur twice *per diem.* The paroxysm of hectic is not always preceded by a chill, but sometimes by rigors; no heat or sweating following. The paroxysm of hectic is shown in the hot stage by a circumscribed blush; sometimes the ankles and knees are swollen, and painful; the tongue is clean and red; but in intermittents it is covered with a scabrous, dark brown lamella. The quotidian is apt to run into the continued.

November 24th, 1835.

Dr. Chapman, *On Intermittents.*

In the acute stage of intermittents, the ancients prescribed cathartics, venesection, warm baths. Dr. C. divides the treatment into two parts. First. During the paroxysm, and apyrexiæ, and lastly to avert the chill by warm applications to the feet, by such medicines as ammonia, camphor, and particularly opium. The last of which acts by equalizing the temperature; frictions on the spine, with stimulants and brushes, and by venesection in the cold stage. The abstraction of a few ounces of blood will ever bring immediate relief, in the cold stage, particularly when the blood recedes from the surface to the inner vessels.

In the congestive variety venesection is salutary. The Russian physicians use cold affusions for the cure of intermittents in the cold stage. During the prevalence of cholera in this city, ice applied to the surface, even in the lowest stage, often resulted in healthy reäction.

Hot Stage.—Treat this stage with James' powders, emetics, pleurisy root, ipec., dulcified spirit of nitre, and laudanum, and effervescent draughts. The following will be found a useful prescription in the *hot stage:*

℞. Gum. arb. et. carb. potash, aa dr. 1,
Spirit mindereri. " 6,
Laudanum minims 30.
M. Tablespoonful every half an hour.

Lind recommends opium in the hot stage as likely to produce perspiration, and a remission preparatory to the bark, and other tonics. But in the inflammatory diathesis, opium is not to be used. In this diathesis inflammatory reäction is apt to fall on the brain, and give rise to cerebral affections.

Our remedy in this case is the lancet, cold applications, etc. Cold water thrown into the rectum is often dangerous. Congestion in the brain produces coma and venous apoplexy. Topical bleedings are to be employed; sinapisms, and after these, vesicatories to the stomach, neck, feet, etc. Opium in some of its forms is valuable in the chill, and during the collapsed state. In the central [abdominal] affections of intermittents, opium is useful, though congestion often falls upon the liver, spleen and other viscera.

THE SWEATING STAGE.—This is diarrhœa of the skin; and is to be treated with solutions of alum and brandy, sinapisms, and blisters to the stomach. (Emetics are to be used only in the absence of the gastric irritation.)

November 25th, 1835.

DR. CHAPMAN, *On Intermittents.*

In this affection, Peruvian bark, after evacuants, is to be relied on chiefly. In addition to this, cloves, sup. tartrate of potash, mixed with the quinine. One grain of the sulphate of quinine is equivalent to one drachm of the *bark.* Sixteen grains of the sulph. quinine was given by mistake to a child of twelve years without injury. The smallest quantity, in some constitutions is oppressive, but even an increase of the dose in those cases will not be attended with the same results.

Dose of Quinine.—Three or four grains, twice or three times *per diem,* in solution with laudanum is best. Externally it may be applied after the cuticle has been removed by a small blister, five or six grains of the quinine being mixed with cerate. This mode may be employed in irritation of the *primæ viæ.*

In this disease arseniate of potash, arsenious acid, or white oxide of arsenic, (sulphate of copper is used by Dr. Physick), ammoniated copper, acetate of lead, many of the metallic salts, *spider's web,* Eupatorium perfoliatum, common rhatania, dogwood, etc., have been used.

Local affections sometimes attend this disease, as inflammation of the eyes; treatment: evacuants and blisters; reduction of the inflam-

matory diathesis. In the typhoid or congestive stage: black pepper and wine. It sometimes runs into the chronic state, falling on the liver or spleen; treatment: topical blood-lettings; mercury in the chronic obstructions of this disease is to be used, cautiously; topical remedies, blisters, ointment of c. tartar; emetics.

Dr. Mackintosh, recommends venesection during the *cold* stage of intermittent fever, and remarks that "he has seen men in the most severe sufferings from chills, relieved after the abstraction of seven, eight, and ten ounces of *blood*, and he has known three ounces to suffice. The relief, which is the most perfect that can be conceived, is so sudden, when a good orifice is made, that it has surprised and delighted every one who has practised the plan. The physiology and pathology of the disease would naturally suggest this mode of treatment, when we recollect that all the blood is by the congestion, from the contraction of the smaller vessels driven to the interior, thus curtailing the circuit of its flow, and creating often dangerous oppression, and *vascular phlogosis*.

Dr. Mackintosh had this plan used on himself in 1810, "and although bark and other remedies had entirely failed, he found that before twelve ounces of blood had been drawn the rigors ceased with all their unpleasant accompaniments, and neither the hot nor sweating stage ensued. A pleasant sense of heat succeeded that of cold, and instead of weakness, he was sensible of an acquisition of strength."

In cases attended with a very violent febrile excitement, exhibit some of the refrigerant diaphoretics—such as antimony; nitre; the saline effervescing mixtures. The following may be used.

℞. Spirit. mindereri, ℥iv,
" nitr. dulc. ʒii,
Vin. antimon. ʒi,
Syrup. limon. ℥i. M. Spoonful every hour.

When the stomach is irritable, attended with vomiting in the cold or hot stage, tepid diluents should be given until the bile is discharged. Opium is the best to relieve this affection.

November 26th, 1835.

Dr. Chapman, *On Intermittents.—Diet and Cure.*

Diet is deemed of too little importance by many. Animal jelly is beneficial in the low typhoid state. Quotidians are of a septenary tendency. Dr. Chapman reprobates the delay in the cure of intermittents, and says we should not wait for the spontaneous cure of

nature. The sooner we employ tonics, the system being prepared by evacuants, the earlier we shall effect a radical cure. When we expect the recurrence of the paroxysm we must employ tonics, and keep them up until the third day. Remedies: sulph. iron, in patients of cachectic diatheses. Opium and camphor are to be used when we expect a chill; carb. ammonia; æther; emetics; blisters over the epigastrium, particularly when we fear that the next chill will be dangerous, so as to be in action at the hour of the chill.

The following formula is useful:

℞. Sulph. quinine, gr. ij.
Dulci. spt. nitre, ʒi,
Laudanum, xxx ♏.
Cinamon wat. f.ʒi. M.

November 27th, 1835.

Continued Fevers.

These consist of paroxysms running into each other without much intermission.

Dr. Rush was devoted to the use of calomel and jalap in the cure of fever, and contributed by his authority not a little to introduce these into practice. Fordyce recommends emetico-cathartics.

In these fevers wait for the partial remission of the paroxysm before giving such remedies, and when chronic affections appear, small doses of calomel are to be employed to correct the secretions and remove obstructions.

Enemata are sometimes better than laxatives. It is important that the alimentary canal be evacuated. The excitement of the liver in the commencement of fever is apt to produce engorgment attended with morbid secretion. Dark stools are good signs in cases of fever, and are always to be hailed as favorable. Mercury given moderately, purges, but largely, it stupefies. One grain acts specifically, promoting the secretions. In the early stage of fever venesection is to be practised, and is to be followed up by smart purging. Fever is sometimes ushered in by a phlogosis. In the treatment of fever local remedies are to be directed to every part affected. When purging is neglected in fever, the accumulations of vitiated matters tend to increase irritation. Indurated fæces in the colon, as a piece of dirt in the eye, inflames and irritates, a surface of such delicate and vital texture. Retention of the urine inflames the bladder, and when any secretory sack or organ retains its excrementitious sub-

stance, the secreting surface is sure to suffer irritation and finally inflammation.

The liver must be managed by evacuants both local aad general, but a protracted use of active catharties will induce a state the reverse of that contemplated, or desired in order to reëstablish health.

Whenever watery discharges follow active catharties, and more especially when this watery discharge has been preceded by bilious stools, we must immediately control them, because their continuation will exhaust the patient.

November 30th, 1835.

Dr. Chapman, *On Autumnal or Continued Fever.*

This fever after it has partially exhausted itself may be arrested frequently by antimonial diaphoretics. Cullen contends that antimony, with a view to its febrifuge virtues, must be exhibited to nausea. The medicinal virtues of this drug is exerted upon the secretory apparatus; dose, $\frac{1}{8}$ or $\frac{1}{10}$ of a grain every two or three hours. It may be given in substance conjoined with nit. potash and the submurias hydr.; ipecac, and opium are also of use as diaphoretics in this fever at certain stages. Tartarized antimony possesses a peculiar febrifuge power independent of its other virtues. Sponging the surface with tepid water and vinegar, or spirits is to be employed; blisters to the extremeties; quinine; nitre. Mercury with the physicians of Philadelphia is seldom carried to the point of salivation, but it may be more generally employed in the South.

Liver complaints frequently follow the abuse of mercury, from the over-exciting that large gland, especially in the Southern States.

December 2d, 1835.

Typhus Fever.

Typhus is propagated with more deleterious effect in winter than summer, because in summer the crowding apartments is less; the origin of this disease is generally among the ill ventilated. This disease rarely appears in tropical climates, and when it breaks out in a ship sailing towards the tropics, the disease yields as the vessel approaches the tropical regions.

Typhus fever when caused by heat generally comes on with chills, and having a tendency to a spontaneous crisis. As caused by cold, it comes on with a collapsed state of the cutaneous capillaries, and a corresponding internal flux to the large vessels and viscera; and, if

reäction takes place, the excitement will be considerable, but if it is not established the collapse continues with internal congestion, etc.

Prognosis.—We must in forming our prognosis consider the manner of the production of the disease, and other circumstances. Generally it is most fatal in hospitals from the effect of ill ventilated wards, and it often continues a destined course in despite of all remedial measures, and works its own salvation.

Among many other bad symptoms attending this disease, the following are the most dangerous: Tears; weakness; intense heat, and cold surface alternately; early sweating; pains in the back; abdominal fluctuation, etc. The brain is the prime moving machine in congestive affections, the impression made on any part of the system, being transmitted to the brain, thus as it were referring it to the judge, to decide as to its innocuous, or noxious effects; to determine whether it must be expelled or slighted by the recuperative energies of the system, and if the tribunal shall decide that it is an enemy to the peaceful citadel of health, it reäcts on the part or organ on which the impression was first made, and thus, a determination of all the fluids to this part occurs, in order to rectify the lesion produced by expelling the fugitive enemy of life. The difference between inflammation and congestion is, that in the former the veins are loaded, in the latter, the arteries.

December 4th, 1835.

On the Pathology and Treatment of Typhus.

In this affection the nervous energy is weakened by the typhoid tendency of the constitution, and in proportion to the potency of the morbific agent. The great viscera become the seat of violent and congestive reäction, and being points of irritation it follows that they are also the centers of fluxion. Hence the frequent lesions and functional derangements. The arterial system being endowed with a higher order of vitality, and of course, more to be actuated by external or foreign stimuli, project the arterial fluid with such rapidity in these glandular textures, that the venous action is overwhelmed and soon becomes unable to restore the excess it has received, and in consequence of this primary congestion we may have inflammation.

The efficacy of purgatives is ascribed to the carrying off all sordes; whenever the *primæ viæ* are oppressed with feculent matter they are especially useful, promoting reäction, equalizing the circulation.

Calomel in moderate doses and castor oil are especially useful in these cases. There is a point at which purging should cease, of which we are warned by the watery discharges.

Use, in typhus, cold applications in the boldest manner, or sponging the surface. Immersion is dangerous when we have much congestion, particularly in delicate constitutions.

In local determinations, we must resort to local and general bleeding; if we do not, essential injury will result. After these we use diaphoretics. They were once relied entirely on during the reign of the humoral pathology, and were used to eliminate the offending matter; but this is now only recollected as a false theory and a bad practice. It is always wrong to condemn anything from its mere abuse.

In the mild cases give the neutral mixture, ammonia, the dulcified spt. of nitre, opiates. Antimonials, which have lost ground since Hamilton introduced the purgative plan, need not be carried so far as to nauseate. When the lungs are affected we mix ipecac with antimonials.

Occasional success follows the most preposterous practice, and he who exults over a cure of typhus by stimulants, forgets it was due to the conservative power of the constitution. In the early stage of typhus, venesection is useful. It is stated by *** of Dublin, where he saw two thousand cases in which blood-letting was practised, that it was found to save more than any other plan. Prejudice, retards the advance to truth in science. The actions of life are performed badly, not for the want of strength, but from being overwhelmed.

The proposition in philosophy that, to move a given weight, the power must exceed the weight, applies in typhus fever. The vessels of the system perform their actions sluggishly, and to relieve this we must reduce the mass of circulation, and thus we shall calm the boisterous ocean agitated as it is. The vital energies from the commencement are paralyzed, and evacuants, without care, will destroy the elasticity of the system. In the lower stages, the system lies as an exanimate mass. This condition comes on generally in winter, and the system sinks without an effort to extricate itself. This sometimes happens in autumn and in the best constitutions. The leading indication here is to exonerate the system by removing congestion, and by restoring the powers of the body.

December 7th, 1835.

Stage Second of Congestion.

It is difficult to fix the stage of any acute disease. As it regards the treatment, we must be govered by the state of the system. In typhus the tissues of the stomach are affected. Stimulants are to be avoided in affections of any organ bearing marks of phlogosis. No stimulant is better than carb. ammonia, because it acts on the capillaries, promoting the secretions. The dose must be small, given too freely, it exacerbates the fever and increases debility, from an expenditure of power; wine whey promotes its operation; they act together, aiding each other. The action created by the two articles is more natural than any other. Camphor next claims our attention as a remedy. I add with it, opium and ipecac. These relax the surface; but sweating is not to be carried too far; to produce an imperceptible vapor is far more to be desired; tepid vinegar applied to the surface; blisters also; I do not let blisters remain after they redden the skin. Serpentaria and whey, with bark, are said to be good.

Brown placed opium at the head of the stimulants; in large doses it suspends action, but in small doses it raises excitement, which is continued, if repeated. It removes morbid vigilance. The sulphate of morphia acts like a charm given either by the mouth, or rectum. To meet the same indications, musk and castor may be given, but they are not so good; they are often adulterated. But as a stimulant, Madeira or sherry wine is the best. Two pints of wine are generally administered daily, but if given in too great a quantity it exhausts the system. Carbonic acid invigorates and greatly excites the system as in porter, ale, etc. In the German hospitals the mineral acids were used with success. I think they are useful in the hæmorrhagic stages. Turpentine is prescribed in all low diseases in this city.

December 8th, 1835.

Determinations to the head, with delirium are cured with cold water, ice, vinegar; to have the head shaved is of much importance; have water poured on it; topical bleeding; stimulating applications to the feet; if sleep be wanting attended with general excitement, cold affusions for the arms, bleeding, sulphate of morphia; for vomiting, emetics of warm water, effervescing draught, carb. potash, gum arabic, fresh milk, magnesia and calomel, with a modicum of opium;

coffee; acetate of lead in half grain doses, tinc. of cloves, fomentations to the stomach, pediluvia, with mustard. An opium pill will remain long in the stomach. Before giving the above we should be sure there is no phlogosis. This is a convulsive action of the diaphragm, for which use cups to the neck; vinegar; anodynes; antispasmodics, or musk, etc.

Drinks.—Lemonade, apple water, barley water, ice in small quantities often quench thirst sooner than large quantities; cold drinks sometimes produce diaphoresis. The appetite must be restrained, lest we check the tendency to convalesence. Light and company to be excluded. Free ventilation, and cleanliness to be observed; sprinkle the floor with venegar or cologne water. Nitrous vapor is calculated to obviate contagion; also chloride of lime. Absolute rest.

Relapses are more dangerous than the original disease.

Convalescence. Remove the patient in another room; shave him, etc. No man feels like a gentleman with a dirty shirt. Begin to feed him with farinacious diet, wine, porteree, soups, etc. Often, a little at a time, being a golden rule.

ART. VIII.—*Antaphrodisiac Property of Belladonna.*

MOBILE, ALA., July 21st, 1858.

DEAR SIR—More than a year ago I discovered while giving belladonna to a gentleman sorely affected with hooping cough that it possessed powerful antaphrodisiac property, which I have never heard attributed to it. The gentleman stated that during the whole time he was taking it he was unable to accomplish even an erection.

The discovery of this property, I immediately saw would be of great value in the treatment of diseases of the genital organs, to prevent erections and their consequences.

As chordee is one of the most painful and troublesome complications of gonorrhœa, and one for which I have never seen a certain remedy, camphor, opium, and lupulin as often failing as suceeding in preventing it, the external application of cold and belladonna only

being efficacious at the moment, I thought that the latter internally administered would be the very remedy, and so I have found it. It only fails when the system is insufficiently under its influence.

In the case of the gentleman mentioned, through whom I made the discovery, as long as he felt a slight stupefying effect from the belladonna, his cough was completely kept under, and this effect he endeavored to maintain, though he never complained of the sensation being very disagreeable. This is the measure of its action and the point to be reached, and not to be exceeded. In his case one-fourth grain of pure extract three times a day, answered perfectly. But as the extract is of uncertain strength, and individuals are differently affected by the same dose, of course, each case will require that the desired dose should be arrived at by experiment.

I have never known patients to complain of any very unpleasant effect from it, as from large doses of laudanum at bed time.

Reasoning by analogy, I used the belladonna in a case of distressing nocturnal seminal emissions with perfect success, without the patient experiencing any other effect from it than the prevention of the distressing accident.

May it not act in some peculiar manner upon the neck of the bladder and prostatic urethra in diminishing the irritability of these parts, as I see it has been lately recommended as a specific in incontinence of urine occuring in children? If this is its action, of course it is secondary, as its first and powerful impression is on the brain.

Speaking of gonorrhœa, induces me to mention a remedy that I have seen more successful in the chronic stage, and in all cases after the inflammatory symptoms have subsided than any other: I first used it while on the coast of Africa, about seven years ago, in which climate all gonorrhœas are apt to become chronic and very difficult to cure. While there I had a case of gleet under treatment that resisted everything prescribed by the books, and although temporarily checked would constantly return. Finally, I put him upon the use of the following injection, without any internal medicine:

℞. Creasot. gtts. x.
Acid. tannic. grs. x.
Aquæ ℥iv. M. Inject four times a day, retaining each injection a minute or two; first washing out the urethra with an injection of cold water before using it.

In a few days he was well, and remained so, until by some imprudence his disease was reproduced, when the injection again would in a few days check all discharge. This, as all other injections for the urethra, should be continued at least three days after all discharge has ceased, using it two or three times a day—otherwise, the disease will return. Since I first "worked out" this prescription, I have scarcely ever used any other, and have not yet met with a single case *(uncomplicated with stricture)* of discharge from the urethra, *after the inflammatory stage has passed*, that was not rapidly cured by it. I have also used it in cases where a stimulant and astringent were needed, in salivation, chancres, etc., with much satisfaction.

This injection, with the following balsam-mixture is generally my whole treatment of gonorrhœa, and I have been satisfied with it:

℞. Copaiba.
Spts. æther. nitros.
Spts. lavend. comp.
Aq. calcis, aa. ℥i.
Tinct. opii. ℥ij.
Sacch. alb. ℥vi; ft. mist. S. Teaspoonful morning and noon, and two teaspoonfuls on going to bed, first shaking the mixture well each time.

Of all the balsam mixtures I have seen and used this is decidedly the best. The dose being small, the taste pleasant, and seldom, if ever, disagreeing with the stomach.

I don't wish to be understood to claim this treatment as specific, as that would immediately provoke its condemnation by all scientific physicians. For there are idiosyncracies of individuals and phases of disease to which no simple remedy will universally apply.

Respectfully, yours, etc.,

J. F. HEUSTIS.

Dr. Bennet Dowler, New Orleans, La.

Art. IX.—*Idiopathic Priapism.*

The reading of the preceding paper suggested the subject of *idiopathic* priapism, which, as it is one of the obscurest affections, should for that reason be investigated. The few remarks which follow, will, it is hoped, induce others to contribute what they know from experience

and observation, in regard to this singular affection. By the way, if belladonna be a reliable remedy as an antaphrodisiac, *à fortiori*, it might be valuable (though I do not know that it has been used) in idiopathic priapism.

The causes, symptoms, treatment, and results of morbid affections of this character, should not be ignored through the fastidious notion that writing upon such subjects is indelicate. The æsthetics of science consist in useful facts and in their logical interpretation. The ideal of the true, the beautiful and the good, is for the physician, not the arbitrary rules of taste and beauty, but sound minds in healthful bodies—not a Venus chiselled in marble, but a vital Venus free from maladies.

The history of priapism, except as a traumatic lesion or a symptom of some other disease, seems to have been singularly neglected, or rather never noticed. It has been invariably ascribed to urethral, vesical, prostatic, calculous, gonorrhœal, anal or other maladies.

Priapus, son of Bacchus and Venus, a rather rural god, worshiped by the ancients, especially by gardeners, shepherds, and vintagers, presided over the function of fecundity. This kind of priapism has no connection with the morbid affection, which is *sans* penchant à l'acte vénérien; indeed, this is often true of the symptomatic variety. Idiopathic priapism is not necessarily if ever a cause of infecundity, and, conseqently, there can be no need of the aphrodisiacs, concerning which some respectable authors write; while many charlatans advertise these illusory nostrums in the newspapers. If such remedies ever cure sterility, it is by curing some malady, of which sterility is only the accidental effect. As sterility is sometimes imaginary, it may be cured through the imagination, upon which, this class of remedies act. While persisting anaphrodisia involves the forensic question of divorce, priapism, though a morbid affection, is not open to such cause of action. Idiopathic priapism is chronic, being disagreeable rather than painful, and is under the control of the will, except in the sleeping or semi-waking state.

Several protracted cases of this affection have fallen under my observation in gentlemen of different ages, both married and single, whose health in other respects was good. Some of these cases occurred to medical men of intelligence, who had used various means both hygienic and medicinal without benefit. As the paroxysm is periodical, occurring usually after a sleep of some hours duration, a gentle-

man informed me that he had tried quinine but without advantage. A gentleman who purposed marrying, but deferred it with the hope of being cured of this disagreeable affection, tried not only medicine, travel, sea-bathing, etc., but a mistress also, yet without any change. He remained *in statu quo.*

This malady, for such it may be called, as already stated does not appear to be the result of any known disease of the viscera, bladder, or urethra. Neither venereal disease nor spermatorrhæa can be invoked as its cause, in some instances at least, without rejecting the most reliable testimony. Although the present condition of some of these cases is unknown, sufficient information has been obtained at sundry times to prove that it is of long duration, and is little amenable to medication. Cold local baths of the genitallia, and hygienic measures are useful. As this affection seldom occurs during the first good sleep, but almost always afterward, especially during the half waking or excessively prolonged sleep, the latter should be avoided. When thoroughly awake, and especially on rising from the horizontal position, volition always is quite sufficient to control or keep in abeyance the priapism. Sleep is the constant precursor to this affection, so far as the few cases alluded to are to be relied on for establishing a general rule. Although this subject is not sufficiently ripe for speculation, yet it suggests certain parallel, analogous, or allied topics, which, however, will not now be examined, as the pathology of certain diseases, including somnambulism, the natural history of sleep, the phrenological hypotheses of Gall, the physiology of the brain and spinal cord, and psychological medicine.

While writing the last page of this article, the new edition of Lallemand's most excellent work on *Spermatorrhœa** was recieved—from which the following extracts are taken with a view of not only enriching this barren paper, but as a reply to some unanswered letters of inquiry received some months since. M. Lallemand does not allude to idiopathic priapism (a designation not to be taken as absolute but apparent); he assigns as its principal cause worms in the rectum of spermatorrhœal and erotic patients. The following are believed to be nearly all the cases, in which this symptom appeared among his numerous histories of spermatorrhœa, whether as the seqelæ of gonnorrhœa, masturbation, or other morbid affections; whence it

* See Catalogue of Books in this Journal.

may be inferred from his large experience, that secondary or symptomatic priapism seldom occurs. These cases will not only indicate M. L.'s therapy, but also show the differentiæ between the primary and secondary condition; the former having, as already stated, no apparent connection either with other diseases, or erotic ideas, dreams, etc.

CASE XXVI.

The habit of masturbation contracted spontaneously at the age of fifteen, and continued until the age of twenty—Nocturnal and diurnal pollutions—Increasing disorder of the health until the age of twenty-nine—Frequent and prolonged erections—Pain at the margin of the anus, etc.—Cauterization performed without benefit—The expulsion of ascarides followed by rapid recovery.

M. R——, a student of medicine, enjoyed good health in his childhood, but about the age of fifteen was tormented by frequent and prolonged erections. One evening, for the relief of the itching, of which the extremity of the penis was the seat, he rubed the organ violently between his hands. This led to the establishment of masturbation as a habit, or rather as a passion, the patient practising it sometimes as often as eight or ten times a day. His health by degrees became so altered that one of his friends suspected his practices and told him the danger of his situation. By degrees he corrected himself, though not entirely, before he had attained his twentieth year. On his renouncing masturbation, nocturnal emissions supervened, and often occurred two or three times a night. They diminished after a time, but without ceasing entirely, and seminal emissions during defecation and the emission of urine were added to them. Thus his health became daily more and more disordered for nine years, notwithstanding absolute continence, a severe regimen, and the use of sedatives, tonics, and antispasmodics. At length he grew incapable of any mental exertion. In 1837, he came to Montpellier, at the age of twenty-nine, in the following condition: Extreme emaciation; face pale; appearance stupid and confused; intellect dull; reasoning powers much affected, the patient being incapable of connecting two ideas on the most simple topic of conversation; loss of memory; constant headache referred to the forehead and temples, and increased by any mental excitement, being then accompanied by nervous tremors, and an almost idiotic state; sleep broken and unrefreshing; constant sighing; frequent attacks of congestion of the head, especially at night; violent noise in the ears resembling the sound of a waterfall; vertigo; stunning sensations, giving rise to a constant fear of apoplexy; timidity carried to a ridiculous extent; panics of fear even during the day; character gloomy, taciturn, restless and irritable; horror of the least noise, and of all society; irresistible restlessness; great weakness; abundant sweats after very slight exertion; almost constant coryza; frequent dry and hard cough; pains in the base of the chest, the region of the heart, and along the spinal column; appetite voracious; dragging at the pit of the stomach; difficult digestion, accompanied with the de-

velopment of flatus; grinding of the teeth during sleep; burning at the point of the tongue; darting pains in the bowels, especially in the rectum; obstinate constipation alternating with violent attacks of diarrhœa; stools containing much mucus and sometimes streaked with blood; periodical pains at the margin of the anus, in the perineum, penis, and testicles; urine passed in large quantities, and very frequently, always throwing down a whitish, thick, and very abundant deposit, involuntary emissions during defecation, both when constipated and relaxed; frequent and prolonged erections by day as well as by night; with constant presence of erotic ideas.

On sounding this patient, I found the urethra very sensitive, especially towards the neck of the bladder, and I consequently thought that the nocturnal and diurnal pollutions were kept up by a state of irritation arising from masturbation. I therefore proposed cauterization. This was performed on the following day, and produced the usual *immediate* effects, but its *curative* effects did not take place as I had anticipated. I then directed the patient to notice his fæces, and a few days afterwards he told me that he had observed numerous little worms passed in his stools. I now ordered enemata of cold water, and salt and water, which however, produced only a momentary effect—probably because the ascarides inhabited the upper part of the intestine. A few doses of calomel, however, caused them to disappear without returning; and from this moment the involuntary diurnal emissions ceased entirely, the nocturnal emissions became more and more rare, and the patient's re-establishment progressed very rapidly. M. R—— returned to his studies with ardour, and long afterwards all functions were perfectly well performed.

It appears evident that the irritation caused by the ascarides in the rectum, first lead this patient to practice masturbation, and afterwards kept up involuntary seminal discharges. I did not discover this at first, because the history of his case, sent me by the patient, was so long, and was characterized by such disorder and want of clearness, that I was unable to arrive at any satisfactory conclusions from such a chaos; his answers were still more vague and unconnected, so that my attention had been chiefly attracted to the state of his intellect, and the abuses he had committed. But after seeing the little success of cauterization, and again reading his notes, I paid more attention to the circumstances attending the commencement of his practising masturbation, and I noticed several symptoms to which I had not before attached importance, such as grinding of the teeth during sleep; burning pain in the point of the tongue; pain in the rectum, and at the margin of the anus; the stools always containing mucus, and sometimes being streaked with blood; and especially the frequency and duration of the erections, and the constant presence of erotic ideas.

When costiveness is present the stools rarely contain any quantity of mucus; its presence alone, therefore, in such a case, would indicate that the rectum is irritated by ascarides. But a still more characteristic point is, the long duration of the erections. When involuntary spermatic discharges are induced by any cause except this,

the erections diminish in proportion as the disease advances, first becoming incomplete, and afterwards disappearing entirely. When, therefore, energetic and obstinate erections continue, in spite of the great wastings of the body produced by them, they must be kept up by some other stimulus to the organs than the natural one, and I believe irritation by ascarides to be the only cause capable of producing this effect. This, on the other hand, agrees perfectly with what I have already stated concerning their influence on the genital organs.

CASE XXVIII.

Masturbation at the age of ten—Seminal emissions produced by horse exercise—Nocturnal, and afterwards diurnal, pollutions—Constant erections—Stools relaxed, and containing abundance of mucus—Burning in the anus—Cauterization with slight benefit—Expulsion of ascarides followed by rapid and complete recovery.

Alexander A——, of moderate stature, the son of robust peasants, enticed by the example of his companions, contracted, when about ten years old, the habit of masturbation, which he practised for a year before he obtained any seminal emission. From seventeen to eighteen he had sexual intercourse, but he afterwards returned to his former habits. He soon complained of general lassitude, weakness of the extremities, shortness of breath, and a sense of suffocation after the least exertion; loss of appetite; difficulty of digestion. Very abundant seminal emissions were frequently excited by horse exercise, and occurred without erection, although not without slight sensation. At a later period he suffered from severe and prolonged palpitation from slight causes; frequent colds, and an almost habitual cough, attended with expectoration of mucus mixed with much blood. At this period, A—— mentioned his condition to a medical man, who explained the cause of his disorders, and A—— at once corrected himself; but nocturnal pollutions soon appeared. At first these were very frequent, but after a short time they only occurred three or four times a week, and at last only three or four times a month. Still the patient's palpitations, difficulty of breathing, and digestive disorder, continued to increase. Flushes of heat to the head were added to these symptoms, together with pain in the loins, which extended with a creeping sensation, along the vertebral column, and was distributed to the shoulders and arms; frequent cramps and chilliness of the extremities; extreme weakness of the legs, and a frequent desire to micturate and defecate. The patient gave up music, in which he was a proficient, and took a dislike to society, especially that of women: timid, irritable, and unsettled, incessantly occupied by thinking of his disease, he was unfit for any occupation, became a prey to despair, and was several times on the point of yielding to the impulse to suicide, which constantly obtruded itself before him.

In this condition A—— came to consult me, in the month of October, 1836, being then twenty-one years of age. I at once perceived that the nocturnal emissions had given place to diurnal ones, and the minute details into which the patient entered confirmed me fully in this opinion. Each time that he went to stool, he had an emission

from the penis of a greater or less quantity of viscid matter, which presented the characteristics of badly formed semen. His stools were repeated two or three times a day; they were liquid, contained a large quantity of mucus, and left a severe burning pain in the rectum. His urine was habitually muddy, thick, and of a disagreeable smell, and after its emission a thick gummy matter, which left a mark on his linen, remained at the orifice of the glands. The patient was, besides, annoyed day and night with incomplete but very constant erections.

Having observed, for several days, the presence of semen in the urine, I performed cauterization of the prostatic portion of the urethra. Fifteen days afterwards, a sensible improvement was evident in almost all the symptoms; yet no farther progress was made, notwithstanding the use of Spa water, iced milk, etc. The stools still continued liquid, and mixed with mucus, resembling a thick solution of soap and water. This really dispiriting condition continued during three months, when I learned that the patient had several times noticed ascarides in his stools.

In a few days after, he was freed from this source of irritation by means of injections, and from that moment his re-establishment progressed rapidly. His love of occupation soon returned, and he applied himself diligently to the study of pharmacy.

This, then, was a case in which the involuntary emissions were *kept up* by the irritation of ascarides in the rectum, although it would appear that masturbation was the cause of their origin. I have recently been consulted by one of my former pupils, for a similar case, in which the discharges were very serious, and had resisted the most various modes of treatment. They were attributed to masturbation, and the patient's confessions justified this opinion; yet a passage in his letter convinced me that a mistake had arisen on, at least, one point. After speaking of supposed hemorrhoids, which irritated the margin of the anus, the patient added that the pain and itching he felt there were such, that he often introduced his finger forcibly into the rectum, and had several times brought down ascarides on withdrawing it. This circumstance, previously neglected, caused me to think that the ascarides, if they had not caused the pollutions, at all events kept them up, and I prescribed accordingly, with success. We must remember, then, that the emissions may be kept up in persons who have practised masturbation, by the presence of ascarides, even in cases in which these entozoa have not excited the habit; and on this account it is necessary to consider their presence with much attention. On the one hand we must be on our guard against attaching too much importance to the occasional presence of one or two of these worms in the fæces. In such cases the want of success of vermifuge remedies shows that the ascarides are not of so much importance as they may have been considered. We must, therefore, be careful in all cases, not to draw conclusions too precipitately from first appearances. No disease, in fact, requires more patient research and greater tact in its diagnosis and treatment, than are necessary in complicated cases of spermatorrhœa.

To resume. The cases reported in this chapter show that affections of the rectum excite involuntary emissions. First, mechanically; by compressing the seminal vesicles during the passage of fæces. Secondly, virtually; by the extension of irritation from the rectum to the seminal vesicles.

All causes which oppose an obstacle to the exit of fæces act in the former manner. I have recorded cases in which the mechanical obstacle was placed at the margin of the anus, (cases 15, 16, 17, and 18,) because, in such cases, the cause is perfectly isolated, and its action is evident: but it is also evident that any physical action like that resulting from horse exercise, (nineteenth case,) from carriage exercise, or from remaining too long in a sitting posture, as well as all medicines which tend to produce constipation, may be followed by the same effects. In all cases of this kind the influence of the rectum on the seminal vesicles arises from its distention by fæces, and is a perfectly *mechanical* action.

The other phenomenon is essentially *vital*. The diarrhœa, (case sixteenth) the ascarides, (cases 22, 23, 24, 25, 26, 27, 28) and the eruptions at the anus, (case twelve) could only act in this manner. The same may be said concerning the action of injections, either too hot or too cold, and of certain drastic purgatives.

In many cases too, the distention and the irritability of the intestine act simultaneously on the seminal vesicles. Hemorrhoids and fissures of the anus, for instance, cause pain and irritation, and give rise to spasms of the sphincter, at the same time that they form an obstacle to defecation. Obstinate and continued costivness too, is rarely exempt from heat and irritation in the rectum and its neighborhood; and eruptions about the anus are often accompanied by an irritability of the sphincters, opposing defecation.

We have seen (case twenty-one) that chronic inflammation of the urinary organs may excite, by its influence, so great a susceptibility of the rectum, that the fæces are no longer able to be retained; and here cauterization of the mucous membrane of the genito-urinary passage sufficed to dispel the irritation of the rectum, so that the patient got rid, at once, of his diarrhœa, his incontinence of urine, and his involuntary seminal discharges. The influence of the rectum on the genito-urinary organs is then reciprocal; and plays a much more important part in causing spermatorrhœa, than if it acted only by mechanically compressing the seminal vesicles. Its due consideration is, therefore, of much importance in the treatment of these cases.

CASE L.

Masturbation—Extreme weakness of the limbs and senses—Erections excited by percussion of the occiput—Catheters left in the urethra—Rapid recovery.

Dubourdeaux, æt. twenty-one, a soldier in the 36th regiment of infantry, of strong constitution, practised masturbation at the age of fourteen as often as three or four times a-day without much injury to his health. At the age of twenty, he entered the army. A few

months afterwards, he contracted a blennorrhagia, and while suffering from it performed a long march. He was scarcely cured when he took another long march, bivouacking at night for about a month. Otitis occurred, and was treated with leeches and blisters, but ended in suppuration. After this was cured, the patient suffered much from noise in the ears and vertigo, and his sight became very weak.

On the 21st of December, 1830, D—— came to the hospital of St. Eloi, in the following condition: itch; enlargement of the prostate; sense of weight in the rectum; extreme weakness of the limbs; serous infiltration of the hands, legs and feet; and almost entire loss of vision. After the itch was cured, the paralysis of the inferior extremities increasing, issues in the loins were prescribed, but produced no benefit.

When the patient came under my care, he could neither stand, nor distinguish the numbers on the beds in his ward. He told me, that on accidentally striking his occiput, he had experienced a lively sensation resembling that produced by ejaculation, together with injection of the corpora cavernosa, producing more or less complete erection. He had since frequently repeated percussion of the occiput, which was always followed by the same phenomena; the voluptuous sensation induced seemed to pass through the whole length of the spinal cord, to the extremity of the sacrum. Some circumstances leading me to believe that the patient abused this discovery, and practised manœuvres he did not admit, and the swelling of the prostrate at the same time rendering the discharge of urine difficult, I left catheters in the bladder, increasing their size gradually, and taking care to withdraw them as soon as sufficient inflammation had been excited, and to replace them when the pain in the urethra had subsided.

The result soon convinced me that my suspicions had been correct. By degrees the paralysis of the lower extremities, the œdema of the hands, and the weakness of vision were relieved. After introducing the catheter six times in as many weeks, the patient had recovered his strength, stoutness, and the free exercise of all his functions. He left the hospital a few days after, confessing his errors and promising not to repeat them.

From the first information given by this patient, I thought that the attack of otitis had probably produced some chronic affection of the brain or its membranes, but the sequel showed that the almost perfect paralysis of the lower limbs, the swelling of the hands, and other symptoms, exclusively arose from masturbation. I have already mentioned the advantages derivable from the presence of a catheter in the urethra, in checking masturbation; but in the present case, a very remarkable circumstance offered itself, viz., the influence exercised on the genital organs by percussion of the occiput. The effects of this procedure were so constant, that the patient was able to procure himself erections at will, and to give himself up to his passions without restraint.

CASE LIII.

Natural phimosis—Erections at the age of eight—Attempt at coitus at nine—Vesical catarrh—Diurnal pollutions—Paraplegia, etc.

A peasant consulted me for his son, æt. fifteen, who, for two years, had experienced a constantly increasing paralysis of the lower extremities. On his sides and loins were marks of numerous issues which had been tried during two years. Large excoriations had formed on the sacrum and trochanters.

On examining the genital organs, I noticed that the prepuce was very narrow, and on pressing it to get rid of the sebaceous matter which presented at its orifice, the penis became erect. I learned from the parents that this boy had erections at the age of eight; and that, at nine years of age, he had been found attempting coitus. The boy himself admitted that the itching with which he was tormented led him to rub the genital organs, and thus induced manœuvres which he had since continued.

The first symptom that presented itself was frequent desire of micturition, and this was followed in about a year by complete incontinence of urine. In the course of the second year, the patient's legs grew week; he lost his intellectual capacity; digestion became disordered; diarrhœa came on; and the discharge of urine and fæces caused excoriation of the skin. Salt and aromatic baths, tonics, excitants, etc., had been just as useless as issues. The cause of the disease was unsuspected.

Masturbation had become very rare, but the urine was thick, muddy, and very fetid—so much mucus was passed, that I was unable to make sure of its containing semen—but the patient had constant pollutions at stool.

I first performed ablation of the prepuce; and eight days after, I cauterized the bladder and surface of the prostate. A month afterwards, the urine was perfectly transparent, and presented a healthy appearance; it was no longer passed involuntarily. Sensibility of the skin of the lower extremities had returned. Improvement was here arrested, however, and I lost sight of the patient.

This patient had never been subjected to the influence of bad example, and had always been well cared for. He explained clearly how he was led to practice masturbation; and circumstances gave an appearance of truth to his recital. It is, then, to the irritating action of the sebaceous matter that we must attribute his unfortunate condition.

CASE LI.

Sickly childhood—Nervous temperament—Masturbation rare—Coitus still more so—Symptoms of aneurism gastritis—Nocturnal pollutions—Predominance of erotic ideas—Tension at the nucha—The application of cold lotions to this region followed by considerable improvement.

A tax gatherer, æt. thirty-four, of nervous temperament, whose childhood had been very delicate, from his having suffered from digestive disorder, with frequent vomiting, consulted me. He had prac-

tised masturbation occasionally about the age of sixteen, and at a later period had had sexual intercourse, but never more frequently than three or four times a week. At eighteen years of age, he suffered from palpitation of the heart, attended with oppression of breathing, pain in the stomach, and involuntary muscular contractions. These symptoms gave rise to suspicions of aneurism, gastritis, etc.; in consequence of which, frequent abstractions of blood, both locally and generally, and the use of blisters and issues, were prescribed: with the exception of the issues, these means all proved injurious. The blisters evidently seemed to favor the occurrence of nocturnal pollutions, which took place three or four times a week about this time, taking the place of voluntary emissions. Shortly before he consulted me, this patient attempted sexual intercourse by the advice of his physician, but found himself much worse afterwards. All the precautions recommended to prevent the return of the nocturnal pollutions had also proved unsuccessful, and the patient had remarked that these were more debilitating in proportion as they took place with less signs of erection or feelings of pleasure. Their effects were often felt for several days.

The extraordinary presence of erotic ideas in this patient struck me forcibly. Notwithstanding the small development of the genital organs, nothing could remove lascivious images from his imagination; they were present during the most serious study. In vain he gave up theatres and amusements; in vain he had recourse to serious books, and religious or scientific discussions; he was constantly assailed by libidinous thoughts, which presented themselves under a variety of forms, and were ever present in his dreams. He experienced, also, an habitual sense of tension and uneasiness in the posterior and inferior region of the head. Of all the means employed, cold lotions applied to this region alone produced any notable diminution in the frequency of the nocturnal pollutions; but this effect was never of long duration.

In this patient the causes by no means tallied with the serious effects produced, or with the persistence of the disease. Numbers have practised masturbation and coitus much more frequently without experiencing any ill effects. On the other hand, the constant presence of erotic ideas showed an activity of the genital instinct, which was by no means in relation with the condition of the genital organs. The sensation referred to the nucha, and the effects of cold lotions applied to this part seem to indicate that the affection was produced, or at least kept up, by abnormal excitement of the cerebellum.

In several cases of involuntary seminal discharges, I have found the patients complain of habitual heat, dull pain, and sense of tension in the occipital region; sometimes accompanied with pulsation of the arteries. One of my patients experienced a nocturnal pollution whenever his head rested on a soft pillow. I must admit, that in such cases I have not obtained much success by using applications to the nucha and its neighborhood; indeed, I have only once seen temporary improvement result from this treatment.

On the other hand, these symptoms are much more rare than any others of which I have hitherto spoken. Thus, for example, symptoms which we may refer to the brain, are much more frequently presented. There are few patients who do not experience diminution of memory and intellect, pain in the frontal region, pulsation in the temporal, and weight in the anterior and lateral parts of the head, with attacks of vertigo, dazzling of sight, and cerebral congestion: some even have serious falls; and the integuments of the face are much more frequently red and burning than those of the nucha. Symptoms of chronic stomach disorder are still more common, and it is by no means extraordinary to see accidental irritation of the stomach increase or re-excite involuntary seminal discharges. The influence of the cerebellum in causing spermatorrhœa is, therefore, a subject which requires further research, with varied and numerous cases, in order for it to be properly understood.

In such cases my experience leads me to recommend the application of ice and leeches to the nucha, when particular symptoms are observed in this region, or when other means have failed. But in order to judge the effects of these remedies fairly, it is necessary to abstain from all other treatment at the same time, and to guard against preconceived opinions. Exaggerated opinions have done more injury to the truth than the most violent opposition.

It is impossible, for instance, to admit, with Gall, Voisin, Londe, Chauffard, etc., that we should always direct our remedies towards the cerebellum in cases of satyriasis, nymphomania, etc., or that the cerebellum is always the origin of the phenomena which take place in the genital organs. Such an assertion scarcely requires a serious consideration. It is hardly necessary to refer to the powerful and immediate influence exercised over the thoughts, dreams, venereal desires, erectile tissues—indeed, over all the functions, and all the organs of the economy, by the presence of well formed semen in its reservoirs. Such effects are seen daily, and constantly, and have been well understood for a long time. Ascarides, by causing irritation in the rectum, suffice to excite long-continued erections, even in children, and to incline towards abuses and excesses; or, by acting directly, to bring on debilitating involuntary emissions. Sebaceous matter retained between the prepuce and glans, may produce the same effects. In women, excision of the clitoris takes away the passion for masturbation. It is well known, too, that the presence of an eruption on the labia often excites nymphomania. How do these facts accord with the opinions of those who would refer all such influence, and would direct all their remedial measures, to the cerebellum?

There are other arguments which I regret to see incessantly brought forward to prove the action of the cerebellum on the genital organs. As an example of these, I may mention the effects which *sometimes* accompany apoplexy of this organ.

Gall well explains the proneness to masturbation in such hydrocephalic children as attain the age of puberty, by remarking that this affection, acting only on the brain, allows the cerebellum a predominating influence. This deduction seems correct, and may be

applied with just as much truth to idiots and Cretins. But apoplexy of the cerebellum instantly abolishes the function of the part attacked: to explain pathological erections by this alteration, and to conclude that the cerebellum is the exciting organ of the genital function is, therefore, an evident contradiction. Instead of laying stress on such facts as these, Gall and his followers should have regarded them as serious objections to their system; they should have sought to discover how erections could take place in spite of the greater or less destruction of the cerebellum; they would then have seen that such erections are less common than they are supposed to be in cerebellar apoplexy, and that they much more frequetly accompany injuries of the spinal cord, etc.,—circumstances which the adversaries of phrenology have taken care to remark: so true is it, that truth makes itself known by all ways.

The exaggeration and false reasoning that have obscured all discussions relative to the true seat of the genital instinct must not, however, induce us to forget the importance of pathological facts. What I have said, should make the profession take into consideration the influence the cerebellum may possess in causing involuntary discharges of which they are unable to discover the cause, especially when such discharges are accompanied with special systems referred to the occipital region.

Priapism sometimes occurs in yellow fever. In the first case of this fever (a sporadic one) which I was called to treat, and the notes of which I now subjoin, this phenomenon was presented.

New Orleans, July 17, 1836; morning.—E. C., aged twenty-five, born in Ireland, resident in the Southern States three years, a stout, muscular canal ditcher, one week from Mobile; sick six days, during which, he took three small powders (prescribed by Dr. K.) Skin cool, orange colored, with mosquito petechiæ or small bloody extravesations on exposed parts; countenance vacant, staring, idiotic; eyes yellow, injected, prominent; abdominal tenderness, chiefly at the liver; comprehends questions; no pain. Bowels costive; no urine but once in small quantity, in four days. Pulse, air like; (became fuller in the evening); thirst; nausea; vomiting of a lead and dark matter, with mucosity; tongue and gums, furred with dark yellow substance. Breath offensive; respiration quick. In the night, hæmorrhages from the gums, ears, and stomach; debility, cathartic; cups; sinapisms; lemonade; enemata.

July 18th.—Delirious; drove the family out, walked the street, where he fell, in a convulsion, brusing himself badly. Became rational; complained of cramps in his legs; vomited blood; hæmorrhages from the ears and gums. Pulse quick and variable. In the evening

anenema, caused him to arise from bed; had a small black-vomit like stool; suddenly fell into violent convulsions; arms rigidly flexed, fists clenched, universal rigidity, body bent back, muscles contracted in lumps; mucous foam at the mouth; eyes widely opened, prominent, fixed, without winking, face livid. Breathing, laborious, noisy, hissing. The spasms and backward curving of the body relaxing, he became noisy, and tried to get out of bed; the entire strength of several persons was required to restrain him, until his senses returned. After four similar paroxysms he expired at sun set, having had towards the close of life, *a strong continuous priapism.*

PROGRESS OF MEDICINE.

ART. I.—*The Antagonism of Opium and Quinia:* By M. Gubler. Translated by J. P. BARBOT, M. D.

M. GUBLER read a paper before the *Société Médicale des Hôpitaux de Paris*, on the antagonism between Opium and Sulphate of Quinia, of which the following is a synopsis condensed from the summary published in "*L'Union Médicale*," of May 20, 1858. Being unwell himself, M. Gubler took sulph. quinia in 0.50 centigrammes doses only, and was struck with the fact that they produced humming in the left ear only, although his hearing is equally good on both sides. This peculiar effect occurred three days in succession. As at that time he suffered from a head-ache which was most violent on the right side—on which side it is always greatest in M. G., he was led to suppose that the evident congestion on the right side, neutralized the effect of the quinia, which effect M. G. considered due to the privation of the brain of blood, *(anémier l'encéphale décongestionner le cerveau)* the removal of congestion of the brain. M. G. having recovered his health, resumed his attendance in the wards of the Hospital. He there saw a case of

acute articular rheumatism in which large doses of sulph. quinia and opium had been administered for several days without success. M. G. continued the dose of sulph. quinia—*i. e.* 1 gramme 50 centigrammes with 0.25 centigrammes of extract of opium. Finding that the peculiar therapeutical effects of both remedies were entirely wanting he increased the dose of sulph. quinia and diminished that of the opiate, without, however, any better success.

Lastly, he left out the opium altogether, and gave one gramme 50 centigrammes of sulph. quinia alone, which produced in the most marked manner, the peculiar remedial effects of that drug. And from that time the rheumatism diminished rapidly and markedly. This and other subsequent cases of the same nature confirmed him in his belief that opium was antagonistical to sulph. quinia or, so to speak, its antidote.

M. Gubler, enunciates his particular views of the *modus operandi* of opium and sulph. quinia. According to him, opium produces congestion and hyperæmia, while its antagonist, sulph. quinia, produces anæmia and dissipated congestion—*(anémie et décongestionne.)*

The following are the conclusions of M. Gubler: 1. Inversely to opium, which exalts organic action, producing sanguineous congestion and caloricity, sulph. of quinine acts on the nervous system, by condensing the forces there, in such a way as to arrest organic action, the source of waste, and to diminish as much as possible the afflux of blood in the inflamed parts. *(Sic.)*

2. This *modus operandi* once admitted, we can readily understand the innocuousness of sulphate of quinia in the cerebral symptoms of rheumatism, which, symptoms recent experiments have already tended to show were not due to its use.

3. Moreover, the use of sulph. quinia is indicated in all the inflammatory forms of cerebral rheumatism; opium being serviceable in the nervous forms only, and in these, only when not complicated by fever.

4. Sulph. of quinine and opium, being antagonistic should not be given together.

5. These two remedies may be used as antidotes, to one another.

M. Guérard thought that sulph. quinia did "*décongestionner le cerveau,*" and stated in support of his opinion, that its use produced imminent syncope. Some years previously, while suffering from intermittent fever, he had taken large quantities of sulph. of quinia, sometimes for a month at a time, in a single dose daily. As long as he remained in

the recumbent, position, he experienced no unpleasant sensation, but when sitting, syncope was imminent. He had seen a second similar case.

With respect to the antagonism of opium to sulph. of quinia, he was the more ready to believe it, inasmuch as in his *thèse de concours* for the Chair of Therapeutics, he had shown that the effects of medicines when isolated might be neutralized by combination; and had mentioned that M. Caventou had given strychnia combined with morphia, each in large doses, and that the effects of the combination had been greatly diminished. Substances which are poisonous by themselves, cease to be so when united.

NOTE BY THE TRANSLATOR.—The conclusions of M. Gubler, on the antagonism of opium and sulph. of quinia, although endorsed in a measure by M. Guérard, fail to convince me. Further trials on a more extended scale, would show the correctness or incorrectness of his views. Should his opinions turn out to be correct, quite a revolution would take place in the administration of sulph. of quinine in this city, where, either from fashion or conviction it is most generally given in combination with opium. The *Haustus Quiniæ* of the Charity Hospital, which has done so much service, would then have seen its last day.

ART. II.—*Puerperal Fever:* By M. VELPEAU. Translated by J. P. BARBOT, M. D.

PUERPERAL fever has been for some months the theme of an animated discussion before the Academy of Medicine of Paris. On the 25th May, M. Velpeau took a part in this controversy and spoke for two hours on the subject. The following is a *résumé* of his remarks as published almost *in extenso*, in the *Union Médicale*, of May 27th, 1858. It will be seen, therefore, that M. Velpeau still professes the same opinions that he entertained and emitted forty years ago.

After stating that he had been reluctantly drawn into this controversy, by being directly called upon, and adverting to the opinions advanced by himself from 1818 to 1824, that fluids were probably oftener than solids the cause of disease; that in parturient women the elementary principles of the *lochiæ* may be taken up by the uterine vessels, and, (as he had himself seen twenty times), carried into the veins; that the pathological phenomena of phlegmasia alba dolens are all due to the changes in the fluids, and that when pus existed in the uterus it might be absorbed by the patulous orifices of the urine blood vessels after delivery. M. Velpeau exhibited a thesis on puer-

peral fever, not signed by himself, (but which he admitted *he* had either drawn up or helped to draw up,) in which it was shown that the putrid matters might be carried into the veins and cause peritonitis in parturient women.

Those who have taken part in this discussion, says M. Velpeau, may be divided into two classes—two camps; one side maintaining that puerperal fever is a fever *sui generis*, and the other that it is symptomatic. The former cling to the epithet fever—the latter will not admit it.

The former assert that it is due to crowded localities—their insalubrity—contagion, etc., etc. M. Velpeau shows that if puerperal fever were due to over-crowded rooms, it should be more frequent in large cities and large hospitals, than in country villages, and more common in the larger than in the smaller hospitals. He read statistics from the hospitals of Paris tending to show the contrary. He quoted Dr. Robert Lee to show that Dr. L. (as well as himself,) had occasionally seen more cases in their private practice, among the better classes, than occurred at the same time in the large hospitals.

Contagion has been by some considered as the cause of this disease. M. Velpeau cited cases to show that these so-called cases of contagion were probably mere coïncidences.

And now for the symptomatic doctrine, *i. e.* that puerperal fever is produced by material lesions *(lésions matérielles)*.

M. Béhier says that he always found phlebitis to be the incipient stage, *(point de départ)*. M. Cruveilhier, and (I believe) M. Cazeaux, think that inflammation of the lymphatics is the most common.

Here are two important and serious lesions, apart from the inflammation of the uterine appendages and the peri-uterine parts, and the many morbific influences to which a woman is liable after delivery. The close connections between the peritoneum and the organs of gestation; the bruised and lacerated condition of the latter after delivery, and the prolonged and violent efforts on the part of the mother necessary to expel the product of conception—which leave her panting and exhausted—all these, together with the fact that all at once, an enormous vacuum is formed—the sudden change in her circulatory vessels, the immense pressure upon them thus suddenly removed, give rise to a predisposition to congestion, and produce the condition which develops inflammation. When we take into con-

sideration the great extent of the peritoneum, we can readily understand that its inflammation must materially disturb the functions of all the organs over which it is reflected, besides causing thereby great nervous disturbance. Peritonitis is also a frequent cause of death after the operation for strangulated hernia.

The lesions which follow delivery as lymphangitis, phlebitis, erysipelas, phlegmon, etc., etc., are still further complicated by the possibility of the pus passing into the general circulation—as phlebitis is far from being always adhesive. My ideas of the manner in which the absorption of pus takes place have been greatly misinterpreted. I believe that the disease is sometimes produced by purulent absorption, and sometimes by phlebitis. Dance, who has been set up in opposition to me, and who did not write on the subject till five years after I had, says it was due to phlebitis alone.

It has been objected, but of this I am well aware, that pus is not always toxic. That would depend on the quantity and quality of the pus. The experiments of MM. Castelnau and Ducrest, have demonstrated that pus injected into the veins in small quantities, frequently repeated, was fatal, thus disproving what *seemed* to have been proven by M. Gaspard. M. Bérard has said that pus globules could not enter the veins; this, I never could be convinced of. Besides, pus alone is not the constituent of the globules; there is besides, the fluid in which it floats; besides, these cells may be disaggregated. Some micographers assert that white blood corpuscles cannot be distinguished from pus globules. All connected with this, is doubt and uncertainty. A person pricks himself in the hand when dissecting. The axillary glands become engorged and inflamed; still nothing can be discovered in the track of the lymphatics, and yet it cannot be denied that in this case, a morbific particle has traveled from the prick to the axilla, producing adenitis. Pus may undergo many metamorphoses, which we cannot understand or account for, but of which we can judge by their effects. Twenty years ago, Bonnet showed, that when pus was altered and combined with sulphuretted hydrogen, it became almost as violent a poison as hydrocyanic acid. At any rate, if in lymphangitis, the glands may arrest the progress of the morbific matter and neutralize it; such an event cannot occur in phlebitis. In the latter case, the pus molecules, carried into vessels which continually increase in diameter, and in which the current increases rapidly,

may ultimately reach the heart and give rise to serious consequences.

Angeioleucitis and phlebitis, when circumscribed, are not fatal, but can be readily subdued. But non-adhesive phlebitis is rapidly fatal because it soon becomes general. * * *

I do not consider phlebitis to be the cause of puerperal fever. It aggravates but does not cause it. Nor do I think it is produced by purulent absorption. I consider it to be an inflammation, beginning at the uterus as a centre, and spreading rapidly therefrom to the peritoneum, which, like a large curtain conveys the flame rapidly to all the other organs. This peritonitis occurs in a constitution impaired by many morbific causes, as must be that of a parturient woman. It is useless to enumerate them.

In fact, puerperal fever is a puerperal peritonitis; a local disease, due to a special cause, and modified by special causes.

M. Velpeau, terminates by alluding to his mode of treatment, which years ago, as now, was as follows: The use of mercurials in large and especially in frequent doses; calomel internally, and mercurial frictions externally, purgatives, baths; keeping a uniform temperature in the sick room, having a special and competent nurse for the patient, visiting her himself four or even six times a day. He has since derived great benefit from the application of enormous blisters, and says he has cured many by the above means used simultaneously. M. V. is aware that it would be exceedingly difficult to follow the above treatment in hospitals, but it can be used in private practice. He still believes it will be successful in puerperal fever, when there is no purulent absorption.

Acad. de Méd., June 8, 1858.

M. Cazeaux denied the correctness of M. Guérin's deductions, whose conclusions were totally wrong. They were drawn from his observations in twenty-one cases. Now, M. Cazeaux had, in one hundred and three cases made daily notes of the transverse and vertical diameters of the uterus after delivery, and did not, in a single instance, meet with what M. Guérin said *he* had found sixteen times in twenty-one. M. Guérin had evidently mistaken an effect for a cause. It was well known to obstetricians that nothing was more irregular than the contractions of the uterus after delivery. If we should adopt M. Guérin's views we would be continually spreading alarm in families, and foreseeing danger when none existed.

Acad. de Méd., June 15, 1858.

M. Depaul reviewed the opinions of those who had taken part in this discussion with the view of refuting them. From his remarks about M. Cazeaux, who views puerperal fever as being due to special changes in the blood of pregnant women, which changes, peculiar to the puerperal state, may be so much increased, during and after delivery, that the blood corpuscles will be converted into pus globules. M. Cazeaux cited M. Andral's authority in support of his views. M. Depaul said, that in our day, when the microscope has made us so well acquainted with leucæmia, it was doubtful whether M. Andral would endorse M. Cazeaux's views on the subject.

M. Depaul himself considers puerperal fever to be a primitive disease, with its own peculiar appearance and symptoms, and entirely distinct from metritis, peritonitis, and from the purulent and putrid infection which may occur after delivery.

With regard to its treatment, M. Depaul declares he has derived the greatest benefit from mercurials, and that he did not perceive any advantage resulted from the application of the enormous blisters so highly recommended by M. Velpeau.

But a more important question would arise, namely, its prevention. M. Depaul insists that peurperal fever is more frequent and severe in special hospitals than private practice, and considers that these results are due to the crowding together of women. Consequently, he considers that large lying-in hospitals are injudicious. He does not desire that they should be entirely abolished, but thinks that they might be advantageously replaced by small infirmaries at a distance from each other, in which but few parturient women should be admitted at a time, or by sending lying-in women, in small numbers, to general hospitals, and better than all would it be, to deliver the poorer classes in their own houses.—*Gaz. Hebdom. de Méd.*

ART. III.—*Puerperal Fever:* By M. JULES GUÉRIN. Translated by J. P. BARBOT, M. D. (*Gaz. Hebdom. de Méd. et de Chir.*, June 4, 1858.)

ON the 1st June, 1858, M. Jules Guérin, the distinguished editor of the *Gazette Médicale de Paris*, addressed the Academy on the subject of puerperal fever. We append a summary of his remarks:

Although not being an *accoucheur*, said M. Guérin, he could consequently pretend to no superior acquaintance with the subject under discussion, yet he had since 1840, been struck by the uncommon difference in the sequelæ of delivery in different women. Everything seemed to go on favorably with some and everything unfavorably with others. He sought to discover the cause of this, by studying the physiological condition of parturient women. In 1846, he made it the subject of serious study, in the lying-in wards of Dr. Louis, in in the Hôtel-Dieu. His conclusions are drawn from careful notes made in fifty cases. He had also studied the best treatises on obstetrics, and particularly those of Dubois, Velpeau, Cazeaux and Chailly, without being able to find what he sought for: *i. e.* the influence of uterine contractions on the sequelæ of delivery.

Whenever the uterine contractions occurred regularly, and the uterus had receded by the third day into the pelvic cavity, everything went on favorably; but whenever the contractions were slow or delayed, fevers, rigors, and the precursors of a puerperal affection occured. M. Guérin thinks that in the latter cases, the admission of atmospheric air into the uterus, and its action upon the coagula and fluids therein produced putrefaction and its consequences.

M. Guérin insists upon it, that whenever, after delivery, uterine contractions do not take place promptly, and that the uterus does not resume its normal position and nearly normal size, the consequences will be, that this viscus will become an open, suppurating wound, of a special *sui generis* nature, from the following causes:

1. The peculiar physiological conditions of the puerperal state.

2. The particular condition in which a puerperal woman is placed, by reason of the peculiar emanations from her body, which give rise to a species of *peculiar medical constitution*, which M. Guérin calls *infectious*. These toxic emanations are particularly active and malignant in localities where women are crowded together—where puerperal fever occurs so often as to be, in a manner, permanently located, and to have impregnated everything therein. When once this morbific influence is developed in these epidemic foci, it becomes as difficult for the female inhabitants to escape it as it is for individuals to escape cholera or typhoid fever in localities where the diseases are epidemic.

3. The peculiar conditions of the uterine wound, bathed by a special

fluid, composed of special elements, due to a special function, which becomes all at once suppressed and superseded by another special function, the mammary secretion.

4. Lastly, the air which enters into the uterine cavity is not removed, but becomes confined, corrupt and transforms the uterus into a cloaca.

From the above facts M. G. draws the conclusion that puerperal fever is a fever *sui generis*, produced by inertia of the uterus in parturient women, whereby the contractions not occuring promptly and energetically, the *placental wound*, instead of cicatrizing, suppurates, and this suppuration gives rise to the usual morbid condition of suppuration, namely, purulent absorption by the lymphatics and veins.

M. Guérin says that puerperal fever may be ordinary or epidemic; may be epidemic, produced by general miasmatic influences, or infectious and communicated by direct inoculation.

In the treatment of puerperal fever we should keep two main objects in view: 1. To favor immediate cicatrization of the uterine wound. 2. Bring back, as rapidly as possible, an uterine wound which shows a tendency to suppurate, to the state of the closed wound. Ergot is the best remedial substance for that purpose, and should be exhibited immediately after delivery when the uterine inertia seems disposed to continue. The remaining treatment should be based on general principles.

M. Guérin closes by declaring that a thorough study of puerperal fever, and its pathology, carries out what has been shown by statistics, to-wit, that lying-in hospitals are dangerous and destructive *(meurtrieres)* institutions, which shows the necessity of suppressing them entirely, whatever may be their form or name.

ART. IV.—*On the Value of Tonic Treatment in some Diseases of the Brain, more especially in cases of Ramollissement:* By FREDERICK C. SKEY, Esq., F. R. S., F. R. C. S., etc. Surgeon to St. Bartholomew's Hospital.

I wish to speak to-day of what is called "Ramollissement,"* or softening of the brain. I do not wish to speak of its pathology, I

* Roston did not believe "Ramollissement" to be the result of inflammation, but of a certain degeneration of the brain; especially as febrile symptoms and headache are absent.

know very little about that, as to whether it is inflammatory or febrile, or what not. All I pretend to know or to tell you is that the disease, as we see it, begins insidiously by loss of muscular power, and it occurs most frequently in men about the middle period of life; the gait or walk of such a person is unsteady, and seems natural to ask a surgeon what may be the cause of this unsteadiness or irregularity. A banker or a banker's clerk finds his style of writing changes; he has power, *quoad* power, he can use a dumb-bell, but he cannot regulate this power so as to write a letter, as he previously had done; his urinary system becomes affected, and his urine dribbles away, and even the rectum, from forgetfulness on the part of the patient, becomes partly paralytic; there is loss of memory or incoherence of ideas, small eccentricities appear. This man will spell some words badly; these are signs of recent cases: there is little or no implication of the reasoning powers, at least to any extent, but the loss of power, as in handling a pen to write, is most peculiar, as well as the irregularity of spelling of monosyllables badly or backwards in what is written. But if you wish for a more minute description of the disease you will find it in the works of Rostan and others. Now, these cases are common; this train of symptoms occurs in men who have undergone long anxiety in business, or otherwise; men of Parliament or the Stock Exchange, whose "all" may sometimes depend on some bold speculation, or on some cargo of goods at sea, or the like; or this disease will occur in men who have had exhausting fevers or other maladies; or again, in the case of a man who rides with hounds five days a week, four hundred miles a week, and it may be, drinks wine, eats very little, marries late in life, suffers from venereal exhaustion!—his nervous system becomes "broken down," as it is called. What is the condition of the brain then? Is it a condition of excess of vascular or vital force, or the opposite? Can any of you recall a case of "ramollissement" as it used to be treated a few years ago? Happily for yourselves, perhaps not; but the principal point was to keep always in mind "chronic inflammation," and to treat it accordingly. This poor gentleman must first be reduced, made to keep quiet, his diet regulated, his wine and fox hunting stopped, and three grains of grey powder with rhubarb, given at clock-work intervals, for what are called the "secretions," or to touch the gums for this chronic (?) inflammation. Next, his skin was steadily looked to, and that great catholicon of surgery lads, mindererus spirit, with antimony, was ordered, spoiling what little appetite the unfortunate patient may have had. He was rigidly confined to the house—but, mind you, with all this excellent drugging his speech does not improve; he progresses, but it is from bad to worse. Very well! Now that is one view—now for another. Mark that there is a slow pulse, everything is below par, as I call it. In this, then, "chronic inflammation," some people count on their ten fingers all the drugs I use or adopt. I am very glad of it, for we have too much routine and rubbish in what is called "general practice." The eyes of the public are upon us; are you then justified in lowering this man with your antimony, and your grey powder, and

your mindererus spirit? Oh no! But some Solon says you weaken the patient in order that he may get strong. In these cases I could never understand that kind of logic; believe me, if you wish to succeed in practice, you must give up such an idea; you must study nature a little more, and books and journals less. All the medical world of Europe is progressing; but we are still tied down to grey powder, and oceans of physic, and bleeding, whereas what is required is that we follow the vis medicatrix, and take advantage of the hints she affords us! Well, then, so much for that; now for a case of "ramollissement" as it is called. About two years ago a physician called on me; he said, "you are wanted down to So-and-so (150 miles in the country), Mr. So-and-so (a rich country nobleman) has forced a catheter through his urethra; the poor gentleman has got 'ramollissement,' you know that we are not so uneasy about; that is incurable of course; if you can do anything for it well and good; but his faculties are completely gone." Well, to make a rather long story short, Sir B. Brodie and I saw him, and a fortnight after he came up to town, to his residence in Belgrave-square, and I had nearly the entire management of the case.

It is exactly in this sphere of life, of rich noblemen, merchants, or political men in the fashionable West-end squares, that we can alone catch glimpses of these two opposites—viz., the excess of high living, and the excess of sangrado bleedings and starvation or low living of us the doctors! Many of these are probably "heart disease," and a patient dies of a fainting fit, called weak brain, but it is weak heart! Well, the more I came to look at this case of this gentleman, the more I said to myself, the man is dying of exhaustion; I noticed he was better after dinner; I heard that he had had convulsions; this did not frighten me. Now, I want to ask you a curious question; have you ever seen a sheep killed? If not, I would advise you the next time you are near Aldgate Market, just to look at the thing for yourselves. Just before all the blood is gone from the sheep, it is horribly convulsed; remember that fact also in weak children who are convulsed. Convulsions, in fact, as you will meet them in practice, are eight times out of ten the result of a very irritable state of the medulla oblongata or chord, which causes very slight irritations elsewhere to excite violent reflex or convulsive movements; thus, worms or indigestible food will cause convulsions where the nervous centres are weak or irritable. This condition of convulsions to my mind is almost always one of "exhaustion" rather than congestion; just mind that fact when you go into practice—convulsions, as caused by anæmia or "exhaustion."

But to go on with the case: I could not find that this gentleman had had any tonic treatment. I knew that if the brain be anæmic, it cannot go on long in a normal manner, for nine out of ten cases of "ramollissement," are due to anæmia; so I decided to let him go back to his old mode of living. I gave him a pint of claret a-day, that he was accustomed to, in place of water-gruel! He seemed to improve on it. The ratiocinations of his friends did not come true that it would kill him, so we let him have also quinine and iron in

place of leeches and water-gruel, and grey powder, antimony, and mindererus !

I studied the case for a short time; there was a manifest improvement every week. I was called one day; he was a little worse; did I bleed him ? No; I had the experiment with the sheep in my mind; he is a gentleman of very great eminence. It would have appeared very brilliant in a "bulletin," like the brilliant operations elsewhere, that we came up the fifty-ninth minute of the last hour of his sad existence and opened the carotid or temporal, but I did nothing of the kind: I increased his wine. Well, at the expiration of three months, that gentleman made a political speech that utterly astonished his constituents. He can now ride to the foxhounds as well as ever he did, and in the changes and chances of Parliament has filled a very important place; he is in fact to all intents and purposes cured !

Now, a few words on another case. I was called to see another gentleman, who, I was told, was attacked with "epileptic or some fits" every fortnight. I found that they were not perhaps epileptic, as he was never incoherent or deprived of consciousness.

He was a "*bon vivant*,' as many of these patients are. The symptoms came on two years previously; his pulse was all along small and weak, quite incompetent to the work, as I thought, of supplying a large and active brain. His doctor had ordered him, as a great stretch of the roborant plan, two wine-glasses of claret at dinner, mixed with water, and pump water, "usque ad nauseam," the rest of the day. How can you prevent or cure disease on such trash as that? Mind you, he was a "*bon vivant*," and had now come to believe that the cardinal point of his cure was rest and starvation. Well, I ordered him the first day a thing he very much wished for—two rattling tumblers of Bass's best ale per diem, and other treatment in accordance with that plan; he got better. His lady sent for me, however, one night, and I met her on the stairs looking very dolorous indeed. I thought he was dying, or dead; but she said, with a solemn face, "he took advantage of our prescription, what *will* become of him? He has taken to-day seven tumblers of ale !" "The deuce he did; but he is better of it. I am delighted," I said ; and it really did him no harm but good ; he had ridden twelve miles, and was tired, and quenched his thirst in Bass's ale accordingly ! Now, that gentleman soon lost his fits, or at least he had a slight fit once in three months in place of once every fortnight. He got on most famously under the strengthening plan, as in the former instance; but in an evil hour he went down to the country in the summer, and on the return of his "fit" the next to hand surgeon, with a red lamp, was sent for, who bled him, gave him the orthodox doses of calomel and colocynth, followed up by—and—yes—but he never breathed again !

I have had now nine or ten of these cases of ramollissement; they all have had slow pulse—a condition always improved by tonics. The heart is perhaps at the root of the disease rather than the brain ; some of these patients had alarming syncope—that is, heart, depend on it, not brain. I met Dr. Latham and Dr. Ferguson with one case, and we had a good deal to do to give force to the heart and pulse.

Now, I do not want at all to say—that in some of these very cases we may not have had "ramollissement," I merely contend for the position, that leeches, oceans of physic, and starvation, are not the proper remedies. I will only say a few words relative to another case, which was seen by three of our ablest physicians in London—two pronounced it "ramollissement," and the third "tubercle." I think tubercle in the brain is a very rare disease in adults; this patient had excessively weak pulse; he had married late in life, and in many other particulars he was the exact counterpart of the case already given; he was rather forced on me as to treatment. I gave him wine and the ferrocitrate of quinine in large doses—a remedy I have great faith in. Well, in three months he was quite recovered. I have said already I do not believe this disease to be of the nature of inflammation; with heat, pain, redness, swelling, etc., it strikes me as rather of the nature of gangrene, and as arising from anæmia, not hyperæmia; this last gentleman, I ought to say, had an issue ordered for him in Dublin. Well, I have no objection to an issue in these cases, nor am I frightened at stopping an issue. There is something of the fabulous about what is written and taught in lectures as to stopping issues. This gentleman's issue healed up, or rather I took off the plaster, and never had that abiding faith in its efficacy that would induce me to put it on again. In the earlier stages an issue may do good; it can at least do no positive harm, like other things which have had more than a questionable character, as specifics for "ramollissement;" indeed, specifics so called, which unquestionably have hurried many patients to their graves, and which I would implore you to consider well in all their bearings before you adopt them. *Dublin Hosp. Gaz.*, March 15, 1858.

ART. V.—*On Matters of Novelty or General Interest, as at present exhibited in the Practice of the Hospitals of Paris:* By GEO. SUCKLEY, M. D., late Assistant Surgeon, U. S. Army.

[THE following extracts are taken from a long and interesting communication dated at Paris, March 27, 1858, and published in the *N. Y. Jour. Med.*, for July, 1858.]

The Parisian journals of medicine, like those of all other countries, are constantly filled with new projects and methods of treating disease, which, although backed up by successful statistics, real or apparent, and the publication of isolated cases, merely live their day, and are shortly after forgotten. I do not purpose to take up all the novelties in medical treatment which have been advocated during the past winter: *Foremost among the new things of the day, is the revival of Laennec's antimonial treatment of chorea.* The novelty of the renewal consists in the exaggeration of the plan, and the heroic doses administered.

To Mons. Gillette, of the Hôpital des Enfans Malades, is due the credit of the renewal of the antimonial treatment, which is now pursued as follows, for children say eight years of age: The first day twenty centigrammes (about fifteen grains) of antim. tart. is given; on the second, twenty-five; on the third day, thirty. These quantities are dissolved in about three ounces of gum-water, and commence to be given to the patient, fasting, at an early hour of the morning, at the rate of a tablespoonful an hour, until it is all taken. During the administration of the doses no solid food is allowed, but the patient may take a little clear broth; the patient is also kept on his back while taking the medicine, but in the afternoons is allowed to get up and eat the usual hospital diet. If this course has not been sufficient, you will wait for the space of four days before recommencing the treatment, giving them forty centigrammes (about seven grains) of the remedy, in the same way, on the first day, and increasing the dose five centigrammes (nearly one grain), for each of the two succeeding days, observing the same regulations as before. If this does not cure, you again wait four or five days, and then commence with fifty-five centigrammes, augmenting the quantity daily, as before, and following the same rules. If the drug operates too much on the intestines, add a little laudanum to the solution. If these trials do not effect a cure, or some radical amelioration, the treatment by tartar emetic should be abandoned.

Blanche, at the Hôpital des Enfans Malades, until recently, relied upon the shampooing process in treating children for chorea. When employing this latter method the average time of cure was twenty-five days. Blanche now has, to a certain extent, abandoned the shampooing treatment for the antimonial; but not entirely, as a very obstinate case has lately resisted the complete exhibition of the antimonial treatment, in which he has been obliged to return to the shampooing process. This is nothing more than an addition to the mountain of proof that we cannot obtain specifics in medicine.

Becquerel does not seem to think much of the foregoing plan, as shortly after it had been publicly proposed, I saw a case of severe acute chorea in his wards, which he treated in the common sense way by following the indications. The case was that of a young girl, who, through cold, had a sudden suppression of the menses. Chorea manifested itself immediately, in a most aggravated form. In addition to the shower bath and cold douche, which are his "sheet anchors," in this complaint, in the view of the obvious cause of the malady, a vicarious flow was induced by leeches, a large number of which were applied. I saw the patient a few days after, when she was rapidly recovering.

Trousseau's favorite method of treating chorea is by the administration of the sulphate of strychnine, in the following manner: ℞ Sulph. strychniæ, centigrammes five; syrup. simpl. grammes one hundred. Misce. Cap. coch. mag. ter in die.

Dr. Nathaniel Miller, of Providence, Rhode Island, now in Paris, informs me that he has seen a case of aggravated chorea, which had completely resisted Trousseau's treatment—carried on until the pecu-

liar poisonous effects of strychnine had become dangerously manifested, rapidly cured by the antimonial treatment.

Mons. Briquet has lately advocated the electric treatment for lead colic. He has demonstrated, very conclusively, that the seat of the disease is in the muscular parietes of the abdomen, and not in the intestine. By his treatment, the pain ceases after every application of the agent, leaving the patient comfortable for an hour or longer. In the mean while, the ordinary treatment, for the elimination of the lead, can be employed. During the application of the electricity, the pains are greatly increased, but soon subside. Becquerel does not adopt the plan, but relies on the usual treatment by sulphuric acid and evacuants.

A writer in the Gazette des Hôpitaux of December 15th, 1857, giving the results of the so-called "purgative treatment" in the typhoid fever of children, as shown by the practice of M. Beau, makes use of the following strong language in opening his subject: "The purgative method, indubitably the best for the adult," (!) "should it be used in typhoid fever of infants (children)?" If the purgative method* is "indubitably the best for the adult" suffering from this disease (a theory which I think very few American physicians will admit), it is not relied upon by Becquerel, who prescribes but little for this disorder except good nursing; although, in rare cases, where there are severe local complications, he occasionally bleeds a little. Bouillaud, at the Charité, treats nearly all cases of this disease by bleeding.

The employment of mercury, in the Parisian hospitals, is almost entirely confined to its use as a cathartic, in which case very small doses of calomel are given, mixed usually in powdered white sugar. Except in the treatment of specific venereal diseases, the administration of mercury, to obtain its specific alterative effect, is almost entirely confined, in the hospitals of Paris, to the treatment of puerperal peritonitis and congestion of the liver.

Several points in the general treatment in these hospitals are well worth noticing. Foremost is the prevalence of the "let alone treatment," unless there is a strong indication to the contrary. This seems to be particularly the case with Becquerel. In his service, also, whenever a patient is bled, the blood is analyzed; if more than the normal quantity of fibrine is found, the bleeding is repeated, and again, and again, if the superabundance of fibrine continues.

A short time ago, I witnessed Becquerel apply the actual cautery to several cases of ulcerations of the os uteri. The agency of the electrical cautery apparatus was employed; and I noticed that he took pains to connect the wires, and heat the metal of the cautery, *outside* of the vagina. At first, when using the electrical cautery, he was in the habit of heating the iron near to, or directly upon the surface to which he was to apply it; but finding that even in the comparatively short time he had been thus using the agent, that two cases of metro-peritonitis had occurred, he deemed it best to employ the

* The "purgative method" of Larroque is the one employed. It consists in the administration of an antimonial emetic at first; which is followed afterwards by salts, repeated continually for three or four weeks, in sufficient quantities to produce four or five evacuations daily!!

iron already made hot before the introduction into the vagina, in the same manner precisely that the common actual cautery is used.* The same physician, in ordinary vaginal examinations, makes use of the *tri-valve* speculum by preference.

In surgery I have seen but little new since writing my former article. In that paper I mentioned Maisonneuve's method of amputating with the *écraseur*, and stated that the object sought by this mode is to lessen the danger of phlebitis and "purulent absorption." Judging from the success following the removal of hemorrhoidal tumors, etc., by this instrument, and the comparative infrequency of phlebitis as a consequence, it was but fair to suppose that, in hospitals where all operations with the knife had been unsuccessful from that very cause, there must be something more than mere accident to occasion such a manifest difference where the écraseur is used. Thus far, Maisonneuve has amputated ten times in this manner, as follows:—two arms, two forearms, one thigh, and five legs. Of this number there have been two deaths; but on a severely exact post-mortem examination, no traces of purulent absorption or phlebitis could be detected.

The case of amputation of the thigh formed one of the two fatal cases. It is a pity that, in the view of the experiment, instituted there was not a larger portion of amputations of the *thigh*, for comparison and examination. Since the means used at first for breaking the bones have been improved and altered, and brought to their present perfection, the stumps following the operation are very good.

The case of injection of iodine into the knee-joint, for the cure of chronic synovitis, reported by me heretofore, was considered *cured* twenty-eight days after the operation. Several similar operations have been performed in the different hospitals within the past two months; and I have heard of one case in which both knees were injected.

Chassaignac has recently amputated the neck of the os uteri with the écraseur. I witnessed two of these operations, which were readily performed, and accompanied by very little hemorrhage, perhaps not more than two teaspoonfuls each. The ultimate results of these cases I have not yet ascertained.

The treatment of fractures is so faulty in Paris, that, to do the subject justice, it would require more space and time than can be at present spared. For some years past, all attempts to apply the extension treatment to a fractured thigh have been abandoned, for the reason that all the methods known to the French surgeons were liable, if extension be kept up, to be followed with sloughs of the ankle, instep, etc. The plan of making extension by adhesive plaster bands, and the "straight apparatus," until March, 1858, had not reached the "focus of medical knowledge," although it has been in successful practice for over seven years in the United States.

* A single case of metro-peritonitis had also occurred in a vast number of instances in which the common cautery had been used. Whether this was the *only* case that has occurred in Becquerel's wards, or the only case which has occurred during my informant's connection in Becquerel's service, I do not now remember.

The treatment usually employed here for *ununited* fractures, is by the seton. Occasionally, however, cures have been effected by exsecting the ends of the fractured bones, and then dissecting up for a short distance, the periosteum from each fragment, and invaginating, as it were, the portions of the membrane towards each other.

In Ricord's hospital, Hôpital du Midi, there are at present the usual number of afflicted. I learn, from the chef de clinique, M. Poisson, that since the last edition of Ricord's letters, a point has arisen to notice, which somewhat staggers the previous theories of that eminent specialist, concerning the convertibility or non-convertibility, of his two divisions of chancres; this is the fact, that chancres on the "face" (lips)? are *almost always* of the *hard* variety. To settle this point, a vast number of experiments would have to be instituted, which are precluded by the cruelty of submitting the subjects to such a risk, as well as the great danger of legal processes afterwards. Ricord's treatment for indolent non-suppurating scrofulous buboes of the groin, is to touch the surface in points all over the enlarged gland, with a red hot iron—each point burnt being of about the size of a pea.

The "Annales d'hygiène publique et de médecine légale" of January, 1858, contains a paper by Mons. Ambroise Tardieu, Physician to the Lariboisière Hospital, on the medico-legal bearings of the crime of "*pédérastie.*" His work contains a record of the alarming and frightful extent of this crime in Paris, as developed by the examination of two hundred and five individuals either actively or passively addicted to the vice, together with remarks upon its effects upon the health—its diagnostic signs, and its bearing as a contingent of other crimes.

Dr. Tardieu is the chief medical examiner to the police in cases where expert testimony is required concerning rapes, etc., and is also a man of sufficient standing in his profession to have been appointed visiting physician to the most new and beautiful hospital in Paris. The disgusting details through which he has had to wade in making this report, have been manfully met. Actuated by devotion to science, and a sense of duty, he has written a paper singular in the extreme, upon a crime happily almost unknown in America. I have mentioned the existence of this report, for the benefit of those pursuing medico-legal studies, and in the words of the reviewer of the work, my excuse is—"La science est comme le feu, elle purifie tout ce qu'elle touche."

ART. VI.—*Analysis of fifty-two cases of Epilepsy observed by the author:* By DR. SIEVEKING, Physician to St. Mary's Hospital. (Proceedings of the Royal Med.-Chir. Society, vol. i. No. 3, 1857.)

THE fifty-two cases analyzed had occurred exclusively under the author's own observation, and the conclusions were limited to points with reference to which satisfactory evidence could be obtained.

Sex.—Twenty-four were females, 46.15 per cent.; twenty-eight were males, 53.84 per cent.

Age.—The following is the distribution of the cases throughout the different periods of life: Under ten years, seventeen; from ten to twenty, nineteen; twenty to thirty, four; thirty to forty, four; forty to fifty, seven; over fifty, one; or from infancy to the age of twenty years inclusive, 69.23 per cent.; from twenty-one to forty years inclusive, 15.38 per cent.; from forty-one to fifty-five years inclusive, 15.38 per cent. Arranged according to sex, we find during the first decennium, eight males and nine females; during the second, twelve males and seven females; during the third, two males and two females; during the fourth, the same number of each; during the fifth, two males and three females; during the sixth, one female. The male sex, during puberty, therefore, seems to exhibit a more marked proclivity to epilepsy than the female; at later periods the ratio returns to the equality shown to prevail during the first ten years of life.

Causes.—Hereditary tendency could be traced only in six cases, or 11.1 per cent. A definite cause was assigned by the patient or the patient's friends in sixteen cases, or nearly one-third of the whole. Amongst these, otorrhœa is mentioned twice; fright, twice; injury to the head, twice. The cases differ in the relation they bear to the occurrence of the seizure.

Preminotory symptoms.—The occurrence of an "aura" is a point on which observers have expressed different opinions. Comprising under this term all the premonitory symptoms indicating the approach of a fit, it is noted in twenty-seven out of fifty-two cases; the most common was a sense of giddiness, and impairment of vision; sometimes the patient suffered pain in a definite region of the body, or, though unable to explain the sensation, was aware of some change, from which they knew that a paroxysm was about to take place. The sensation was never described as a puff of wind or aura in its verbal sense.

Individual symptoms.—Headache is a very frequent concomitant of epilepsy. It was observed in thirty-three out of fifty-two cases, or 63.42 per cent. The mode of its occurrence varies; the patient either suffers habitually or very frequently from it, and the symptom bears no immediate relation to the paroxysm; or the headache occurs shortly before the fits, so as to usher them in; or, again, it affects the individual after they are over. It was constant or frequent in 36.5 per cent.; it occurred before the fits only in 7.7 per cent.; it occurred after the fits only in 17.3 per cent. Biting the tongue is justly regarded as an important corroborative symptom; but it is by no means uniformly present, nor does it constantly occur in the different paroxysms affecting the same individual. The tongue was bitten in seventeen cases, or 32.7 per cent. The urine was tested for albumen in nineteen cases, and it was found temporarily present in one, permanently in one. It was also tested for sugar in fourteen cases, and this ingredient was not found once—a result which seems irreconcilable with the observations of Dr. Goolden.

Results of treatment.—The author ventured to express a feeling of

scepticism with regard to the positive certainty of any cure of epilepsy. He believed that, in the majority, no organic lesion, in the ordinary anatomical sense of the word, is present in the commencement of the disease, and that, in a large number, none seems to result from the recurrence of the fits. It appears that a diathesis is necessary to its occurrence, and that this may be suppressed or held in check; but whether it may be eradicated, is a question which he would not venture to answer in the affirmative. He was satisfied of the power of well-selected remedies in repressing, and often indefinitely postponing the paroxysm, and he particularly insisted on the importance of dietetic and regiminal treatment. The number of apparent cures was fifteen, or 28.85 per cent.; in other instances, more or less benefit was obtained. The duration of the disease before treatment is commenced has an obvious influence over its curability. Eight of the fifteen (apparent) cures were wrought in cases that had lasted one year or under, four were of two years' duration, one of three, one of six, and one of eight years. The treatment adopted had varied with the nature of the constitutional affection in each case; but he was able to draw this general inference, that the main indications which should guide us, are to remove local irritation by counter-irritants, to promote the healthy action of the secernent organs, and to give a tone to the constitution by vegetable and metallic roborants. The author expressed his belief that there was no specific for epilepsy; the salts of zinc certainly fail to remove it in many cases.

In a postscript he detailed the results of an analysis of the Returns of the Registrar-general with reference to sex in deaths from epilepsy during seven years; which gave six thousand seven hundred and twenty-nine males, and six thousand one hundred and forty-nine in females, or 52.26 of the one sex to 47.73 of the other.—*Ranking's Abs. Dec.* 1857.

ART. VII.—*The Phenomena of Spinal Irritation, and other functional diseases of the Nervous System:* By THOMAS INMAN, M. D., Physician to the Northern Hospital, Liverpool, and Lecturer on Medicine at the Royal Infirmary School of Medicine. (8vo., Churchill, 1857, pp. 201.)

WHAT Dr. Inman proposes to show in this volume is—

1. That the symptoms attributable to "spinal irritation" have nothing to do with the spinal cord, or the nerves arising from it.

2. That the majority, if not the whole of them, are due essentially to the same cause which produces the spinal tenderness.

3. That the spinal tenderness results from overstraining of the fibrous origins of the muscles attached to the spinous processes.

4. That the spinal tenderness is analogous to that experienced at the origin and insertion of muscles in other parts.

5. That the weaker the individual is, the greater is the tendency to fibrous pain.

6. That the most common causes of the pain and tenderness, in any part of the muscles, are constitutional or acquired debility.

7. That debility increases equally the irritability of the muscular and the nervous system.

8. That before hysteria can manifest its presence there must be debility from some cause or other.

9. That debility may show itself in the muscular or nervous system, or both.

10. That debility affects the nervous system as a whole or in sections—*i. e.*, mental, sensitive, motor, organic.

11. That functional affections in any one or more of these parts have long been recognized as emanating from deficient vital power.

12. That anything which deteriorates the vital power has a direct tendency to aggravate the complaints referred to.

13. That the muscular and nervous irritability are subject to the same laws, and that the remarks applicable to the one are, *mutatis mutandis*, applicable to the other.

14. That the link connecting hysteria with spinal disorders is constitutional or acquired debility.

15. That, as regards curious mental phenomena, excess of sensibility in the nerves of common or special sensation, a propensity to spasmodic actions and to irregular organic phenomena, there is no essential distinction, they are simply different facets of the same die.

16. That the essential distinction between genuine hysterical and muscular affections is, that a large amount of bodily rest is necessary for the cure of the latter, while it is not so absolutely requisite for the former.

17. That, for the future, it will be necessary to discriminate between pain arising from muscular fatigue, cramp, or fibrous stretching and genuine neuralgia, and that there will be neither precision in diction nor a clear idea of treatment until the distinction is made.

These are the principal propositions which are set forth in the work before us, and to the establishment of which Dr. Inman adduces evidence which must be allowed by every one whose prejudices will not interfere with the fair exercise of his reasoning powers. At any rate the evidence appears to be sufficiently cogent to us.—*Ranking's Abs., Dec.*, 1857.

Art. VIII.—*Summary of the Transactions of the Philadelphia Biological Society:* Reported by Henry Hartshorne, M. D., Recording Secretary.

Jan. 18th, 1858. Dr. Wm. A. Hammond read a paper "*On the Injection of Urea and other substances into the blood;*"* giving an account of

* See N. Amer. Medico-Chirurg. Review, March, 1858.

several series of experiments instituted in order to determine the correctness of Frerich's explanation of uræmic intoxication, by the conversion of urea into carbonate of ammonia, and resulting, among other conclusions, in the opinion, that this theory fails to be sustained, and that the carbonate of ammonia is not, itself, more poisonous than urea.

In the brief discussion which ensued, the fact, mentioned in the paper, of the non-appearance of ammonia in the breath after the injection into the blood of urea mixed with vesical mucus, was noted, as being contrary to expectation based on other facts. Dr. Hammond explained that, in his view, the conversion of urea into carbonate of ammonia, which occurs in the presence of mucus out of the body, will not take place in living blood.

Dr. S. W. Mitchell remarked that *healthy* mucus has not been found to hasten this decomposition of urea; but that, out of the body, urea will, without any ferment, undergo spontaneous conversion.

Reduction of temperature by depletion.—Dr. S. W. Mitchell mentioned that, having occasion, recently, in a case of insanity, to take from a patient a very large amount of blood—one hundred and eighty-nine fluid ounces—in a short space of time, he observed that the temperature of the blood underwent a marked decrease with each depletion. Being, at the first, 100° Fahr., it fell by degress to 96.2°. During subsequent convalescence it again gradually rose, being at the time of the last examination, 98.7°.

Intestinal absorption.—Dr. Joseph Leidy made the following remarks: The observation of Goodsir, that the epithelium of the intestine appears frequently to be thrown off in laminæ, exposing the basement membrane so as to absorb directly, has been generally denied by others; but the true mode of shedding the columnar epithelium of the intestine has not yet been demonstrated. In examining the intestinal canal of insects, as flies, beetles, grass-hoppers, etc., the structure of which, although very delicate, is essentially the same as that of the intestine of higher animals having a columnar epithelium upon a basement membrane, and beneath this a muscular tunic—on placing the mucous membrane upon the object-glass of a microscope while yet living, Dr. Leidy observed that, when a little water was added, endosmose taking place, the oldest and thinnest cells received the fluid most rapidly, while these were pushed out by the presence of others, thus squeezing the liquid through them. It is, therefore, probable that the epithelium is shed *cell by cell*, the oldest cells being crowded out by the new ones forming beneath. This can be seen by any one under the microscope in the intestine of the common house-fly. The nutritious matter in contact with the epithelial cells passes into them by endosmose, and thence through the basement membrane into the subjacent vessels. Dr. Leidy has never seen the epithelium stripped off in layers, although he has often watched the process in mice, serpents, salamanders, and frogs, in all of which the mucous membrane has a similar structure. He has, however, very often observed the new cells pushing themselves between the old ones, altering their shape,—the round ones being seen at the bottom, and the elongated oval or pear-shaped ones above.

Feb. 1*st*. *Oxaluria.*—Dr. J. Cheston Morris remarked that it is only within the last thirty-five years that the occasional occurrence of oxalate of lime in the urine has been noticed. At first it was only surmised to exist in this excretion from the fact that calculi were often found to consist partly of it. But soon after the perfection of the microscope, and its general employment in clinical investigation, octohedral crystals of various sizes and great brilliancy were observed, which were proved by chemical investigation to consist wholly of this salt. The discovery of the nature of these crystals would probably have been made earlier, had not observers been led astray by the fact that oxalate of lime, when precipitated ordinarily from solutions of lime-salts by oxalate of ammonia, is amorphous. Lehmann, however, states that when extremely dilute solutions are used, and the precipitate is examined with high powers, minute octohedra may be distinguished. It is obvious, however, that in physiological chemistry some circumstance must exist, modifying the mode of formation of this most insoluble salt, as we find it occurring in quite large and beautiful crystals, as the in cells of various plants, and even in the urine of man and other mammalia. This modifying circumstance I take to be the extremely gradual formation of oxalic acid in the course of the chemical changes constantly taking place in all organic fluids. The formation of oxalic acid is always due to the partial oxidation of the substance when it is procured, whether in the processes of nature, in the vegetable and animal kingdoms, or in our laboratories by the action of nitric acid on sugar. Probably no other substance has undergone so rapid a change of opinion as to its pathological importance. Regarded at first as a sure fore-runner of mulberry calculus, it was supposed to depend upon a peculiar diathesis as its cause. Then it was supposed to be due to the consumption of vegetables containing oxalates; a view refuted by the utter insolubility of oxalate of lime in any of the fluids of the body, and the consequent impossibility of its penetrating the blood-vessels—yet supported by Wilson and Donné. It has been found to be increased by the use of carbonated liquors and of nitrogenous food; after epileptic convulsions, in chorea, chronic bronchitis, typhus fever, spermatorrhœa, and dyspepsia. In these diseases, we have either a disturbed respiration or an excessive retrograde tissue-metamorphosis; and to these we must look to discover the cause of the appearance of oxalate of lime in the urine. The changes which the substances introduced into the animal system undergo, are those of progressive oxidation; hence it is easily seen that a diminished supply of oxygen, or an increased waste of tissue, would be accompanied by an imperfect oxidation.

But from being regarded as a pathological constituent of high importance, oxalate of lime has now come to be considered an almost normal substance in the urine. It may be found in almost any specimen of urine which has been allowed to stand until the acid fermentation has occurred. Dr. Golding Bird believes the cause of this to be its deposition from a sort of solution in the urine, an opinion in which I cannot concur, as proof is wanting that it is soluble in *any*

urine. It is well known that urine undergoes an acid fermentation, commencing soon after its expulsion from the bladder, and continuing for some days, during which uric and oxalic acids are formed; after a time, however, a second fermentation occurs, in which the urea is converted into carbonate of ammonia. What explanation can we give of the occurrence of these apparently opposite phenomena in the same liquid under the same circumstances? The ferment which acts in both cases is probably the vesical mucus; and the substance changed must be either this mucus, the coloring matter, or the creatin and creatinin, as these are the only other nitrogenized substances present. That it cannot be the mucus is proved by filtering the urine, after which it still ferments, though more slowly. That it is connected with the presence of creatin may be inferred from the fact that Dr. Miltenberger and I (Med. Examiner, 1854) have found crystals of creatin formed by spontaneous evaporation in microscopic specimens of urine containing oxalate of lime. If we take $C_8 H_9 N_3 O_4$ as the composition of creatin, two equivalents of the latter, with fourteen of the oxgen would yield one equiv. of uric acid, one of urea, two of oxalic acid, and eight of water, thus:

$$2 (C_8 H_9 N_3 O_4) + 14 O = C_{10} H_4 N_4 O_6 + C_2 H_4 N_2 O_2 + 2C_2 O_3 + 8HO.$$

I suppose some such reäction as this to take place as long as any creat n remains in the urine, thus explaining the acid fermentation; when this has ceased to occur, the urea decomposes into carbonate of ammonia. This will also explain the crystallization of the oxalate; for, as this is a slow process, the oxalic acid is very slowly presented to the lime salts of the urine, thus fulfilling the condition given by Lehmann as necessary for the formation of the crystals. Prof. Jones, of Georgia, succeeded in obtaining them by endosmose, to which he attributes this formation; but this is obviously only another method of *very gradually* presenting the oxalic acid to the lime, the main condition requisite for their formation.

Dr. Hammond observed, that he had frequently found oxalate of lime in the urine, in his own person and that of others, during *health;* large crystals, both of the dumb-bell and octohedral forms being presented. Dr. Hammond does not credit the existence of an "oxalate of lime disease," nor of a diathesis, such as is described by some English pathologists, characterized by excess of urea.

Feb. 15th. Dr. T. G. Richardson read an elaborate paper by Dr. George Patin, of Galt, Canada West, upon the *Functions of the Spinal Cord, as illustrated by experiments on cold-blooded animals;** endeavoring to show occasion for some modification of the theory of reflex action of Marshall Hall, and for the opinion that *perception* is one of the attributes of the spinal cord, and especially of the medulla oblongata.

Dr. Leidy remarked that the experiments narrated in the paper did not appear to him entirely conclusive, as the movements described might be automatic. This was the view taken by Dr. Leidy of the results observed by him in analogous experiments made several years since, upon frogs, flies, etc. He believed that the conveyance of

* See N. Amer. Medico-Chirurg. Review, May, 1858.

impressions, in insects, for instance, to the chain of ventral ganglia, should be expected, without supposing perception to produce the apparently voluntary movements. While experimenting at one time upon pigeons, Dr. Leidy kept one alive, after the removal of the cerebrum, for nearly two months. It spent much of the day in walking up and down the room, but never passed through the open door into the room adjoining, the dark shade of which appeared to have the effect, through the retina, of a bar or obstacle. All its motions seemed, to Dr. Leidy, to be automatic and unreasoning; approaching the fire, for example, for warmth, but getting so near as to burn itself, having, repeatedly, to be picked out of the ash-pan. Acts are often set down as evincing consciousness, which are realy automatic, such as the darting of a frog at a bright object, or of a fly against a window-pane. The same act will be repeated a thousand times in the same way, without learning by the experiment, it being the mere result of an *impression* on the organs of sense.

Mr. W. Parker Foulke suggested an inquiry, in regard to the somewhat complex and determinate movements of newly-born animals, which could not possibly have learned by experience how to perform such acts; if these were automatic, how do they differ from those described in the paper of Dr. Paton? Moreover, in using the term "perceptive act," does Dr. Paton mean that the animal is believed to be *conscious* of the impression through the spinal cord, and to act voluntarily?

Dr. Richardson considered that this was his meaning, and urged, that in some of the experiments related in the paper, sensibility appeared to be shown by the *purposive* character of the movements made. Thus, when an animal, whose brain has been removed, is lightly touched about the neck, and makes a special movement to brush off the irritant, this motion is only one of many movements possible to the same muscles, and one of the *least frequent* or natural to them. We must therefore, infer sensibility to account for this determinate movement, instead of the more simple and uniform contraction, such as reflex action produces.

Dr. H. Hartshorne believed that different *modes* or *degrees* of irritation of the same surface might produce variable contractions of muscles, under reflex action alone.

Dr. Leidy confirmed this opinion, by examples, in the movements of frogs, flies, etc., from which the brain had been removed.

Dr. Richardson:—Do not these examples really show the possession of sensibility by the cord or ventral ganglia, for which the author of the paper contends?

Dr. H. Hartshorne considered that the statement of the paper, that reflex actions are always uniform under impressions upon the same part, is true only when the impressions are like in degree, and are conveyed by the same nerve-fibres, to the same central ganglia. Thus, the impression of light upon the retina, which, by one reflex circuit causes the contraction of the pupil, will, by another, induce a more general movement of muscles, or the act of sneezing, or the flow of tears. Moreover, a purposive or determinate character in move-

ments does not remove them from the category of automatic actions. Thus, for instance, those called instinctive in lower animals, and others, in the higher, designated as sensori-motor by some physiologists, are still *reflex*, that is, dependent on an *impression* from without, acting through a complex nervous apparatus. Dr. Hartshorne has not, in fact, seen satisfactory evidence of the existence of *will* in any animal except man,—the actions of all the others being explicable upon the idea of more or less complex automatism; and, where the subject is so obscure, it is most philosophical to assume only the one idea or supposition, which will account for the facts, rather than two, as we must do when, in such experiments as those alluded to, we suppose consciousness and will to exist.

Dr. S. W. Mitchell observed that many very similar experiments to those of Dr. Paton had been performed and recorded by Dr. Dowler, of New Orleans; one of which was more extraordinary than any narrated in Dr. Paton's paper; in which an alligator, whose brain, spinal marrow, and viscera had been removed, yet exhibited movements apparently as intelligently guided as those common to the animal in its natural state.

Dr. Hammond had seen a rattlesnake, the head of which had been cut off, and its skin and viscera removed, yet coil itself up and strike.

Dr. Hammond then read a paper *On the alterations produced by Intermittent Fever in the excretion of urine, and on the action of the Disulphate of Quinine;** tending to show, so far as one case, carefully observed, can prove, that the excretion of uric and phosphoric acids is increased during the paroxysm, and that this excess disappears under the use of quinine.

Dr. H. Hartshorne made some remarks *On the best means of advancing Biological Science at the present time*, adverting to the danger, which he considered a principal one in the science of the present century, of *logismophobia*, *i. e.* the dread of reasoning, as opposed to simple observation, and expressing the view, that, in every scheme of scientific inquiry, there should be, after the model of Bacon's "New Atalantis," room, not only for observers and experimenters, but also for "depredators," "compilers," and "interpreters of nature." Dr. Hartshorne made an appeal, also, in favor of the organization of a system of *joint investigation*, by observation, experiment and otherwise, by members volunteering for the purpose, in connection with important questions or problems, illustrating this proposition by a series of questions, which might be found capable of determination in such a mode.

Dr. Leidy, Dr. Richardson and others expressed their approbation of this proposal for organized investigation.

Dr. Hammond, in connection with it, gave an account of the German "Verein für gemeinschaftliche Arbeiten," which has been in existence for several years, numbering many hundreds of members, on the Continent and in Great Britain, and issuing, in its archives, a remarkable number of interesting and valuable productions.

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* See Amer. Journ. of Med. Sciences, April, 1858.

March 1*st.* Dr. Walter F. Atlee presented a preparation of an acephalous child, and read a paper, giving a description of the monstrosity.*

Dr. Leidy described a similar one, having neither head nor arms, now in the Museum of the University of Pennsylvania.

Dr. J. C. Morris inquired whether the state of the placenta had been examined into in either of those cases? Dr. Morris had noticed, that in several instances of acephalous fœtus, fatty degeneration of the placenta had occurred. Dr. M. had also attended a patient, who was delivered at full term of a fœtus, which had died at the 7th month; fatty degeneration of the placenta was found to be complete. The same mother was afterwards delivered of a child at full term, but which was not more developed than is usual at the 7th month. It has been imagined by some, that fatty degeneration of the placenta at the end of pregnancy, may be the cause of labor; but such a case as that just mentioned, tends to disprove this.

Dr. Leidy remarked, that the condition of the placenta could not account for such a monstrosity as the one exhibited by Dr. Atlee; as this, from its nature, must have been determined in the embryo: having no upper extremities, as well as no head. The upper extremities always pullulate before the formation of the placenta. In this case, also, the placenta must have been healthy enough to furnish nutriment for all parts of the body, which were present.—*Proceed. Acad. Nat. Sci.; Biological Dept.*

ART. IX.—*Infant Mortality in large cities:*

D. Meredith Reese, M. D., LL. D., of New York, editor of the *American Medical Gazette*, Vice President of the *American Med. Association*, etc., presented an interesting paper at the last meeting of the *American Medical Association*, (1857) and published in its *Transactions*, entitled *Report on Infant Mortality in large cities, the sources of its increase, and means of diminution*, from which the following extracts are taken:

First. By "infant mortality" we understand the *mortality of childhood*, or, in other words, that fearful item in our official bills of mortality and reports of interments, which records the appalling fact that nearly ONE-HALF of the *whole number of deaths*, especially in large cities, occur in infancy, and before the subjects have reached the *fifth anniversary of their birth.* In the city of New York, as will appear by the accompanying statistical table, the whole mortality of the last half century amounted to 363,242 (including the still-born) while

* See Am. Journ. of Med. Sciences, April, 1858.

the number of deaths *under five years of age* are shown by the same table to have been 176,043, which is nearly forty-nine per cent. of the entire mortality of the city, and this for fifty consecutive years. As in New York, so in other large cities, the proportion of infant mortality will be found to vary inconsiderably if at all, so that it may be safely estimated that one-half of the population enumerated in the census die before the age of five years; and hence the annual infant mortality includes little short of a *moiety of the human race,* unless it can be shown to be greater in our large cities than through the country and through the world. But of this hereafter.

Of the fearful *increase* of infant mortality in New York, regarding this as a type of other cities, we have the testimony of the same statistical table. In the year 1853, the deaths under five years numbered 12,963, while in 1843 only 4588 such deaths occurred, showing the apalling increase of 8375 within ten years, which is vastly beyond the proportional increase of the population of the city during the decennial period, as shown by the census. Moreover, this increased infant mortality in 1853 as compared with 1843, is in a ratio very far beyond that of the aggregate of the deaths in persons of all ages, in each of these years respectively, found in the same table. The deaths under five years in 1853 were 12,963, while the deaths of all others in the city of every age numbered only 9739; so that the *infant mortality* exceeded all the other interments for that year by 3224 ! This single fact exhibits in a striking light the importance of the subject of infant mortality in view of its frightful extent, and its alarming increase within ten years. In 1843, the infant mortality exceeded the half of the aggregate mortality of the city by only a few *hundreds;* but in 1853, the excess over one-half the entire number of interments of all ages in the city reaches as many *thousands.*

These particular years have been selected only because the semicentennial table from which we quote includes the fifty years ending in 1853, and because it was only in 1843 that the City Inspector was by law empowered to record all the deaths occurring in the city, irrespective of the place of their interment, and hence from this latter date he vouches for the reliability of the table. The mortality for 1854, '55, and '56 is appended for comparison, thus bringing our data down to the present year. Other corroborative tables, as those of Drs. Emerson and Condie, of Philadelphia, Dr. Lee and others, may be found in the books, and are too familiar to the profession to be reproduced here. They all show the enormous extent of infant mortality, and its amazing increase, the latter being a fact peculiar to our American cities, for in the great cities of the old world the mortality of infancy has been annually diminishing for many years.

This single fact exhibits in a striking light the vast importance of our inquiry. Why should infant mortality in American cities be greater than even in Paris ! eight per cent. above Glasgow, ten per cent. above Liverpool, and nearly thirteen per cent. greater than in London ? Why should it be increasing here and diminishing there ? And this too when statistics abundantly show the mean duration of human life to be greater by three and a half per cent. in our American

cities taken collectively than in the cities of Europe? Whether we shall be able to point out the reasons for this remarkable disparity or not, by demonstrating that there are other sources and causes existing and operating here than in transatlantic countries, the fact remains that infant mortality has attained gigantic proportions among us, and is increasing with amazing rapidity; and this too when the general salubrity of our climate, and the facilities for sustaining and preserving life with us, are superior to those possessed by any country on the globe.

The *first year* of infancy, as we have seen, exhibits the most appalling waste of life, being one-fifth of the aggregate mortality of our whole city population of all ages and from all causes. The dangers to life attendant upon early infancy, and especially during the first year, are well understood by the profession and the public; these arise from a variety of causes, viz:—

1. Defective vitality at birth, hereditarily transmitted from one or both parents, whereby the infant is not viable, and perishes from inanition; nutrition and development being physically impossible. These are reported in the bills of interments as cases of marasmus, tabes mesenterica, consumption, etc.

2. Mismanagement of infancy, by parents, nurses, or doctors, in feeding and physicing the newly born; depriving them of the nutriment simultaneously flowing into the mother's breast, as nature's only and all-sufficient supply for nutrition development, and substituting therefor the thousand slops, teas, and drugs which officious grannies, of both genders, are wont to prepare and administer. It may safely be computed that a moiety of the mortality among infants of days, is the direct result of spooning into the stomachs of new-born children some of the worst simples and compounds which they will ever taste through life, in case they survive the infliction. Not merely molasses, or sugar and water, catnip tea, olive or castor oil, goose-greese, spoon victuals, and the like, but salt and water, soot tea, gin sling, and even *urine* are incontinently forced into the infant's throat before it has known an hour of life. Thousands thus perish in early infancy, their deaths being ascribed variously to colic, cholera, diarrhœa, dysentery, or convulsions, though oftener produced by drugging for the relief of symptoms which the mother's earliest milk would have prevented or cured; life being sacrificed by soothing syrup, Godfrey's cordial, Jayne's carminative, or some other vile mixture of molasses and water, with opium and brandy. These sleeping draughts and anodyne nostrums are more deadly poisons to budding life than all the diseases of infantile existence; and to these a very large proportion of infant mortality is justly to be ascribed, for whole hecatombs of victims are thus poisoned out of life, in the very dawn of their being.

3. The ratio of infant mortality in large cities is conceded to be much greater than in country towns or rural districts, and for the reason that in the former so large a proportion of the births take place in the abodes of the indigent, which, if not in garrets, or cellars, or shanties, are sadly deficient in the supply of light, pure air, free

ventilation, cleanliness, clothing, fuel, and wholesome food, so necessary to the health, comfort, and safety of the mothers, not less than their offspring, whose vitality is henceforth to be derived from the maternal bosom in the milk, whose quality depends on the blood which circulates in her veins. Multitudes of infants born under these adverse circumstances of the atmospheric contamination, perish in a few weeks or months for lack of pure air; and instead of marvelling at the extent and increase of fatality among such, we might rather wonder that any survive.

4. How much of the infant mortality in large cities, and its alarming increase, is the legitimate result of quackery in some one of its varied forms, to which sick children are subjected, may be difficult even to conjecture. That "false theories in medicine have slain more of our race than war, pestilence, and famine combined," has been affirmed by high authority. It is sustained by historic evidence. Nor have these medical heresies, the offspring of ignorance, presumption, superstition, and avarice, ever been more rife, especially in large cities, than they have been of late years, and still continue. Every phase of quackery is characterized by an over-weening faith in drugs, and a delusive confidence in specifics, inspired by the brazen effrontery of the charlatans who "by this craft have their gains," and who employ themselves in encouraging the people to become, with the aid of their new system of drugging, "every one his own doctor." Multitudes fall into this snare, and by the purchase of a box of specifics, and a book of instructions, are assured that they are possessed of the remedies adapted to all the diseases to which "our flesh is heir." The popular mind is indoctrinated by these quacks into the belief, that in all ordinary diseases they may confide in these specifics, especially for *infantile diseases*. And that thousands are annually added to our infant mortality by diseases entirely within the control of the healing art, the early periods of curability being lost, in these experiments of ignorance by credulous parents and pretended physicians, is notorious in every city. So true it is in this connection, that "for want of *timely* aid, millions have died of medical wounds;" nor is it less true, that by injudicious and misguided interference with drugs, by the ignorance of mothers, nurses, and doctors, our infant mortality is immeasurably augmented.

It remains, however, now to allude to the very considerable proportion of early mortality among the depraved and vicious families who abound in large cities, which results from the transmission of the hereditary poisons of either scrofulous, scorbutic, or syphilitic disease, from one or both parents to their offspring, whereby their young blood is fatally tainted with constitutional maladies, extending to the second, and even the third generation. * * *

Is there any remedy?

1. No marriage should be permitted between parties, until the physical health of both has been subjected to professional scrutiny. And such alliances should be prohibited by law, to those of either sex, who are the subjects of those diseases which are known to be hereditary or transmissible to offspring, or such as are fatal to

infantile existence. Celibacy should be required by statute of all consumptive, scrofulous, scorbutic, gouty, insane, intemperate, and especially syphiltic individuals of either sex, and this, for grave reasons of state, which concern the public weal. Nor will any course, short of such legal prohibition of marriage, adequately correct the evil of that large proportion of infant mortality thus engendered.

2. To remove the temptations to the unnatural crime of abortionism, and prevent the abandonment and cruel murder of unborn and newly-born infants, among the vicious and depraved portion of our population, for purposes of concealment, as in the case of the illegitimate offspring of shame, foundling hospitals should be provided by the State, in all our large cities, for the reception of infants, and the concealment of the shame of unhallowed mothers, and the protection and preservation of the infant innocents, who are doomed to abandonment by the guilty authors of their being. These charities, wisely conducted, would diminish the still-born and premature birth interments, in all our large cities, by a moiety at least; while they would almost annihilate the plea of necessity, urged in behalf of the horrible trade of abortionism, and thus lessen the number of its victims. Lying-in asylums, for expectant mothers, irrespective of character, whether married or unmarried, in which such might conceal their shame, and then "go and sin no more," are equally called for, to prevent the double suicide, so often resorted to, by such; while diminishing the extent and increase of infant mortality, the foundling hospitals being made open to them all.

3. The "poor we always have with us," by a sacred legacy from the common Father of us all, and hence, the duty and responsibility of caring for such is recognized in every Christian community. But we have seen that among the suffering poor in our large cities, a fearful ratio of our infant mortality is found; and chiefly among the thousands of families, unreached by any of our public charities. The habitations of the poorer classes of our population, are for the most part in narrow, contracted alleys, filthy courts, or underground cellars; or, at best, in what are called tenant-houses, in the miserable apartments of which, thousands of families, each cook, eat, and sleep in a single room, without the light, ventilation, or cleanliness essential to the life of either parents or children. Under such adverse circumstances, often destitute of wholesome food, comfortable clothing, or necessary fuel, the children of such families sicken, pine away, and die, prematurely, to an extent wholly unappreciated by the public, and unrelieved by the philanthropy of either the church or the State. Nor will this increasing source of our infant mortality be arrested, until the civil authorities shall, by public law, require the erection of dwellings for the poor, in accordance with the laws of health and life; and until, in all our cities, there shall be a sanitary medical police, whose duty it shall be to enforce such laws. No medical treatment can, by possibility, arrest diseases, or diminish their fatality, while the victims are found in the squalid and filthy abodes of the indigent, from which pure air, and often the light of heaven, are

excluded, as among the wretched multitudes of our "cellar population," who furnish annually so large a share of our infant mortality.

4. The erection and endowment of hospitals for sick children are an imperative want in all our large cities, demanded alike by philanthropy and the public welfare, for the children of the poor, who thus only can be removed from the fatal atmosphere of their homes, by which their diseases are engendered, and within which their recovery is impossible. And as each of such habitations of the poor becomes, for lack of air, ventilation, and cleanlines, a centre of disease among its inmates, so, also, is it a nucleus, whence its atmospheric poison radiates through the neighborhood, infecting, by a physical necessity, the whole vicinity. It is thus that endemics become epidemics, and a filthy tenant-house is the source of pestilence, infecting the section in which it is located, and often sweeping over a wide space, or including a city in its ravages. Hence, if the public authorities be indifferent to the claims of humanity, let them be moved by their fears; and let childrens' hospitals be founded, for the reception of the squalid offspring of the indigent, as a measure demanded by considerations of the public economy and public safety.

5. Infant mortality in large cities, in a great multitude of examples, which no man can number, is caused by the impure and adulterated milk, and other unwholesome articles of food, etc. * * *

Table of Mortality in the City of New York, for the fifty years between 1804 *and* 1853 *inclusive. Classified according to age.*

Still-born and premature births	24,164
Of one year and under	78,762
Between 1 and 2 years	40,281
" 2 " 5 "	32,896
" 5 " 10 "	14,351
" 10 " 20 "	14,820
" 20 " 30 "	41,740
" 30 " 40 "	41,351
" 40 " 50 "	29,114
" 50 " 60 "	17,948
" 60 " 70 "	12,879
" 70 " 80 "	8,278
" 80 " 90 "	3,769
" 90 " 100 "	813
Of 100 and upwards	105
Ages unknown	1,971
Total	363,242

ART. X.— *Yellow Fever Epidemic of* 1856, *at New Castle, Jamaica.*

DR. LAWSON, Deputy Inspector of Hospitals, communicated an elaborate paper on this subject to the Army Medical and Surgical Society (Feb. 6, 1858). The paper was amply illustrated by cases, tables, a plan-drawing of the mountain ridges on which New Castle stands, and

a photograph of the barracks, etc., showing their position and distribution.

The most novel and interesting features connected with this report, which is much too long to abstract or abridge without destroying its integrity, are the following :—

1. It denies the conclusions arrived at by Baron Humboldt, Dr. Fergusson, and others, of the possibility of yellow fever occurring at an elevation of 4,000 feet above the level of the sea. On this point the author states : "An opinion has long prevailed that the severe forms of tropical fever could not originate or spread at a considerable elevation above the sea."

The grounds for this opinion seems to be the statement of Humboldt, that yellow fever was confined to the low country on the coast near Vera Cruz, and that it did not pass the farm of L'Encero, elevated 3,045 English feet above the sea, "the heat there being insufficient to develop its germ ;" and that of Fergusson, with reference to the varieties of fever occurring at different elevations in St. Domingo.

The remarks of these authorities were no doubt correct for the time and place ; but it must not be thence concluded that all the conditions requisite for the production of these forms of fever, except that of suitable elevation, were present in the cases they adduce. Their deductions, therefore, require to be applied cautiously to other localities, and may render certain modifications of the conditions henceforward inapplicable to the same localities at another time.

2. It disposes satisfactorily enough in the present epidemic of the non-contagious nature of yellow fever. The following are the grounds for this conclusion : That out of 156 men, taken indiscriminately from the different barrack-rooms, very few of whom could have had yellow fever before, and who afford 210 instances of exposure for 24 hours to the emanations from the sick in the fever wards, that only eight were subsequently affected with fevers of any sort, of whom three died ; while from the remainder of the troops in the cantonment, amounting at the commencement of the epidemic to 523, there were 89 attacked with fever, of whom 38 died. Putting these numbers in the germ of a centesimal ratio, for the sake of comparison, they stand thus :—

	Total strength.	Attacked per cent.	Died per cent.	Died per cent. of those attacked.
Men who attended fever cases	156	5.1	1.9	38.0
Men who did not attend fever cases	523	17.0	7.3	43.0

It likewise disproves the commonly received opinion, that a temperature of at least 80 degs. Fahrenheit is necessary to develop its germ, whatever that may be.

This fact is illustrated by the following table of temperature :—

1856.	6 A. M.	2 P M.	6 P. M.	Mean.
July	67.5°	74.2°	72.5°	71.4°
August	67.1	73.8	72.0	71.0
September . . .	67.4	73.7	71.8	71.0
October	65.3	72.7	70.1	69.4
November . . .	62.2	69.2	64.2	66.6
December . . .	60.1	68.8	63.5	65.3

the disease having prevailed as an epidemic at New Castle with a mean of 10 degs. lower, and continued until the mean temperature had fallen 5 degs. more.

The author confirms the fact previously noticed, of the suspension of yellow fever under the influence of heavy rain, which has been observed in every epidemic at Sierra Leone, at which time the fever assumed a purely remittent type. He inquires, "Can this be explained on the assumption of yellow fever being propagated by specific contagion?"

He concludes by stating, with reference to New Castle, that the above facts seem to leave open no other inference than that the yellow fever there in 1856 arose from local causes.

Whether similar causes were in operation there in other years, and if so, why they did not lead to a similar result, are questions that the present state of our information does not admit of being answered. It seems, however, that in addition to the ordinary local causes of disease, an epidemic constitution is necessary to account for the prevalence of fever.

Two out of three of the medical officers present were of opinion that it arose from local causes, and one that it might have been imported.—*Med. Times and Gaz.*,—*Am. Jour. Med. Sci.*

ART. XI.—*Therapeutic Action of Mercury.* By F. W. HEADLAND, M. D.

1. It is of use in inflammations, by decomposing the plastic elements of the blood.

2. It is of use in syphilis and other diseases, by eliminating bad matter from the system.

3. It is of use in some diseases, by increasing the natural secretions.

Prop. 1.—*Mercury is of use in inflammations, by decomposing the plastic elements of the blood.* Becquerel and Rodier have shown that in acute inflammations the proportion of fibrine in the blood is increased on the average to 5.8 in the 1000 parts, nearly double the amount of health. Mercury is an *antiphlogistic;* it directly counteracts inflammation by diminishing this fibrine. By impoverishing the mass of the blood, it weakens the heart, and thus it both counteracts effusion and assists absorption. It is slower in its effects, but more lasting in its action, than either blood-letting or antimony. Less adapted than the latter for sudden emergencies, because having no immediate but only an indirect power over the heart, it is better suited to cases where inflammatory effusion has already occurred. In the end, mercury diminishes the corpuscles and albumen, as well as the fibrine. It thus leaves weakness behind it, and always does some harm as well as good. To use it, is like pointing a battery against a multitude in which our friends are mixed with our enemies, so that on discharging a volley we slay both together.

In the eruptive fevers and in cholera, where the fibrine of the blood is much diminished, instead of increased, the employment of mercury to any extent is for this reason perilous. Fortunately, as generally given, in the shape of calomel, etc., it is seldom absorbed in such cases as these ; and this is probably the reason why we so seldom hear of its ill effects, though these are commoner than the cures effected by its use.

Prop. 2.—*Mercury is of use in syphilis, and other diseases, by eliminating bad matter from the system.* It is the deliberate opinion of a considerable majority of intelligent surgeons that there are certain cases of syphilis in which the proper administration of mercury causes a cure; that this cure is a more certain result of the use of mercury than of any other remedy; and further that it is more effectual, inasmuch as it is seldom followed by secondary or ulterior symptoms.

In dividing under three chief heads the cases of syphilis which may occur in a constitution not previously under the influence of the poison, I am only expressing what I believe to be a common opinion at the present day, and which is very well set forth, as far as I am a judge, in the public lectures of Mr. Lee.

A fresh sore results from venereal infection. During four to five days the inoculated poison must work in the tissues about the surface of this sore, before it can be sufficiently elaborated to affect the general system through the blood. If, during this time, the sore sloughs, then the poison is destroyed and sloughed away before it is ripe. If we produce this effect by caustics, being sure that we are in time, the same occurs. Then we need not, I think, administer mercury.

In a second class of cases the sore ulcerates. Its edges are rough and jagged. It has not sloughed away; but the poison having ripened, has passed along the absorbents to the glands in the groin. These swell and suppurate, and with the pus the poison is discharged ; it does not further affect the system. In such a case, we need not, I think, administer mercury.

In the third case, the inflammation on the surface of the sore is of the adhesive kind. The edges are raised, the surface is somewhat cupped. The whole has a horny feel. There is little irritation ; no sloughing. It is the chancre of Hunter and Carmichael, more or less marked. We will suppose that the period of ripening has passed. The poison now infects the blood; it does not stop at the glands or inflame them. It passes straight into the system at large, and a rash declares that the constitution is affected. When a chancre of this kind is destroyed by nitric acid, or some other caustic, then all may go well; but when it has had time to mature, then, I think, we *should* administer mercury. We must introduce the only medicine of which we are thoroughly convinced that it posesses the power of efficiently striving with the enemy who by subtle means has now effected an entrance within our stronghold.

Syphilis, with the eruptions, and ulcerations, and necroses, and the cachexy, that result from its working, is obviously a poison which itself decomposes and impoverishes the blood; and it seems to me not unreasonable to suppose that mercury, in its destructive action, seizes

first on those parts of the blood which are most diseased or most liable to putrefaction; that it grapples thus immediately with the fermenting and multiplying virus of syphilis, decomposes it, as well as those materials of the blood on which it has commenced to feed, and eliminates both these and itself together by the glandular outlets of the frame. To cure the corrosion of the vital parts, the mercury corrodes somewhat further and deeper, and sweeps all away. Its use may be compared to the wise plan of blowing up a number of houses to save a city on fire.

To be of any good the mercury must be given while the syphilis is comparatively recent. In the later forms of the disease, called secondary and tertiary, the mischief is already done; the blood is depraved and a new phase of morbid action has taken the place of the first. Here, iodide of potassium is the remedy, and mercury, if ever given, should be given most cautiously, or in cases where its use was culpably omitted at the first.

From what I have already said, it will be seen that I do not consider it of much importance what plan of administering mercury in syphilis is followed, so long as any plan is carried out effectually. The practice of inunction is very efficacious, but is now unfashionable, because uncleanly. The internal use of calomel, etc., is more perilous, but it may be best carried out by giving doses at frequent intervals. The fumigating plan may astonish us by producing a most notable effect just when we least expect it. Mercury should not be given to produce much salivation. If the mouth is sore only, we know that the system has as much of the mercury as it can hold.

In such cases of syphilis as are common amongst soldiers and the *habitués* of the out-patient rooms of our London hospitals, we may often pause before we determine to administer mercury. The true chancre, which calls for it, is comparatively rare amongst those who have been often attacked ; and when the constitution has long been accustomed to the syphilitic poison at various stages, and is liable at any time to receive a new infection which shall render our care nugatory, it is better perhaps to abstain from adding to the existing cachexy by so potent a depressing cause as mercury, and to rely most on other more gentle evacuants, on external treatment, and hygienic measures of a simple kind. In such cases as require the iodide of potassium, this may be given with less apprehension. The secondary and tertiary disorders, in which mercury often does little or no good, are in many points to be compared to scrofula, where also, except in small doses, this remedy is contra-indicated, whereas in both classes of cases the preparations of iodine are used with admirable effect.

In other diseases besides syphilis, it is very possible that a morbid poison may be occasionally got rid of in the same manner as I have supposed to be the case here. In lithic, gouty, chronic rheumatic, and dyspeptic disorders, a mercurial cathartic may sometimes eliminate along with itself the decomposed materies morbi. This is the action of mercury to which the name of "alterative" is commonly given.

Prop. 3.—*Mercury is of use in some diseases by increasing the natural se-*

cretions.—This medicine increases all the secretions of the body With regard to the three great glandular outlets, it affects the bowels most, the skin next, the kidneys least. But in most cases where any of these secretions are repressed or diminished, or where general elimination is advisable, mercury may be given with greater or less advantage. It is in common use as a cathartic, but it lowers the system, and should be employed warily. It is of great utility in hepatic dropsy, but generally does harm in granular disease of the kidney.

Of all organs in the body, mercury is most attracted by the liver. It is most difficult to produce salivation in cases of inflammation or suppuration of that organ. In an instance of chronic poisoning by mercurial fumes, where the individual died twelve months after removal from the source of those fumes, mercury was found in the liver, but in no other organ or part of the body. Mercury increases the secretion of healthy bile. This is not only a matter of vulgar observation and belief, but has been scientifically demonstrated by Buchheim and others. The bile I believe to be *the tonic of health;* the material which performs the same function in the sound system as that which is discharged by bitter medicines in disease. Mercury is a general eliminative; it is also a *catalytic* blood-medicine. There are two ways in which it may act in controlling disease. There is probably yet a third way, to which I would call special attention here. Mercury, in small doses, is used with more or less effect in all the diseases which are controlled by quinine; and I can think of no explanation more calculated to account for this than the supposition that the mercury is of use by causing an increased secretion of bilè, part of which, required in the blood, and not destined for excretion, is reäbsorbed into the blood, as shown by Liebig, and used in the system. In intermittent diseases, in remittent and bilious fevers, in many cases of debility, in chronic gout, rheumatism, scrofula, etc.,—diseases where quinine is frequently of use, and where the function of the liver is at fault,—mercury in small doses is often given with effect. In such cases we must by no means combine it with opium. This combination may be suitable in some peculiar cases of local inflammation; but it often hinders the proper action of mercury, instead of promoting it, and checks the elimination which is attended with such advantage.

I must now conclude this brief sketch of the action and uses of mercury. Were we to believe all that has been said about this drug, we should welcome in it a cure for all the ills that afflict humanity, the veritable panacea of the old dreamers. Unfortunately we cannot do this. But it is an agent of wondrous properties, a remedy of vast and varied power; perhaps, altogether, the most useful in our Pharmacopœia. It is the very prince of that class of remedies, unfortunately too few, that are capable of entering the system, of grappling with a disease in the blood, and of coming off victorious in the struggle.

Putting aside for the moment its use in the phlegmasiæ and in chronic disease, were it only for its power in syphilis, it would be of the utmost value to us. The syphilitic poison is connected in some

manner or other with about half the disease of towns. Is it likely to die out? Fordyce, on being asked this question, replied with another —Are men becoming tired in the pursuit of pleasure? The great moral ulcer of our social system, in spite of all the efforts of philanthropists and legislators, is hardly likely to be healed as long as men continue in their present state of mind.

"Audax omnia perpeti
Gens humana ruit per vetitum nefas."

Moreover, it is said, and I think with a fair show of reason, that syphilis is even gaining upon us in London. Is it wise then to do what some would advise, to let the only valuable weapon with which we are armed against it drop through our fingers? The more we know of its real actions, of the mystic processes of its absorption and operation on the system, and of the comparative physiological tendencies of the various forms in which it is administered, the better shall we be able to wield it with skill and effect. In a line of investigation and of patient experiment on disease and remedy lies our best and wisest course; and so long as we steadfastly pursue this widening path, so long at least we physicians can say, that at the bottom of our box of Pandora there lies a Hope which is not yet flown.—*Lancet.*

ART. XII.—*Results of Treatment in seventy-one cases of Pneumonia in the Glasgow Infirmary; with some Remarks on Dr. Bennett's novel views on this Disease:* By JOHN BELL, M. D., Physician to the Infirmary, etc. (Concluded from the March number.)

1. *Mortality.*—On a former occasion I have pointed out, that the ratio of mortality in pneumonia will not enable us to arrive at a correct conclusion regarding the superiority of one plan of treatment over another, unless the cases in the respective series of observations be identical as to their stages when submitted to treatment,—to the extent of lung, or lungs, involved,—to their complications,—to the season of the year,—to peculiarities of constitution of patients,—their age, sex, and the sanitary condition of their abodes, etc. Now, as no such system of classification has been made in the different series of cases which Dr. Bennet compares, I hold that no conclusion regarding the comparative value of different plans of treatment, *can* be deduced from the respective rates of mortality. No reliance, therefore, can be placed on the first test,—its character is very bad.

2. *Duration of the Disease.*—If medical men had agreed on a definite basis, according to which the duration of the disease could be calculated with precision and uniformity, then duration might be considered as a tolerably fair test, from which conclusions could be drawn as to the relative value of different plans of treatment. Unfortunately, no such standard has hitherto been adopted. Dr. Bennett

admits this, and points out very forcibly the great want of uniformity which characterizes the plans used by different writers for calculating the duration of the disease.

But it may be argued that though Dr. Bennett has committed an error in the manner in which he has brought forward his tests, and though their character should be liable to suspicion, yet the statistical evidence which he has elicited from their application may be of great value. I am quite willing to admit all this; let us, therefore, proceed to examine, as briefly as possible, the statistics which he has furnished.

1. *Treatment by Bleeding, or "The Positive Knowledge of the Experience of the Past."*—Under this head he supplies us with a table, showing the number of cases of pneumonia treated in the Royal Infirmary of Edinburgh, from July, 1839, to Oct. 1, 1849:—Total number admitted, 648; deaths, 222; or *rather more than one out of every three.* In contrasting this result with that which has followed the natural plan of treatment recommended by Dr. Bennett, viz., one death to $21\frac{2}{3}$ cases, the apparent superiority of the latter becomes startling. What a dismal gloom invests "the experience of the past," and what a splendid halo of light surrounds the "correct observation of the present!" Why, the names of "Cullen, Gregory and others," must endure longer in the esteem and recollection of the undertakers and their craft, than they can be retained on the monumental marble! Out of respect to the memory of these highly gifted men, who have adorned and advanced our profession, let us bestow a little careful consideration on the important difference in the rate of mortality, and endeavor to find out if any logical parallelism exists between the cases which they treated, and those which came under the care of Dr. Bennett. Therefore, at the very outset of our inquiry, the question that meets us is the nature of the 648 cases contained in the table. We have no information on this most important point. We cannot tell either the stage, or the complication, or the duration, either before or after admission. All we do know is, that Dr. Bennett tells us the cases have been *entered* in the books of the hospital, under the *name* of pneumonia. He admits this deficiency, and says:—"No doubt it cannot be pretended that *perfect accuracy as to diagnosis was attained* in all these 648 cases. It is certain, also, that numerous complications, and the debilitated constitutions so frequently met with in the practice of a large hospital, *seemed to swell the mortality.*" Here, then, we find that he fully admits that no reliance can be placed on the results of this experience, and yet he adduces it as forming the strongest argument in support of his views. Again, I do not understand on what principle he can call the results obtained from the practice of the Edinburgh Infirmary, from 1839 till 1849, as constituting "*the experience of the past.*" I think it of great importance to keep this fact in mind. It shows us that Dr. Bennett takes a most limited and unwarrantable view of "past experience"—one that does not include the practice either of "Cullen, Gregory, and the others," whose opinions and treatment he condemns. If he had taken the trouble to have made himself familiar with the writings of these authors, he would have found

"*the experience of the past*" a very different matter from the caricature which he selects out of the *modern records* of the Edinburgh Infirmary (I say, modern records, because they only extend back eighteen years —commencing in 1839 and ending in 1849, only eight years ago); he would have discovered that the disease recognised as pneumonia, and treated as such, was of a very different description from that which has come under his observation; that the pneumonia of Cullen and Gregory was, for the most part, a very acute and sthenic affection, and was rapidly cured by bleeding, provided this was practised early. But Dr. Bennett overlooks all this practical teaching of the past, and attempts to stamp it with obloquy, by adducing a most worthless and equivocal table of the results of the practice employed at the Edinburgh Infirmary during a comparatively recent *period of ten years*—a method of proceeding by no means calculated to promote the advancement of practical medicine. We find, therefore, in the first place, that no confidence can be placed on the statistics which Dr. Bennett furnishes from the records of the Royal Infirmary of Edinburgh; secondly, that these statistics apply, not to the experience of the past, but to that of a *modern* period; third, no mention is made regarding the average duration of the 648 cases.

2. The experience of M. Louis is next appealed to, showing a mortality of 1 to 3½. But as Dr. W. T. Gardner has shown that the results of M. Louis' practice is altogether inadmissible in the present discussion, I will not enter on this point.

3. He next quotes the results of bleeding in the practice of M. Grisolle. He tells us that in recent cases the mortality was one to ten. In cases were hepatization had occurred, the deaths, after bleeding, amounted to one in six. M. Grisolle had fifty cases of the former class of patients, and 232 of the latter, so, by combining the two, we have a gross number of 282 cases and 42 deaths, or one to 6⅔. The only remarks which I have to offer on this evidence are, in the first place, the unfairness of taking the average mortality of a gross number of cases. In this way Dr. Bennett secures what he wishes, viz: to show a large rate of mortality against bleeding. The manner, therefore, in which he has taken the average gives a most unfair and unjust view of the results of M. Grisolle's practice, in reference to the question at issue. Secondly, Dr. Bennett supplies us with no information regarding the average *duration* of the disease under the treatment of M. Grisolle.

4. *The Army Returns.*—He tells us that these show a mortality of one to thirteen cases. But this favorable result is rejected by Dr. Bennett "as of no service to the present inquiry," because "we have no information as to the exactitude with which they were made, how the diagnosis was determined, or what was the treatment." Now, we have seen that the very same defective information exists in the statistics of the Edinburgh Infirmary; but, as they show a mortality favorable to Dr. Bennett's views, their evidence is sustained; on the other hand, as the army returns exhibit an opposite result, they are rejected as untrustworthy. The high professional character to which the medical officers of the army are justly entitled, will not suffer in

the slighest degree by the imputations so unequivocally implied in Dr. Bennett's remarks.

Having found that the experience of the past gives not the slightest sanction to the views of Dr. Bennett, let us next direct our attention to the statistics which he quotes from "the more correct observation of the present day."

1st. *Dietetic Treatment.*—He tells us that in the Charity Hospital of Vienna, under the care of Skoda, out of three hundred and ninety-two patients, fifty-four died, being at the rate of one in $7\frac{1}{4}$. According to Dr. Bennett's own showing, no confidence can be placed in the results of this practice. He tells us, "that remedies are given to meet occasional symptoms." "Occasionally opium was given in small doses, if there was much pain. Venesection was also practised early if there was much dyspnœa, and emetics given if the expectoration consisted of tough mucus." We are neither informed of the number of cases so treated, nor of the character of those treated by diet alone, nor yet of the relative mortality under the two plans of treatment. In such circumstances, I leave you to form your own opinions how far Dr. Bennett is justified in concluding that Skoda's practice proves the superiority of dietetic treatment over antiphlogistic. Dr. Bennett next adduces the experience of Dr. Dietl, in the same hospital. But here we are also left without any information regarding every point that is requisite to enable us to form a correct opinion as to the nature of the cases subjected to the three plans of treatment. We are merely told that the mortality was one in five under blood-letting, under tartar emetic one in 5.22, and only one in $13\frac{1}{2}$ under diet. These results are perfectly worthless, unless we were furnished with the necessary data that would enable us to judge of the comparative severity of the three class of cases. I cannot help thinking that Dr. Bennett has been peculiarly unfortunate in appealing to such statistics—instead of supporting his views, they have the very opposite tendency. I may remark that it is somewhat singular that the rate of mortality under Skoda is nearly double that of Dietl. It is also strange that Dr. Bennett has entirely omitted to mention the average duration of the disease under the dietetic treatment. This is a most important omission, as he deems the duration of the disease as one of the best tests for the efficacy of any plan of treatment. The other test of mortality will not bring out a result favorable to his views. If we add the cases of Skoda and Dietl together, we find the rate of mortality to be about 1 in $8\frac{1}{2}$. But it is still much more remarkable that Dr. Bennett has not supplied us with the results of the more recent experience of the dietetic plan. Though in 1849, the period to which Dr. Bennett refers, the mortality was reported to be only one to $13\frac{1}{2}$, or seven per cent., yet in 1852 the mortality increased to nine per cent., or one in eleven cases, and in 1854 the ratio had risen to 20.4 per cent., or about ONE in *five*. At all events, the high rate of mortality under the dietetic system is most unfavorable to Dr. Bennett's views.

We come next to the homœopathic statistics. The appeal to such a source must be considered alike insulting to the memory of the il-

lustrious dead, and the whole medical profession of the present day, unless we find the facts adduced are so striking and convincing, as to justify such a dereliction. But what are we to think, when we find Dr. Bennett telling us, that the homœopathic statistics are "doubtful"—"not fairly comparable with other hospital statistics?" But he adds, "There can be no doubt, however, that *many* severe cases of pneumonia recovered under a system of treatment which, it appears to me, most medical men must consider to be essentially a dietetic one." Such is the evidence he adduces. No facts, merely his own opinion as to what may have occurred in many cases. Though he neither informs us of the number of cases treated, the rate of mortality, nor the duration, yet, at a subsequent part of the paper, he argues as if he had proved the superiority of the homœopathic over the antiphlogistic treatment. He therefore considers himself authorised, on such data, to include the statistics of this "globule" humbug, along with the more correct observations of the present day, and to appeal to its records, so as to enable him to stigmatize the experience of the past, as being unsound in pathology and disastrous in practice.

Let us next examine the remaining item of the more correct observation of the present day, viz., the results of Dr. Bennett's peculiar plan of treatment. This he denominates, "Treatment directed to further the natural progress of the disease," and describes it as follows:—"The treatment I have pursued in pneumonia is founded on the pathological principles formerly given, viz., not to attempt cutting the disease short, or to weaken the pulse and vital powers; but, on the contrary, to further the necessary changes which the exudation must undergo, in order to be fully excreted from the economy. To this end, during the period of febrile excitement, I content myself with giving salines in small doses, with a view of diminishing the viscosity of the blood. As soon as the pulse becomes soft, I order good beef tea and nutrients; and if there be weakness, from four to eight ounces of wine daily. As the period of crisis approaches, I give a diuretic, generally consisting of half a drachm of nitric æther, sometimes combined with ten minims of colchicum wine, three times a day, to favor the excretion of urates. But if a crisis occurs, by sweat or stool, I take care not to check it in any way." Before we examine the results of this much-vaunted and so-called scientific treatment, let us pause to reflect on one or two remarkable points in the treatment itself.

1. *Its Character.*—Every one who reads the above extract, will not hesitate to agree with me that Dr. Bennett's peculiar plan is of an unique character—a sort of mongrel production—a cross between the dietetic and homœopathic systems. It does not even possess either the purity and virtue of orginality, or of a respectable parentage.

2. How long is it since Dr. Bennett *adopted* this *natural* plan? The first sentence in the passage which I have quoted, is calculated to leave the impression on the reader's mind that he never employed any other treatment. But a little further down, in the same page,

he tells us that it has been "publicly carried on by me in the clinical wards of the Royal Infirmary during the *last eight years.*" We therefore perceive that the more correct observation of the present day dates its origin from 1849. Few will be able to explain the principle on which Dr. Bennett so dogmatically draws the line of demarcation between the past and present periods of medical observation and experience. The only ground we can find furnished for this most important distinction, is the discovery of his *natural plan of treatment.* It is a very remarkable coincidence that this "new light" should have broke out at the commencement, not only of a new decade, but the beginning of the second half of the present century. I have no doubt but that he will consider this as a most auspicious omen for the introduction of *his medical millennium.* Is not the coincidence very striking? It furnishes quite as good a proof of the soundness of his views as any he has produced.

3. "*Crisis.*"—You must have been startled by the introduction of this term. Can any one present explain the meaning of this word when applied to pneumonia? By the "period of crisis," whether does Dr. Bennett mean the suspension of the inflammatory action, or the effusion of liquor sanguinis, or the subsequent "cell growth," or the disintegration of these spheroidical vesicles after they have passed through their *purulent existence,* or their final excretion from the economy? I am at a loss to understand to which of these pathological conditions the terms "period of crisis" are applicable. Doubtless he informs us that this crisis manifests itself by "sweat and stool," and that he takes care not to check it in any way. We can have no difficulty in comprehending how he could restrain either "sweat or stool," but we are at a loss to conceive how a crisis is to be curbed.

Perhaps he means to imply by the term "crisis" a *marked diminution* of symptoms, or in other words, an improvement in the condition of the patient. If this be his meaning, I quite agree with him in thinking that it would be very unwise to check the means which nature takes to effect this salutary change. But the clear and important lesson which she teaches "*by sweat and stool*" is quite overlooked by Dr. Bennett. In fact, these operations of nature, in most emphatic language, decide against his interpretation of her method. Sweating and purging are powerful antiphlogistics. Kind nature employs them in the case of pneumonia, and though Dr. Bennett records with plaudits their effects, yet in the same page he denounces all such remedial agents, as being inconsistent with the more correct observations of the present day.

I have next to direct your attention to the results of Dr. Bennett's practice. He tells us that "on examining into the results of this practice, which has been publicly carried on by me in the clinical wards of the Royal Infirmary during the last eight years, and which has been carefully recorded by the clinical clerks, I find the total cases to be sixty-five, the average age thirty-one years. Of these sixty-two were dismissed cured, and three died, that is one in $21\frac{2}{3}$."

I intend to arrange my remarks on this part of the subject under three heads: 1. The character of the cases subjected to treatment

2. The amount of confideuce to be placed in his statistics. 3. The amount of support which the results of his treatment afford to his propositions.

1. *The nature of his cases.*—In accordance with his definition of pneumonia, the lung in all his cases must have been hepatized. I think we may safely draw this conclusion in the absence of all information on this most important point. We are confirmed in this opinion by reading the numerous cases of pneumonia which Dr. Bennet has so liberally published in the *Edinburgh Monthly Journal of Medical Science.* Though we find that these cases are designated "acute," yet, with one exceptiou, they had passed into the stage of hepatization. Again, in some instances the inflammation of lung had *become merely superadded to "chronic bronchitis."* Let any one read over the symptoms of these cases (of course excluding physical signs), and compare them with the descriptions of pneumonia found in Cullen, Mason Good, Mackintosh, etc., and the conviction will be forced upon him, that these older physiciaus would not have recognized Dr. Bennett's cases as inflamniation of the lungs, but would have treated all such instances as catarrh or peripneumonia notha—affections in which they usually employed some expectorant, aud mild counter-irritation. Therefore the results of the treatment in the class of cases which has come under Dr. Beunett's management, are not comparable with the results of the treatment of the acute peripneumonia of Cullen and others, unless every principle of correct reasoning be violated. Consequently, the favorable rate of mortality in Dr. Bennett's cases will not authorize the conclusion that it has been owing to the peculiarities of his treatment. It has evidently arisen from the mild character of his cases. At all events, no matter how great the contrast may be between the rate of Dr. Beuuett's mortality aud that of his predecessors, it will prove nothing in favor of his views, in consequence of the great dissimilarity that must have existed between the character of the cases which they respectively treated. Again, we are not quite certain that implicit confidence can be placed on the correctness of Dr. Bennett's diagnosis in all the sixty-two cases of recovery. At another part of this paper, I have quoted from one of his clinical lectures two instauces at least, in which he formed an erroneous diagnosis. Taking into consideration the *nature* of these mistakes, I think it is quite legitimate to inquire how many similar errors may have been made in the sixty-two cases of recovery.

2. *The amount of confidence to be placed in the statistics of the results of Dr. Bennett's treatment.*—It must not be imagined for a moment that I mean to impugn Dr. Bennett's veracity. But I do iutend to show that such great inconsistencies exist between certain important statements contained in his present paper, and others which he has previously published, that unless he condescends to reconcile these contradictions, he cannot expect the profession to place ou his statistics that amount of reliance which is necessary to carry conviction, that these results establish the truth of his propositions.

ORIGINAL COMMUNICATIONS.

(Continued from page 564.)

ART. X.—*Outlines of Lectures on Yellow Fever:* By JAMES JONES, M. D., Professor of the Practice of Medicine in the University of Louisiana.

(Continued from the July No.)

AFTER the most rigorous examination of these extraordinary investigations, we are compelled to admit that the exact and regular modes of the extension and propagation of yellow fever have not been established to the satisfaction of the intelligent members of the profession, and that our knowledge of the real and efficient etiological element is yet within the confines of speculation.

There are several accepted modes of dissemination by which epidemic maladies are held to extend their influence. A morbid atmospheric constitution, exhalations from fixed sources of contamination, emanations into mediums in which they are capable of multiplication and reproduction, properly called infection, and contagion, in which the toxic element is reproduced in the person of the recipient. Few diseases are limited to any single mode of propagation, although yellow fever has been considered by nearly all who have treated of its modes of invasion, as a disease disseminated solely and exclusively by some one of the modes enumerated. The exclusive dogmas of its local and of its foreign origin, of its incommunicable, of its infectious, and of it contagious transmission, have been maintained, both within and without the profession, with a zeal and an acrimony unexampled in the history of medical science.

For full and accurate details of all the facts and arguments adduced in support of the various positions assumed in these controversies, I refer you to the 2d volume of Dr. LaRoche's great work, and to the very elaborate and logical chapter in the 2d part of the late Professor Drake's Diseases of the Valley of the Mississippi.

These questions are not mere scientific abstractions; they have a practical bearing upon great social interests, and involve the gravest responsibilities of human health and human life. It is, to me, extraordinary that, on issues so momentous, and in the midst of conflicting

evidence on each side equally respectable and credible, that we will continue to insist so exclusively and dogmatically on the truth of one or the other of these doctrines, in which there is nothing apparently too antagonistic to admit of reconciliation, and nothing in the averred facts positively incompatible. I do not myself believe that the miasm of yellow fever is ever generated, any more than I believe that the spores and spermatoid corpuscles that I observe in black vomit, from the moment of its ejection, continuing hourly to manifest the evidences of their growth and reproduction, are the products of spontaneous generation.

In the earlier period of this controversy, Lind, Fodéré, Humboldt, Pariset, Burnett, Berthe, and others, then and since, equally eminent, both in Europe and in America, advocated what has been called the doctrine of contingent contagion, which meant, according to the greater number, that yellow fever is not transmissible by contagion in a pure atmosphere, but often acquires that power in one already contaminated, and that it possesses in the temperate zones and in Europe a facility of communication that it does not exhibit in its inter-tropical habitats in America. Whether suggested and maintained as a scientific truth, or offered as a compromise in this war of opinions, it is truly remarkable that this doctrine has been more violently assailed by the advocates for a local origin, than that of unqualified contagion. Their own hypothesis resting on a combination of the contingencies of heat, moisture, marshes, filth and putrefaction, should not have repudiated another elaborated from the same material and predicated on similar accessories. It is impossible to analyze in this place the value or the propriety even of the term of contingent contagion. It is neither impossible nor improbable that molecules may emanate from the bodies of the febricitants, which, in one medium may be barren or abortive, and in another, fecund and potent.

We admit, therefore, that this disease, in its endemic forms, is, in many localities, of a local origin, and limited to the sources of its production ; that individuals affected with its worst symptoms, may sicken and die among the unprotected, without the risk of communication, and that it will not be otherwise conveyed by fomites or other ordinary media of propagation.

We admit, also, that when thus apparently generated, either in cities or in ships, that it cannot be propagated, under any circum-

stances, beyond the 46° of north latitude ; so that in New England and Canada in America, and in England, and in Europe generally, except certain parts of France, Spain, Italy and Portugal, there is no necessity for any species of quarantine or other barriers to intercourse. Further, that in the temperate zones, in America and Europe, previously to the middle of May, and after the beginning of November, it cannot spread from one point of a city to another, and cannot be propagated by any mode of infection or contagion.

II. We admit that, when generated in the ordinary established localities, on land, it frequently, by its own mode of infection, is communicated to, and re-generated and multiplied on ships, steamboats, and all their organic materials and contents ; and that, when generated either from this cause or spontaneously in ships or their contents, it may be carried and communicated, in proper seasons, to localities favorable to its propagation. That, therefore, there are phases in the communicability of this disease not always to be satisfactorily established ; that, although the crew and passengers of a ship may be healthy in transitu, the disease my break out on opening the hatches and cargo ; that, in adopting a strict theory of quarantine, no ships or steamers coming to or from ports subject to this disease, or passing through the inter-tropical and southern seas and gulfs in which the disease may be generated, should be permitted to land without being examined, ventilated and subjected to such other detention and purification as the circumstances may require. When the disease exhibits a disposition to extend from cities into villages and rural districts, the unprotected should avoid all exposure to proximity of boats and baggage cars coming directly from infected localities. While a regard for the respectable evidence in favor of the infectious character often assumed by this disease, has demanded the institution of quarantine regulations, to limit its propagation in localities exposed to its invasion, it must be admitted that their protection has not been generally efficient ; that they can only operate against one mode of its production ; that they are opposed by many on strong doctrinal grounds, and by others on interested considerations, who impede their execution ; that they have never properly accomplished the objects of their most ardent advocates, and have made little compensation for the expense, the delay and the loss with which they are necessarily attended.

III. There are strong advocates for the uniform contagiousness of

this fever—persons who advise not only the quarantine of ships and of cargoes, but the detention of the sick and of the well, on infected vessels. In this community, for the quarter of a century preceding the great epidemic of 1853, the proportion of the respectable members of the profession who believed in its infectious disposition, was limited, although many of those in the interior had adduced strong evidence to establish that it had been communicated inland by steamboats and other fomites. No one with whom I was acquainted up to that time, reposed much confidence in the assertion that it was or had been contagious elsewhere. In that fatal year this disease appeared before the usual period of its invasion. It ceased to be, as generally acknowledged, a civic malady, but raged with fearful yet unequal mortality for hundreds of miles in the country, weeks after it appeared in the city; and there was the most indisputable and respectable professional testimony to establish that it was conveyed from place to place, and propagated by the sick, similarly to the most contagious maladies. Few pretend that this was the only or the general mode of its extension. That there was an epidemic atmospheric constitution, was amply established by the wide circles in which it had been ranging, from Brazil, where it was previously unknown, and all the intervening regions on the Spanish main, the Caribbean Sea, the south Atlantic and the Gulf of Mexico. This epidemic exhibited so many novel features—in the peculiarity and gravity of its symptoms, in the modes and extent of its diffusion, and its severe and often fatal effects on subjects considered hitherto partially protected—that experienced and respectable men have hesitated in pronouncing it yellow fever, although it certainly was entitled to no other appellation.

This leads us, therefore, gentlemen, to examine, as has been long since done by others, whether, in these unceasing controversies concerning its origin and propagation, we are always disputing about the same types and forms of the disease. There are diseases ordinarily benign, which, from contingencies more or less unknown, may become malignant, and even contagious; and thus, by adopting the somewhat ambiguous doctrine of morbid elements, as promulgated by the Montpelier school, it may be assumed that there is no pathological or etiological accessory that may not be associated with the original malady.

In reference to the undoubted agency of ships in developing and

transporting this and other diseases, there is a wide field for improvements in naval hygiene. There is an absolute necessity for a proper and efficient arrangement in the construction of ships, and the disposition of the cargo, by which they shall admit of free ventilation and be no longer exposed to the accumulation of foul and putrescent bilge-water, and by which the timber used shall be not only perfectly seasoned, but protected from decay by the various mineral salts in common use, and covered with paint, tar, or other suitable material, to prevent the absorption and generation of miasms.

On the appearance of epidemic yellow fever, the only efficient sanitary expedient is the immediate removal of the unprotected beyond the reach of infection. No persons in this category, and particularly no immigrants lately landed, should be permitted to enter a locality invaded by this disease. The millions so generously lavished in the attendance and interment of the unfortunate strangers in our Southern cities, would have carried thousands safely beyond the influence of this terrible malady—the young, ardent and enterprising, whose lives are an irreparable loss to the country, and whose sickness and death inflict an indelible discredit on the reputation of our southern communities.

It has been well established, that the neglect of local hygienic measures is an important accessory to the development of this disease. During the warm months, at least, every species of offal and putrescent refuse should be removed to a distance from localities exposed to visitations of yellow fever, not only in the morning, but in the evening. The putrefaction of the night is, probably, worse than that of the day, particularly from the absence of upper currents and the constant precipitation. Every foul place should be purified and fumigated, every close one ventilated and insolated. All privies should be thoroughly emptied on the approach of the epidemic months, and deodorized weekly by the chlorides of lime or of zinc, or by the sulphate of iron, during the whole period. In addition to chlorine and ordinary fumigations used in ships and infected districts, in the belief that there is vitality in this miasm, I would use sulphurous acid, easily generated by burning sulphur, one of the oldest antiseptics, highly praised by Boerhaave and Van Swieten, and known both in science and in the arts as the most efficient extinguisher of organic life and of every species of fermentation.

With these brief and limited remarks on the modes of propagation assigned to this fever, which at different seasons and localities may be either singly or collectively operative in its extension, we hasten to conclude this introduction by some observations on the nature of the efficient etiological element by which it is presumed to be generated.

It is scarcely necessary to premise, that almost every hypothesis conceived in the matrix of speculative pathology, to reveal the causation of other pyrexiæ, has been confidently adduced to elucidate the production of this. To specify each, would be to traverse the wide field of general pyretology. In the limited survey we have proposed to institute—after what has been already indicated under the heads of the geographical and climatic habitats, and of the several meteorological and local conditions presumed to be indispensable in the generation of this fever, it will be unnecessary to repeat the arguments intended to demonstrate that heat, moisture, vegetable and animal putrefaction, and other familiar and common agents, are not themselves, either individually or collectively, capable of its evolution. That, from all we know of the status of the unprotected, the constitutional changes manifested in them, from such conditions operating at certain seasons in yellow fever latitudes, are insufficient for a philosophical rationale of the morbid phenomena, and that the application of acclimation as a term expressive of their presumed pathological effects, is founded in a partial and imperfect appreciation of well known and established facts.

The inefficience of mere atmospheric influences for the production of febrile diseases, was early recognized by intelligent etiologists, of whom some inferred, from the great variety of morbid conditions presumptively thus derived, the existence of peculiar exhalations to which these influences should properly be attributed. We do not propose to examine the telluric, volcanic, and other specified sources of atmospheric contamination, nor the supposed mineral characters of these morbiferous elements, as imagined by Sydenham and his contemporaries, or as they have been subsequently invoked by others of authority, as the active and efficient elements in the origin of yellow fever. Regarded either in reference to their frequent and doubtful sources, or studied, either separately or collectively, as the special and efficient cause of this disease, no gaseous emanations will probably ever be recognized capable of exercising the functions of this

obscure influence. *Carbonic acid* and *oxide, hydrogen,* in carburets, sulphurets, and phosphorets, or in its combination with nitrogen in ammonia ; *nitrogen,* as Mitchell termed it, septon, combined in all its forms either of oxide or acid, with oxygen—these and all other gases proposed by various authors, subject alike to all the physical and chemical laws of penetration and diffusion, difficult of confinement, limited in their sources, rarely permanent in their composition, generally incapable of becoming fixed fomites, and not always proved to be present, scarcely merit a serious examination. In their dilute forms, few of them enjoy any toxic, far less febriferous properties, in their more concentrated, they possess none to which the protected and the non-protected, the acclimated and the non-acclimated, the native and the stranger, are not, under all circumstances, eqnally susceptible.

The gaseous products of animal and vegetable decomposition, associated probably with vesicular vapor, carry with them certain volatile and putrescent organic elements, the sources of those offensive and maliferous properties to which Magendie and others have ascribed the origin of this fever. The suggestions of Magendie were founded on the injection of putrid liquids into the veins of animals, of which the symptoms and the morbid appearances were said to be identical with those of yellow fever in man. These experiments were carefully and successfully repeated by the late Professor Harrison, of this University, who, in according with the opinions of Magendie, announced, farther, that he believed the efficient morbid element of this disease to be an organic product of night-soil or other effete material, made soluble in the atmosphere by means of ammonia. The results of these observations in experimental pathology are interesting and instructive, and the hypothesis by no means untenable, the question of most importance remaining to be determined being as to the dead or the vital nature of the ferment thus introduced.

The doctrine of fermentation, in which that complex process is applied to the evolution of fever, is almost coëval with the history of medical science. Regarded in its most comprehensive application, associated with certain changes in the non-morphological organic elements both within and without the system, as with its more familiar transformations, this Protean influence offers to my mind greater facilities for resolving the obscure evolution of this and other epidemic maladies than any other, of whatever chronology or fashion, proposed for a similar object. Shadowed forth obscurely by the Greek

schools, adopted and promulgated by Sydenham, it has reappeared under the auspices of Liebig, as a scientific solution of febrile phenomena worthy of mature consideration. You are already aware, gentlemen, that in one of his reports to the British Parliament, Mr. Farr has given to certain epidemic and febrile maladies, as a class, the title of zymotic—to the morbid fermentation to which they owe their theoretical action, that of zymosis or fermentation, and to the hypothetical ferment by which they are generated, that of zuma. According to our ancient authorities, the zumas, or elementary ferments, of certain maladies, are hereditary and congenital, remaining inactive in the organism until brought into operation by external influences. According to the school of Giessen, the elements or bases of the zymosis are of similar origin, remaining neutral and inert in the fluids until excited into action by ferments from without, they are generally forever exhausted, so that, in certain diseases, non-recurrence is thereby perfectly established.

What is the nature of this ferment? Whether preëxisting in the system, or introduced from without, it is a remarkable fact in reference to this question, that the germs of seeds and germs of disease, have been both professionally and popularly used, in a figurative and in a literal sense, to convey an idea of the morbid element. It is thus, from the days of Paracelsus, we observe that in the reception and propagation more particularly of the exanthemata, so many have conceived the notion of the parasitic nature of diseases, and have imagined, in their origin and development, the indication of special forms of living miasms in specific maladies, to which the discovery of the microscope gave a palpable support. Kircher first boldly professed that all such diseases were generated by animalcules; to which etiological doctrine Linnæus, the great botanist and naturalist, and his pupil, Lysander, gave, in after years, a more extensive application. The essays in which this was promulgated, appeared under the expressive title of *pathologia viva*—the origin and development of diseases from living organisms. In the Notes and Reflections of Holland, you will find an exceedingly interesting chapter on the animalcular origin of epidemics, and in the March No. of the New Orleans Med. and Surg. Journal for 1848, you will read an elaborate and masterly article from Professor Josiah C. Nott, of Mobile, on the application of this doctrine to the origin of yellow fever. Let us not forget Raspail. An empiric in medicine, as in politics and ethics, he has combined more learning

and science in his curious treatise on the Natural History of Health and of Disease, devoted particularly to parasitic pathology, than can be found in any similar work in any language.

The last authorities to which I will refer you, are the ingenious and learned little work on the cryptogamic origin of fevers, by Prof. John K. Mitchell, of Philadelphia, and the late and more elaborate treatise on the same subject, by Groves, taken principally from Dr. Mitchell, who is the author of everything hitherto suggested on the cryptogamic theory, which I bring to your notice as we proceed, because it is not to be met with in any of our systematic works.

I have long contended that the introduction of the hypothetical action or existence of microscopic organisms, whether animal or vegetable, to explain the origin of epidemic diseases, was a narrow and partial view of a very extensive series of phenomena; and that it is only by studying these morbiferous germs in connection with fermentation, of which they are the evidence, the products, and in some forms the generators, that we can give proper value to their etiological influence. It is not my habit, nor is it my duty, in this chair, to dwell on subjects properly belonging to general pathology, yet in relation to this particular doctrine, although it be one neither fully established nor capable of positive proof, the interest of the subject will justify a few observations.

It has been repeatedly demonstrated, that certain fermentations are always, if not necessarily, associated with the evolution of various well-defined living organisms, which are thus incredibly developed and multiplied and become capable, when transported into proper media, of renewing and perpetuating similar processes.

It has been long held that in epidemic climatic constitutions, there is a peculiar corruptive, or, what Craigie aptly terms a febriferous state of the atmosphere, which is not the effect, but the cause of decay and putrefaction in organic matter, and of the peculiar condition of man and of animals associated with their prevalence. While it is difficult to conceive of an atmosphere charged with foreign elements sufficient for the basis of putrefactive fermentation, there is abundant evidence to prove that it may contain the minute corpuscles of ferments in adequate quantity for the generation and maintenance of that process in all organic materials exposed to its action. During our summer showers in New Orleans, palpable stains are left by the rain-drops on

the surface of glass, which exhibit, under the microscope, cryptogams and spores, in various stages of development. These are, doubtless, reproduced and developed to a considerable extent in the moisture of the air, but find their proper nidus on the surface of the soil, on water more or less impure or stagnant, on wood, and other similar material, and on every deposit of offal and of putrescent exuviæ, generated by the accumulation of men and of animals.

Although these organisms and their germs have not been sufficiently studied to establish their exact forms or their proper relations to individual types of fever, that cryptogams, in spores, in spermatoid corpuscles, and in progressive stages of evolution, are always present in black vomit and in the tissues after death, I am perfectly convinced from my own repeated observations.

The facile communicability of the miasm of this disease to ships, its ready production and development in similar structures from causes apparently spontaneous, its attachment to the organic constituents and contents of the holds and cabins, its preference for wooden tenements, as indicated by Rush, and so often established by our own observations, its concentrated activity on ground floors, so often in decay, and in all dark, damp and illy ventilated spots, is peculiarly characteristic of its cryptogamic nature, as contended for by Dr. Mitchell. Its tenacious adhesion to fomites, of every form, its supposed increment in boxes, trunks and bales, are to be included in a similar category.

It has been observed in various epidemic visitations, that they are attended by many novel forms of organisms, belonging both to vegetable and animal life. Among the most remarkable here, in the great epidemic of 1853, were the mildews and moulds that covered every bound volume in our libraries, and made the very vases and candlesticks adhere to the tables.

There are facts in the geographical and climatic habits of plants very corroborative of the cryptogamic hypothesis, in their correspondence with the propagation and decline of yellow fever. Many, as we know, bud, blossom, fructify, and decay, at unequal temperatures, at the South and at the North. Many hibernate or decay in the torrid zone at a winter temperature of 50° or even 60°, when they require a freezing point at the North ; just as we find yellow fever decline at a similar point in the scale in Vera Cruz and Havana, and sometimes defy a frost in certain parts of the temperate zones.

Cryptogams are generally more rapidly developed at night. The putrefaction of the night is also more rapid and often more offensive than that of the day. It is at night that the miasm of this disease is most potent; at night that the majority sicken and present their worst changes, and at night, I am informed, that the greater number die in the hospital.

When exhumed from their old repositories, the germs of small pox revive to desolate new generations, just as the removal of old growths from the soil, and excavations beneath its surface bring out the germination of long hidden seeds. Thus, also, has it been repeatedly demonstrated that in the zones exposed to visitations of yellow fever, extensive disturbances of the earth in which its sporules or ova have been long dormant, have been productive of most fatal and extensive epidemics.

There are, as you all well know, diseases of the widest extension and most destructive character, epiphytic maladies, falling at different periods upon certain parts or on all of the vegetable creation within districts of country more or less limited. Forest and fruit trees, wheat, corn, oats, tobacco, cotton and cane, the grape vine, the potato, the beet—every valuable agricultural product, has been thus frequently injured or destroyed. I will not consume your time by naming the insects and the parasitic cryptogamia universally found to be present and active in those scourges of the farmer and of the planter. There are similar diseases, in their extent and injury, epizoötics, as we term them, prevailing frequently among horses, cattle, sheep, dogs, poultry, and game, which scientific investigations have likewise traced equally to the influence of parasitic vegetable and animal growths, doubtlessly often associated with peculiar atmospheric conditions.

Insects themselves, some of them the most useful, silk worms and bees, are subject to epizoötic diseases, all of which are of cryptogamic production. The very flies, in epidemic seasons, often turn white and die, covered with cryptogams, suffering alike with domestic animals during similar periods. I was informed many years ago, by an intelligent mechanic, frequently engaged in building vaults for the dead, that during our yellow fever epidemics the cemeteries were full of dead flies. While correcting this for the press, in August, 1858, I notice that the flies have nearly disappeared since the prevalence of the epidemic now in progress.

These facts, gentlemen, from comparative pathology, as exhibited in the diseases of plants and of animals, afford strong analogical data for the admission of the same efficient and active organic elements in the production of epidemics, in the absence of positive proof of any other established or known morbid agents. Modern science, however, has demonstrated that many familiar diseases, hitherto ascribed to unknown diatheses or constitutional derangements, are clearly referable to animal or vegetable parasites. This is not the place to enter upon a history of acarides and entozoä, or of their constant association with many of our external and internal maladies. Nor will I indicate here those microscopic vegetations so well known to play a principal part in the etiology of many diseases of the skin, technically dermophytes, nor of similar organisms found in certain grave and contagious diseases of the mucous tracts, farther than to remark that they open the widest field for the installation of some new, more positive and more satisfactory principles, both in etiology and in pathology, and for some more rational processes in prophylaxis and treatment, worthy of the most active scientific investigations.

The poisonous properties of fungi are universally admitted. Various sporadic and epidemic maladies, both in man and in animals, are produced by their ingestion and by that of damaged and decaying aliments known to contain them. That their action was long since adduced to explain the origin of fevers, is evident from the terms in which it is combatted by Baglivi. That their toxic effects, if not their elements are multiplied and regenerated in the organs of the subjects, has been well inferred from the poisonous properties of their secretions and also of their flesh.

It has been amply proved by the observations of Hassall and of his predecessors, that plants and fruit may be successfully inoculated with cryptogams or their sporules, of which the result is rapid disease or decay. It has been also asserted that, under the microscope, the vaccine and variolous virus exhibits corpuscles similar to spores of cryptogams. Magendie and Harrison having produced, as I informed you, pathognomonic symptoms of yellow fever in animals, by putrid injections into the veins, it would be interesting to know the effects of similar fluids under the skin, and, when death does not ensue, their disposition to recurrence by the repetition of the process.

In the latter part of his interesting work on Yellow Fever and Dis-

eases of the Tropics, Leblonde threw out some valuable hints on the nature of miasms and on the utility of inoculating with different animal poisons for the prevention and cure of disease.

"Those," says he, "who have traveled in Africa and in French "Guiana, have doubtless heard tell that they inoculate there against "the serpent, and that by this process the bite of the most venemous "is without effect upon the persons thus inoculated. I have been "many times a witness that the wounded parts did not even inflame." He gives cases of goitres, elephantiases and yaws, cured by the stings and bites of insects and reptiles. He recommends the venom of the rattlesnake as a cure for various maladies. Bally, also, in his great work, published ten years later, found an extraordinary resemblance between the effects of this fever and those of the poison of the viper.

In 1853, Dr. Humboldt, a German, who had practised several years in Mexico, appeared in this city as the author of a mode of inoculation with the poison of certain serpents, by which of 1,438 unprotected persons said to be operated on by him in Mexico, only seven had sickened, two of them fatally, when exposed to yellow fever. He addressed a letter to the Captain-General of Cuba, in 1854, in which he also observed that he had inoculated 385 in New Orleans, of whom none, to his knowledge, had taken the fever. He was invited to visit Havana; the wards and assistants in the Military Hospital were put under his entire control, and all unprotected soldiers and marines who voluntarily submitted, were the authorized subjects of his treatment. Dr. Manzini, of Havana, Humboldt's principal operator, has just published, at Paris, a full account of all that transpired, and has attached, in the appendix, the official report of Bastarreche, the head of the Medical Military Bureau of Cuba. 2,477 were inoculated. Manzini and Bastarreche agree in their description of the effects of the inoculation, the former having entered into longer and more definite details. The local effects were slight. After an average incubation of seven hours appeared alteration and generally reduction of the pulse, lassitude, articular and muscular pains, redness of the face and eyes, febrile heat, tumefaction, redness and hemorrhage of the gums, pain in the back, head-ache, and occasionally a yellowness of the skin, and other symptoms resembling those of this fever, terminating always in health. It was admitted by both parties, that of these 2,477, sixty-seven died of the fever; but, whereas Bastarreche, in his official report,

gives the whole number of those who had the fever as 288, of which said sixty-seven died, Humboldt and Manzini contended that 701 had the fever, of which these sixty-seven only died. By Bastarreche's report, the proportionate mortality of the inoculated who took the fever was considerably greater than that of the uninoculated. It is a fair and, I have no doubt, a true report. The most interesting point is, that only between a ninth and tenth of the inoculated had the fever. Bastarreche, therefore, while informing the Captain-General, that no advantage had accrued from the process, eventually seems to admit that it afforded a certain degree of protection.

(To be continued.)

ART. XI.—*The Remedial Action of Premature Delivery in certain Gastric Derangements during Pregnancy:* By A. H. CENAS, M. D., Professor of Obstetrics in the Medical Department of the University of Louisiana.

OF the disorders of the pregnant state, those of the stomach are the most frequent and distressing, and likewise those that most often compromise the safety of the patient.

Nausea and vomiting generally accompany pregnancy, and are more or less troublesome up to a certain period, viz: that of quickening, after which they disappear, and the patient passes the rest of her time in comparative comfort. But in some cases, the disturbance of the stomach not only persists, but increasing in intensity incoërcible vomiting rejects everything from the stomach as soon as taken, and the patient becoming exhausted from inanition may die if something be not done to rescue her.

The resource that we have at command under such circumstances, and that may rescue the patient, is premature artificial delivery. A resource which from its first introduction by Denman up to the present time, has been resorted to sufficiently often, to constitute it one of the occasional necessary expedients of practical midwifery, as the following cases show: CASE FIRST—Catharine * * *, native of Germany, aged about 20 years, came to the obstetrical wards of the Charity Hospital on the 25th of February last, in the sixth month of her first pregnancy, and suffering under great and distressing irritability of the stomach. History as follows: Up to the third month was tolerably well, suffering very little from nausea, but since that period, has got

worse, and at the present time, the irritability of the stomach is so great that it will retain nothing.

℞ Bi-Carbonate of Soda, . . ℥iss.
Syrup of Morph. . . . . } ää ℥iss. F. M.
Aqua Laur. Ceras. . . . }

Dose—Teaspoonful occasionally. Diet—Lime-water and milk exclusively.

This treatment seemed to succeed for a while, her nausea and vomiting being considerably diminished, and she was able to get about with tolerable comfort. But shortly after this, about the 10th of March, her bad symptoms returned with redoubled violence, and this time the bowels became implicated, reducing her quickly to the lowest ebb.

Under these circumstances, premature artificial delivery seemed to offer the only chance of safety, by removing the cause of the trouble, viz: uterine irritation; and I at once decided to give her the benefit of this chance, which I did by opening the membranes, using for this purpose a male silver catheter, open at its extremity in order that its stylet might be thrust into the amniotic sac. During the course of the day the patient's condition materially improved; her stomach and bowels became more tranquil, and she was enabled to take a tolerable quantity of light food and wine. But uterine action was not fully established until nearly fifty hours after the operation, at the end of which time, however, she had so far recovered her strength and spirits that she bore the pains and fatigues of labor with comparative ease and success.

The fœtus was, as a matter of course, still-born, and seemed to be about six and a half months old. The mother's recovery was rapid, and she left the hospital about a month afterwards, perfectly well.

This case was otherwise of interest, in establishing a fact connected with the odor of the liquor amnii. It is described as nauseous and semen-like; but, as I drew it off from the amniotic sac, through the catheter, into a tumbler, I could detect no smell whatever; its hue or color was opaline or whey-like.

Case Second.—In the month of August, 1852, I was called to see Mrs B., wife of a German, in good circumstances. She was confined to bed, and so much reduced in flesh and strength by incessant and long continued nausea and vomiting, that she was unable to move without assistance. Her husband informed me that she had borne eight children, and was now in the seventh month of her ninth pregnancy, but had never been similarly affected. Nausea and vomiting

had commenced early, and had gradually increased in severity until nothing, absolutely, would remain on her stomach. It was, literally, a case of starvation, which would certainly terminate fatally in a short time unless something were done to avert it. In my opinion, the only thing to be done was to induce labor; but whether, in her weak state, she could survive the shock of delivery, was doubtful. Nevertheless, with the consent of the husband, and at her own urgent request and solicitation, I resolved to give her the benefit of the doubt, and forthwith punctured the ovum with the styletted catheter. It was remarkable how quickly relief followed the removal of uterine distension; for, as soon as a certain amount of fluid escaped from the womb, the patient expressed herself as better. Nausea and vomiting diminished so far as to allow her to take some light nourishment and stimulus, and her strength rallied to that degree that when labor came on, about eight hours afterwards, she sustained its shock with remarkable fortitude and success. The fœtus, which was about seven months old, was born alive, and survived its birth for nearly one week, and would certainly have continued to live if better care had been bestowed on it; but the parents, under the mistaken notion that it was not viable, seemed indifferent to its fate.

Again, premature artificial delivery may be of signal service in rescuing the unborn fœtus under circumstances where the mother's life is beyond the reach of safety. Women in the last months of pregnancy often succumb under some rapidly fatal malady, carrying with them to the tomb their viable offspring. Under these circumstances, the timely induction of labor is highly feasible, and imperatively called for not only by the dictates of humanity, but also by urgent considerations of family interest, as the following cases show: CASE THIRD —Several years ago, I was called in consultation to see Mrs. M. She was in the last stage of typhoid fever, and in the eighth month of her first pregnancy. Her physician had abandoned all hope of her recovery, and regretted the apparently unavoidable fate of her unborn but viable infant. After consultation, however, and with the consent of all interested, it was decided to give the infant a chance of life, by inducing premature delivery. The membranes were accordingly ruptured, and labor, which came on shortly afterwards, was terminated in less than four hours with entire success to the infant, which was born not only viable but in vigorous condition. The mother did not survive the birth of her infant more than twenty-four hours; but during that time

repeatedly expressed her gratitude in the warmest terms for its unlooked for rescue.

Fourth Case.—About three years ago, Mrs. V., in the seventh month of her fourth pregnancy, was attacked with gastro-enteritis. Without detailing the case, I will say, in spite of all treatment, the disease proceeded to a fatal termination. About forty-eight hours before her death, and in view of rescuing her viable infant, premature artificial delivery was proposed and accepted. Labor was induced, as in the other cases, by puncturing the membranes; was of short duration, and entirely successful to the infant, which, though feeble at birth, was gradually and perfectly recovered. The infants in both of these cases were males, who not only survived the immediate shock of their untimely birth, but have grown to be hale, hardy and intelligent boys.

And now as it regards the *modus operandi* for exciting uterine action and inducing labor: Of all the methods in use, I prefer the puncture of the cyst, not only because it is the most easily and quickly done, but because the removal of the uterine distension by the evacuation of its water, is often followed by the best results as it regards the stomach, quieting its irritability and enabling it to retain a sufficiency of nourishment to rally and prepare the patient for the shocks and sufferings of her subsequent labor.

For puncturing the membranes, several instruments have been invented, among the best of which is Lee's styletted catheter, which instrument consists of a silver canula, about the size and length of a male catheter, open at its extremity and armed with a stylet, projected with a spring. But, as these instruments are not always at hand, they can be readily replaced by a gum-elastic or silver male catheter, open at its extremity and armed with a wire stylet. In the use of these instruments care must be taken not to wound the parts of the mother or the head of the child.

Art. XII.—*An Epistle upon Yellow Fever.*

Raymond, Miss., August 11, 1858.

Dr. Bennet Dowler, *New Orleans, La:* Dear Sir,—I hope you will excuse me for taking the liberty of asking you a few questions upon the subject of the treatment of yellow fever I know that your time is very much occupied with your various duties, and, consequently, I

feel afraid that I am drawing too heavily upon your kindness in requesting you to give me the desired information. If you can do so with convenience, you would oblige me by giving me the general course of treatment pursued by those most successful in the treatment of the disease in question.

Are the patients confined under blankets, to induce and keep up a perspirable state of the skin? or are they allowed simply covering enough to make them comfortable, as in the treatment of other diseases? etc., etc.

Very respectfully, your obed't serv't,

GEO. LATIMER, M. D.

EVERY where in the medical world, and by every body in it, is felt one prevailing hope, one great desiderated finality, namely, a clear, precise, and certain knowledge of the method by which diseases, whether isolated, endemic or epidemic, may be cured. In all ages since the eating of

> "The fruit
> Of that forbidden tree, whose mortal taste
> Brought death into our world and all our wo,"

sages and charlatans, the wise and the unwise, have clearly seen that this is the first, last, only aim of the healing Art, which Hippocrates and his successors for more than two thousand years have labored to achieve;

> "Which kings and prophets waited for
> And sought, but never found."

Yet the experience of the ancients for more than two thousand years has been virtually, almost entirely lost, because they have not transmitted to their successors the precise diagnostics and natural classification of diseases, together with their pathology and therapeutics. What satisfactory information is derivable from Hippocrates' account of his visit to the son of Parion, a fever patient, who died on the 120th day, near the Temple of Diana? or what can be learned in the case of Pythion, who lived above the Temple of Hercules, and whose fever did not reach a crisis until the 100th day? or from the case of Heropythus, whose fever proved fatal on the 10th day? Perhaps the first two cases were typhoid, such as Louis and Chomel have described, and such as I have seen and treated in Virginia; cases which have run on to the 100th day. Who can tell? Had Hippocrates

given the diagnosis, pathology and treatment of these and many other cases of fever to which he alludes, the study of these, and their comparison with febrile maladies in every æra and in every place, would, like a gushing mountain spring, have flowed in a continuous and increasing stream down the declivities of time, irrigating the fields of science.

In reality, medical, not less than cosmical phenomena, may be uniform, fixed and certain; yet, owing to our imperfect knowledge of the complete history of the former, they appear unstable, changeful, irregular, and sometimes altogether uncertain. For the individual student, medicine must be perpetually reïnvestigated, nay, re-created out of the past and the present. The labors and the thoughts of others, will, for him, supersede neither the necessity for experimental labor nor the necessity for laborious thinking.

Would not the same medicine, it may be asked, which cured a Greek, or Roman, cure a Louisianian, or a Mississippian? Undoubtedly; provided the cases be precisely alike, parallel in every condition and direction. But how is this identity to be determined and made manifest beyond mistake? Perhaps no written description can be given of the normal, not to mention the pathological appearance and physiognomy of any person in New Orleans so as to be recognizable by a person in Raymond, Miss. It is still more difficult to portray in a recognizable form the differential diagnoses of diseases and their varying stages and rapid metamorphoses occurring in the same epidemic, for which new combinations of remedial measures may be required, and in which judgment and personal observation are involved. For example, there is in a comparatively limited number of yellow fever cases at the very onset acute pains throughout the whole body, as if the pains of dengue, rheumatism, and choleraïc spasms, were concentrated in one individual. A stout man, otherwise courageous, rolls and screams, and annoys his neighbors. A solution of the sulphate of morphia, if not contraïndicated by some condition of the brain, which, however, is not likely to happen, will, in such a case, afford surprising, almost immediate relief, without interfering with ulterior curative measures. The beneficial effects of opiates are not, however, restricted to such cases alone.

The removal of pain is a gain by no means to be despised. Pain is an element of disease of great significance whether it originate in the

dynamics, or in structural changes of the economy. Its elimination augurs favorably for the ulterior treatment by either the Doctor or the Doctress, *Vis Medicatrix Naturæ*.

But neither my time nor the remaining space of the Journal will admit of going into the remedial plans and measures which I prefer in the treatment of yellow fever.

But it may be said—"we asked for bread, and lo! we have received a stone." Not exactly so; for, if the gentleman who does me the honor to ask "whether the patients are to be confined under blankets," etc., etc., etc., will look into this journal, Vols. XII and XIII, particularly the latter, he will find my views expressed at length upon this part of his inquiry, not only physically, and physiologically, but pathologically and therapeutically, and, without going back beyond the times of the Cæsars for authorities, lo! is it not written in Celsus, (*De Medicina*, l. iii. c. vii.) that at the onset of an ardent fever, even in advance of all internal medication, the patient must be *refrigerated* with oil and water, and be put in a well ventilated chamber where he may have abundance of pure air; he is to be covered lightly not suffocated, literally strangled, by bed-clothes, nor *à fortiori* by blankets and feather beds: "*Si vero ardens febris extorret, nulla medicamenti danda portio est; sed in ipsis accessionibus oleo* (the oil may safely be omitted) *et aqua* REFRIGERANDUS EST, *que miscenda manu sunt, donec albescant; eo conclavi tenendus, quo* MULTUM ET PURUM AEREM TRAHERE POSSIT; NEQUE MULTIS VESTIMENTIS STRANGULANDUS, SED ADMODUM LEVIBUS TANTUM VELANDUS EST." I venture to think that ætiology, physiological chemistry, clinical experience, common sense, and instinct, are on the side of the Roman Hippocrates.

There are several particulars in which physicians without experience in yellow fever are very liable to be mistaken. For example, the diagnostication of this malady in its inception is often difficult for the most experienced, though its advanced stage is easily recognized even from written or verbal descriptions. The prognosis is also most illusory to the uninitiated. It is for such, scarcely possible to admit, that a patient with little or no pain having a full, soft and regular pulse, etc., will, nevertheless, die the same or the following day.

To a well informed practitioner who may never have witnessed yellow fever, there is but little mystery in it not common to other fevers, so far as treatment is concerned; for this, whether right or wrong is

generally simple being with most practitioners directed to the repression or removal of such prominent symptoms as arise during the progress of the malady. Those who look forward to eventualities or to symptoms which must be anticipated and prevented, and, who adopt as fundamental a systematic treatment conformably to their theory of the pathology of the disease, without waiting for actualities, will accomplish their purpose best, if at all, by avoiding, as they now generally do, heroic measures; for, notwithstanding this is formidable fever, yet with rare exceptions neither antiphlogistics nor stimulants are now pushed to a formidable extent upon the merely physical principle or false analogy that in therapy the greater force will overcome the inferior, heavy battalions, weak ones. An apothecary showed me a prescription directing an ounce and a quarter of quinine to be administered by the mouth and by enemata to a gentleman, the day before death from yellow fever; another, assured me that an ounce of this drug in solution was applied to a man's skin in this disease, without having produced any appreciable effect. As to myself I have seen what I have seen, that is, a good deal of quininism. * * *

Now, if calomel be deemed in the early stage of this disease, a good febrifuge aperient, and an alterative promoter of the secretions and excretions—if quinine, opium. and diffusible stimulants be accepted as alterative, tonic, calmative, nervine, diaphoretic, and febrifuge, small doses are preferable. For example, if quininism or saturation be deemed essential, two grain doses will effect this end causing the characteristic affection of the organ of hearing, without the impending dangers to vision, to the brain, etc., which sixty grain doses occasionally produce. At least, such is my opinion.

Without having used large doses of opium in my own practice, yet from what I have seen and learned of the practice of others, it appears that this potent drug in large doses is not only tolerated, but often highly beneficial as a febrifuge, sedative and diaphoretic, etc., in not a few cases of yellow fever, particularly in the extensive practice of Dr. M. Morton Dowler, during the present and several former epidemics of New Orleans. Without his permission or knowledge, I will here say for the information of my Raymond correspondent, that Dr. M. M. D. is now using the drug aforesaid, together with refrigerations of the surface in the hot stage, with cold vinegar, whisky, and water.

In conclusion, may I not allude to a stumbling-block, or rather a truism now much in vogue with some writers, namely, that yellow fever and not a few other fevers and maladies are *self-limited?* True! Everything is limited except eternal duration and infinite space. Yellow fever is neither infinite nor eternal. It is limited, but its limits are very irregular whether fixed by its self-determination or by the determination of another. If yellow fever be *self*-limited, and have a will of its own, it is very *selfish* in not making its limits known; for unless its phenomenal manifestations be understood contrarywise to their actual appearance, neither its symptoms nor duration are uniform, but contingent, being not only variable when left to themselves, but more or less controllable for good or evil by medical treatment. Even small-pox, measles, scarlatina, erysipelas, and several other diseases which are the most uniform in character and duration, and the least amenable to medication, are nevertheless, in all probability often cut short or modified both as it regards their tendency to be protracted, or to end fatally. Suppose the natural tendency of yellow fever is to run its course, ending in death or convalescence in a week, but in certain cases being attended with unusual symptoms and complications more or less controllable by art; again, suppose that this tendency, these unfavorable conditions prolonging the disease and giving it intensity so as to end fatally at last, should be changed, is it not reasonable to suppose that this control if timely exercised, would often shorten or prevent this otherwise persisting and fatal tendency inherent in the original malady, or arising accidentally during its progress? To control symptoms is to control results and duration.

While, on the one hand, bad treatment may accelerate the progress and fatal termination of yellow fever; on the other hand good treatment may often shorten its duration, and even cure the patient. It may be well to throw a little cold water on the too ardent zeal of those who profess ability to cut up the disease root and branch, forthwith; but the alleged doctrine of the self-limitation of fevers as a reason for not attempting to control or cure them, is in itself an affected hypothesis, being withal calculated to bring despair to the practitioner and to fill the public mind with an unwarrantable distrust in regard to the efficiency and utility of the medical profession. The physician should not accept the captivating phrases of medical skeptics, concerning *self-limitation* as a safe theory either in medical or

ethical conduct with reference to persons and families who commit under God, their health and lives to the hands of medical men.

This doctrine of self-limitation, which is almost synonymous with therapeutic inaction, generally is accepted neither by the surgeon nor obstetrician; these latter, not the blind diseases and self-murderous aberrations of the disordered economy, undertake themselves to do the work of limitation.

BENNET DOWLER.

TO OUR SUBSCRIBERS.

WITH the present number of this Journal every subscriber will find enclosed a bill made out from the date to which his name is now credited upon our books. These accounts are intended as a "gentle hint" to many of what *our* necessities, and *their* credit demand, which it is to be hoped will be duly appreciated. To others they are sent simply as a statement of their account with us to enable them to correct such errors as may still exist upon our record.

Such was the *culpable* negligence with which the books of this Journal had been kept prior to the past year, that when they came into the possession of the present proprietors, they were filled with the grossest errors of every description. It has been our most disagreeable duty to bring some order out of this chaos, in which we have as yet but partially succeeded; although we have arranged a large number of long standing accounts, erased many "dead-heads" from our list, and added to it nearly two hundred new subscribers in less than twelve months.

To obviate all future misunderstandings with our patrons upon questions of dollars and cents, we earnestly entreat them to inform us forthwith if the bill enclosed to each be found incorrect; and assure them that their *positive* assertions as confrères and gentlemen will be received as better testimony than our Record, and that their names shall be credited upon it to such date as they may acknowledge themselves to be *justly* our debtors. Should they not avail themselves of this opportunity to rectify the injustice which may have been done them by our predecessors, who handing their false accounts down to us have thus caused them to be innocently continued, (so far as we are

concerned,) they must not in future hold us responsible for the annoyance to which they may be subjected by unjust duns.

So great is the expense for publishing this Journal, that we have as yet received nothing in remmneration for our own labor, although a sufficient number of our friends have put their hands honestly in their pockets, to enable us to keep ours out of our own. But if our debtors did themselves and us but meagre justice, this would be far different. For should they all by chance be animated by the same laudable purpose which we know prompts many of them, they would have the satisfaction of acquitting themselves of the wrong done us, (thereby putting money honorably and laboriously earned in our purse,) and of soon receiving the New Orleans Medical and Surgical Journal not only enlarged, but also improved in both typographical and literary excellence.

SANFORD CHAILLE, M. D.
Business Manager N. O. Med. and Surg. Journal.

Mortality Statistics of New Orleans, May 29th to August 22d, 1858, compiled from the Weekly Reports politely furnished by Dr. Baldwin. Secretary of the Board of Health. Population of New Orleans estimated at from 150,000 to 200,000.

Time.	*Total Deaths.*	*Children under 2 yrs.*	*Under 20.*	*U. States.*
June (4 weeks)	531	252	334	375
July (5 weeks)	813	247	380	419
August (3 weeks)	1240	154	385	470

Principal Diseases.	*June (4 weeks.)*	*July (5 weeks.)*	*August (3 weeks.)*
Still born	21	32	19
Tris. Nascent	17	18	9
Cholera Infantum	29	12	6
Infant Convulsions	46	37	50
Infant. Marasmus	25	17	10
Teething	31	24	21
Croup	9	5	11
Scarlatina	2	11	5
Rubeola	1	1	0
Variola	6	6	3
Diarrhœa and Dysentery	54	70	38
Gas. Enteritis	13	6	7
Inflammation of Liver	2	8	4
Inflammation of Lungs	9	9	10
Consumption	46	78	44
Apoplexy	9	13	10
Congestion of Brain	10	7	20
Fever Typhoid	7	15	25
" Miasmatic	13	49	43
" Yellow	2	132	738

Since June 20th the deaths from Yellow Fever have been each succeeding week as follows, 2, 8, 9, 20, 25, 70, 140, 286, 312.

CHAILLÉ.

THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL
FOR NOVEMBER, 1858.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the use of the Iodide of Potassium in the treatment of Miasmatic Enlargement of the Spleen, or Ague Cake:* By HUMPHREY PEAKE, M. D., of Arkadelphia, Ark.

I AM induced to write this article from not being aware of any satisfactory and successful plan of treating miasmatic enlargement of the spleen, as laid down in any of the standard works on the Practice of Medicine. The articles, in these various works, upon splenitis apply but illy to this subject. I desire to speak of that particular enlargement of the spleen, caused, evidently, by the same poison which produces intermittent fever, and in designating it by the term *miasmatic enlargement*, or the more familiar name of *ague cake*, I shall run no risk of being misunderstood. The question of its pathology I shall leave untouched. As to its diagnosis, the books describe it well. No one, in fact, who knows anything could well mistake a case, or fail to make it out.

I have just taken from my shelves the works on the Practice of Medicine by Dunglison, Dickson, Bell and Stokes, Barlow, Bright and Addison, Good, Graves, Gregory, Hooper, McIntosh, Wood, and Watson, and looked over their treatment of this affection. All of these works seem to favor, and recommend a purgative plan of treatment, in conjunction with leechings, cuppings, blisterings, etc., over the affected organ. Bitters, and some of the preparations of iron are generally combined with the purgatives. This was the plan adopted

by Mr. Twining, whose experience was gleaned in India, where the disease frequently occurred. Most of the works named give his treatment *in extenso* with the formula for his celebrated spleen mixture. Dr. Dickson says that he cannot recommend any particular plan of treatment for this troublesome affection, but thinks he has found most benefit from the use of iodine combined with mercury, a soluble condition of the bowels being maintained the while. The different iodides, particularly those of iron and mercury, seem to have proven most successful in the hands of many. But it is needless that I should enumerate the various articles which have been used; what I wish to do is to call attention to the use of the iodide of potassium. Dr. Dunglison is the first American author who speaks of the use of this drug in enlargement of the spleen, and says, that it may be prescribed in these cases in the dose of two grains three times a day; the dose to be gradually increased. Dr. Gregory, whose excellent work on the Practice of Medicine has never, I believe, been republished in this country,* says, in reference to the same subject, that the preparations of iodine, particularly the iodide of potassium, in doses of from five to eight grains three times a day have been highly extolled. Dr. Hooper in his "Manual of the Principles and Practice of Medicine," and Dr. Barlow in his "Practice of Medicine," also speak favorably of the use of the same article in the affection under consideration. I was first led to the almost exclusive use of this drug in the treatment of these cases, from using it as directed in an article by E. F. Sankey, in the "Association Medical Journal," and republished in the July number for 1856, of the New Orleans Medical and Surgical Journal, for the treatment of intermittent fever. The cases in which, from having read the aforesaid article, I had concluded to use the iodide of potassium, were some in which, in connection with enlarged spleen, was an ague which returned several times shortly after having been arrested with the disulphate of quinine. To my entire satisfaction there was no return of the chill, and the ague cake soon disappeared. This happened in four cases. There came a man to me who had had an immense enlargement of the spleen for over two years. He had consulted several physicians and had been treated by them in the usual manner without relief. His spleen extended far below

* This work, *Elements of the Theory and Practice of Physic:* By George Gregory, M. D., with notes by Drs. Potter and Calhoun, (in 2 vols. 8vo.) was republished by Hogan & Co., Phila: 1831.—B. D.

the crest of the ilium when in recumbent posture, and reached considerably beyond the median line of the abdomen. I put him upon the use of the iodide of potassium, and iron, and in less than a month his spleen had regained its normal size, and he excellent health, which for two years had been very poor.

Since that time I have treated a number of cases in like manner, and with the most complete success. My practice being in a malarial district where these cakes are common, and it having been told in the neighborhood that I could cure them, most of them have fallen to my lot. I now prescribe for them with nearly as sanguine hopes of success as I do for ordinary cases of chill and fever, forty-nine out of fifty of which I cure, mainly with the disulphate of quinine. A remark I wish to make concerning those cases is this: The worst I have ever seen were in persons whom, while they have been continually exposed to malarial influences, have, nevertheless, suffered but slightly, and occasionally from the more ordinary symptoms (chills, etc.,) of malarial poisoning. My ordinary prescription is the following :

℞. Potassii iodidi, ʒvi.
Spiritûs lavendulæ compositi, ʒiv.
Aquæ cinnamomi, ℥vii. M. S. A teaspoonful 3 times a day.

I generally give the following preparation of iron at the same time:

℞. Ferri et quiniæ citratis ʒii.
Confectiones rosarum, q. s. Make 60 pills. S. Two to be taken three times a day.

Sometimes I order a dose of blue pill to be taken twice a week. Four weeks nearly always suffices for a cure even when the cases have been of a year's standing. Not unfrequently they are entirely cured in two weeks.

Art. II.—*A case of Poisoning from drinking Whisky, successfully treated by Stimulation and Revulsion:* By Alex. H. Manford, M. D., Bayou Bœuf, La.

On 2nd July, J., a pupil in the Academy of L. J. B. Shaw, Bayou Bœuf, St. Landry Parish, whilst engaged with the citizens of the vicinity in preparing for a Fourth of July celebration, drank freely of a very common article of whisky; was soon under its influence and

feel asleep (as the company thought) early in the day. A rain coming on the people dispersed leaving J., on the site of recent operations exposed to descending torrents.

I had occasion to visit the said site in the evening, and discovered J., lying under the rostra erected for the occupancy of the orators, having been placed under this imperfect shed by some considerate negro. Over his features was spread a fearful pallor. I knelt down and discovered him pulseless at the wrists ; the heart almost without action, and the lungs performing their functions very imperfectly from the stagnation of blood within their substance. The body was covered with a cold clammy sweat—there being evident paralysis of the cutaneous vessels attended with a lethargy similar to that often seen in a bad case of pneumonia typhoides. I had him carried to the nearest house, and hurried home to select the most appropriate remedies indicated by his condition. I had scarcely made this selection before Mr. Shaw came desiring me to hasten and see his pupil, for said he, " I am afraid he is dead." I replied that I had scarcely any hope of his recovery—"for," said I " his case is evidently a bad instance of poisoning."

We hurried to Mr. Reed's, whither he had been carried. I ordered his wet clothes to be immediately removed, and had him placed on a pallet upon the floor. I now instituted a more critical examination, and found another truly formidable symptom which I had previously overlooked, viz.: a suspension of deglutition, with the jaws firmly locked and sanious saliva issuing from between his set teeth.

Treatment.—Frictions with capsicum over the pulmonary and cardiac regions; as soon as signs of warmth appeared on the surface, epispastics; frictions to the arms and inferior extremities with sinapisms; vesicants of aqua ammonia to the whole vertebral column. I continued the frictions on the exposed surfaces.

In one hour from the commencement of treatment evidences of returning animation were apparent ; a slight but very imperfect pulse at the wrists, and slight tremors in the muscles of the arms and legs. His jaws were prized open by inserting the handle of an iron spoon between his teeth, and a few drops of aqua ammonia diluted highly were swallowed with difficulty. By perseverance the patient was induced to take camphor in substance, which had a happy effect. Enemata of warm soap suds were served every half hour, and persevered in until free evacuations from the bowels were induced.

In six hours the pupil had so far recovered as to admit of removal to the Academy. The reäction was followed with fever and cerebral congestion, which yielded to local abstractions of blood and an ordinary antiphlogistic formula. Mr. J.'s convalescence was often interrupted by disagreeable attacks of dysentery, requiring our higher order of opiates and astringents, for their suppression. Finally his body was suffused with an exanthematous eruption of a syphilitic character which was removed by an exhibition of the iodide of potassium.

ART. III.—*On the Influence of Acids in conjunction with Mercurials:* By HUMPHREY PEAKE, M. D., of Arkadelphia, Arkansas.

I GOT the notion when a small boy, that if within a dozen or so of hours after having taken a dose of calomel, a person should eat apples or peaches, or drink cider or eat pickles, or use vinegar in any way, that he would almost certainly pay for the indulgence by a mercurial salivation. I was probably a dozen years old, and had been salivated, I recollect, on as many as four occasions, after having yielded to the temptation thrice to eat apples, and on a fourth to drink some cider, after having taken calomel the night previous, before I was fully convinced of this truth. My recollection of those days serves me of many others who paid the same penalty for like offences. My father is a physician, and I have heard him caution persons not to eat or drink any thing sour for some time when he was giving them calomel. On one occasion, I recollect that an old gentleman, a neighbor, came to our house who had taken calomel two days previous by my father's advice. It being in the apple season, we had just made some cider, of which he was very fond. Thinking that a sufficient time had elapsed to prevent a salivation following its use, my father consented to his drinking some. He was profusely salivated by the next day.

This notion prevails to some extent among the people of this, and I presume, also, those of other countries. Many of the inhabitants here are from the older States. Many are fully convinced that this is a true poison, particularly those in the lower ranks of society. I have not seen physicians who had observed and acknowledged the fact.

My father, who has been practising for thirty-five years, tells me that he is as well satisfied of this fact as of any other in medicine.

I began my professional studies with this idea, which had grown as I grew. Indeed, I thought it an acknowledged and universally known fact. In course of time, I became a student of Dr. Howard Smith, now Professor of Materia Medica in the New Orleans School of Medicine. Not having seen this noticed in any book, but thinking it a fact well known to the profession, nevertheless, I asked him one day, the cause of it—why the use of acids, shortly after having taken mercury, salivates a person? To my utter astonishment, he answered me that he had never heard of such thing.

I now began to investigate the matter, but never saw any allusion to it in any work, although I examined many. I became a pupil, after a while, of Dr. S. Weir Mitchell, of Philadelphia—a gentleman thoroughly educated in the schools of France, England and the United States. I asked him the same question which on a previous occasion I had asked Dr. Smith. His answer was that it was something he had never heard of. Still, not satisfied, I asked his father, the late lamented Professor J. K. Mitchell of the Jefferson Medical College, and with a like result. He had neither observed nor heard of the fact, for such it certainly is. Since I have been practising medicine, I have sought opportunities of satisfying myself fully in regard to it. These opportunities, it is true, have been accidental, but this does not lessen their value. Mine is a country practice, together with that of a village of fifteen hundred inhabitants. It is very seldom that I desire to salivate a person, and feeling so well satisfied of the truth of the idea advanced in this paper, I have not felt myself at liberty to experiment to the end its verification or falsity. Accident, however, has afforded me ample opportunities of satisfying myself. I have known many persons salivated even by the use of buttermilk while under the use of calomel; and in a few cases of pneumonia where I desired a speedy and slight salivation, I am satisfied I have brought it about more speedily than I otherwise would have done, by allowing the patient to drink of this article, which is generally palatable and desired.

I believe it to be a fact that a person in health is more easily salivated than when sick, and I have attempted to account for this by supposing it due, in some way, to the acid gastric juice, which is in health secreted in so large a quantity, and which in sickness is greatly diminished.

I am in the habit of warning all my patients whom I am giving mercury, in accordance with the opinion herein set forth, and it is very seldom now that I find one of them salivated.

If the opinion I have advanced shall be found correct, it becomes one of great therapeutical value, and one of still greater importance in the prophylaxis of that loathsome and even dangerous affection—ptyalismus hydrargyratus.

Many, after having read the foregoing, may consider that if it were a fact it would have been known long ago; but this does not follow as a consequence. It appears to me, as I said in the outset, that many persons are aware of the truth I have stated, and that some of these have gotten it as a sort of tradition. It was just so with vaccination. The dairy folks of Gloucestershire believed no stronger in the fact that to have cow pox was a preventive of small pox, after the labors of Jenner, than they had done for years before. Yet he was the first medical man to call the attention of the profession to the great truth.

The writer of this only desires to call attention to what he considers an important fact not generally known.

ART. IV.—*Natural History of Yellow Fever:* By BENNET DOWLER, M. D.

In all the demonstrative sciences the rules are certain and infallible; but when we apply them, our fallible and uncertain faculties are very apt to d-part from them and to fall into error. All knowledge degenerates into probability; and this probability is greater or less, according to our experience of the veracity or deceitfulness of our understanding, and according to the simplicity or intricacy of the question. —(HUME, Philos. Works, I, 236.)

IF I may venture to attach any importance to my researches into the natural history, treatment, and pathological anatomy of yellow fever, the initial papers of this series are, as is supposed, the least valuable part of my labors, since they will probably contain neither my more matured, perhaps more correct views gained by twenty-two years' experience, nor my numerous post mortem examinations in this malady. Even now, after a long experience, I cannot flatter myself that I can give a satisfactory exposition of yellow fever, and, *à fortiori*, this task could not have been accomplished so well during my earliest researches.

In falling back upon the past as the point of departure I feel justified by valid reasons. The existing records of the great epidemics of New Orleans are meagre in every respect, and particularly as it regards their internal history, symptoms, treatment and morbid anatomy. The mortality statistics do not afford much pathological or therapeutic information. The mere historical relation of these exciting and sad events, well deserving of the pen of the historian, how much soever it may command the reader's attention, especially when embellished with rhetoric, is little important to the rigid pathologist who seeks for unadorned facts, and finds no details dry or uninteresting if they be true, relevant, and suggestive.

Memory is ever treacherous, particularly during the excitement, alarm and hurry of an epidemic. The actors in the great events of the sick room for the last quarter of a century are trending tombward. And if they wish well to their successors, to science, and to society, may they not find occasion to carry this wish into effect by leaving a record of their experiences, failures, and successes—a legacy which will be highly prized? What physician would not be thankful now, or in the next century, and in the next, and the next, if the first epidemics in New Orleans had been, or shall be faithfully chronicled at the bed side? Such transmitted informations concerning the past might lead, if anything wild, to important deductions in treatment—might tend to settle on a comparatively firm basis, much that now remains unsettled in therapy. At all events the analysis of the past conjoined with the present is the most promising route by which certainty or amelioration can be achieved. A single mind, however, is seldom competent to observe from his own isolated stand-point the whole field of action, much less to record it deliberately in writing. The general who looks over the whole field of operations knows but little of the details of the battle, until each officer sends in the report of his movements, including killed, wounded and missing. Many competent medical men for very insufficient reasons excuse themselves from contributing anything to the treasury of medical knowledge, affirming that they have no time to devote to this purpose, that others have written, or can write, much better, etc. Were all to sin by omission after this manner, the horizon of knowledge would be greatly contracted, and progress would be reversed. Where there is a will and a wish there is a way.

Should any considerable portion of these papers be continued, which is doubtful,* the reader will probably be sufficiently apprized of the general nature and progress of medication in yellow fever as practised in New Orleans for many years. If he cannot discover unity, he will diversity ; and if he cannot accept the present confused and variant therapy, he may, perhaps, find something in that which has been wholly or partially abandoned without sufficient reason, for novelties of less value for which neither rational nor empirical medicine affords any warrant.

The facts which I propose to communicate are of a common or average character, not being selected as in any degree extraordinary or exceptional. At first sight they may appear wholly useless seeing that they are not, at present, brought forward to establish any general system, special doctrine, or therapeutic panacea. Had I at present the wish and the will to lay down certain postulates; had I the leisure to select and copy all such recorded facts as might appear to favor the validity of these propositions, putting aside or underrating all others, I might comment, argue, and draw *ex parte* conclusions with whatever plausibility my knowledge of the science of logic and the art of logomachy would permit.

In practical medicine much is uncertain, much is at best probable only, and the latter probable in variable degrees, while little is absolutely certain. All uncertainty, however, is in the imperfection of our understanding, not in the science itself.

I do not purpose to attempt an interpretation of the recondite and complicated phenomena of yellow fever; nor is it intended to offer an explanatory lecture upon each, or perhaps, any case. If it be possible or expedient to publish in this Journal a sufficiently large number of these clinical histories, they may, as already indicated, afford data from which the reader may draw conclusions as to the measures good or bad, which have been resorted to in the treatment of yellow fever during many epidemics. It is hoped that those who have neither witnessed these epidemics nor had experience in this disease, will find in these histories of facts, something of interest as well as something

* The contingency here alluded to turns upon the question of original matter, of which there is an alarming dearth. I hope, or rather I wish, that others would occupy the original department of this Journal.

I write the present paper at intervals between visits to the sick of the prevailing epidemic. I might apologize for imperfections, etc., but this is so common a practice that it will no longer disarm criticism.

of a countepoise to mere speculation, which however beautiful and harmonious in the closet, recedes the moment the stern realities of the bed-side present themselves fully in view of the anxious, but unhappy man who undertakes, or is expected to kill the fever without hurting the patient.

These memoranda, will, to a limited extent, be classified : Thus examples of symptomotology may naturally be arranged so as to illustrate the rise, progress, and termination, or some stage of the disease, or they may serve to illustrate the principal or allied remedial measures used for the cure of the disease, etc. Every case was not seen in its inception, or in its acme. But a great many observations made in any one stage, or in any one day of the disease, will give the average or general characteristics with sufficient precision. Some cases in my own practice, especially in non-epidemic years, which were noted twice daily, occupy over twenty pages, and will be given in an abridged form only. Where the treatment is indicated, the technicalities of the formulæ have not been often reproduced, because such details are not necessary for the reader, nor admissible within the limits prescribed for this series.

Previous to the great and widely extended epidemic of 1853, I have reason to believe that in the conduction of this Journal a portion of its patrons outside of New Orleans, entertained the opinion that the subject of yellow fever, then almost unknown in rural and village practice, occupied too much space in its pages. But the extension of this epidemic to the most unexpected rural districts and towns throughout the Southern States, together with its probable reäppearance in the North-Eastern States, and in the Peninsula of Europe, localities where it has been most severe in the past century, as well as during the first and second decennia of the present, have given to this malady a paramount and permanent importance for almost every practitioner. Hence, judging from the logic of events there can be little reason to fear that the quantity of the contributions on this subject will give dissatisfaction, provided the quality be acceptable. The most unattractive paper, if founded on observations carefully made, will, doubtlessly, contain some fact suggestive of useful thought of practical application. At all events, every honest effort in this direction may justly claim consideration, though it fail to answer all the questions, the solution of which, the reader is most desirous to obtain.

It is not the processes, but the utilitarian results of scientific researches which awaken attention and excite a profound interest in the public mind. But the more patient cultivators of science accumulate, compare, and study facts even though their satisfactory generalization may not as yet, be within reach. The facts of geology had been long accumulating, long observed, but their rational classification, interpretation, and scientific uses are of recent date. Analogy indicates the same good fortune in store for certain obscure departments of practical medicine, if the necessary data be accumulated, treasured up and preserved, and be diligently compared, revised and generalized.

In selecting cases I have not been governed by the principle of selection in the ordinary acceptation of that term. I have copied a few of those which were the most convenient, not such as specially support my own views, or the views of others upon doctrinal subjects —not such as were extraordinary for mildness or malignity—not such as exclusively represent antiphlogistic, or any other mode of treatment—not such as best represent my more matured appreciation of remedial measures. Every epidemic develops some new remedial combination or phase of these, or revives others almost forgotten. Panaceas wax and wane more rapidly than the moon. Some favorite prescription for large, small, or medium doses which glimmers for a time in the reigning darkness, goes out like a meteor before the steady advances of the invisible foe whose victims fall by thousands just as before. The multitudinous and ever varying remedies from the slops called ptisans to the most concentrated preparations—from infinitesimal to heroic doses, show that a rational therapy of this disease, a treatment founded on its peculiar pathology, including the why and the wherefore, and well defined principles, are desiderata, yet unachieved, perhaps, unachievable.

The truth is, the pathology of yellow fever is not well understood. To call it inflammatory, adynamic, malignant, bilious, hepatic, gastric, nervous, humoralistic, hæmorrhagic, or yellow, *ad infinitum*, affords no very satisfactory principle technically called rational calculated to guide in the treatment, be the latter antiphlogistic or stimulant. The phases of the disease are many, and withal illusory. Even the epithet *yellow* is ill-chosen. *Red* would be more characteristic. In the initial and fully developed primary stage, a *redness*, an almost erysipelatous flushing of the skin, is, perhaps, a hundred times more common than

yellowness. The latter is sometimes absent, or but slightly marked, even in the advanced and fatal cases, being generally the sequel to the former. The redness of the skin, one of the most important diagnostics of the disease in the Caucasian race, (as well as yellowness) is absent in the African, which renders the diagnosis of the disease in negroes a matter of extreme difficulty.

In this state of uncertainty concerning the rationalistic pathology and therapy of this disease, an enlightened empiricism not only affords hope, but a great deal more, that is relief in numerous cases. It is not proven that a knowledge of the cause, nature, and pathology of yellow fever or any other disease, with few exceptions, is essential to the cure. Even intermittent fever, which of all the fevers is best known and the easiest treated, is not an example of a purely rational but of a purely empirical therapy. The physician gives quinine in this disease empirically, not because he can from its ætiology, or its special pathology, or morbid anatomy, discover any adaptation or fitness in this remedy beyond mere experience. If all the world be in error concerning the pathology of this disease, the efficacy of antiperiodics is not thereby invalidated. Syphilis and mercury are not known to be filiated in a rationalistic relation. Few diseases can be treated upon a rationalistic basis. The chemist can explain how acidity of the stomach may be temporarily neutralized by alcalis—a physicist or a physiologist, how a morbid heat may be diminished by refrigerants, as cold water, evaporation, etc., not to name other examples.

If, therefore, the therapy of yellow fever can be deduced neither from its cause nor special pathology, the physician must fall back upon the old empirical or experimental method. What has cured one will cure another in the same circumstances, though no rational explanation or principle can be assigned for the same. Hence the necessity of a continuous and augmenting stream of experimentalism; that is to say, numerous and faithfully recorded cases of disease, treatment, etc. Indeed, this is the surest means of attaining the rational treatment so much desiderated, while it serves to enlarge and improve the domains of experience itself. In the present state of our knowledge, which is the better book, a book of cases? or a book of pathological speculations? Facts must be, speculations may or may not be, valuable. The former, however unattractive, afford the reader materials for thought; the latter not only furnish a symmetrical theory,

but save the reader the labor of thinking also, but the whole may be as writings upon the sand, which the next wave obliterates.

It may be said that in distrustiug rational, and in adopting empirical mediciue, the physician is pursuing the method of the populace, and the charlatan. True. But the thoroughly educated physicians and physiologists, with few exceptions, are alone competent to diagnosticate diseases, to foresee their tendency, to appreciate the effects of remedical ageuts. In a word, the natural history of disease is to others, for the most part, a sealed book.

Let science rain cases and facts, seeing there is a lamentable drought of reliable pathological and therapeutical principles. Notwithstanding the abundance of yellow fever literature extant, there is a great dearth of special histories and clinical reports. If posterity could be consulted, it would probably attach more value to a few hundred faithfully recorded cases of each epidemic, than to all the explanatory theories, fine speculations and dogmatic systems now extant concerning the ætiology and pathology of this destroying pestilence, which threatens to reëstablish itself in the North, on the Atlantic coast of America, and in Peninsular Europe, where it formally raged as severely as within the torrid zone. Witness the late epidemics in Lisbon, Portugal, and in Norfolk, Virginia.

In New Orleans, yellow fever has prevailed, at intervals, for sixty-two years ; and, if the small number liable to the disease during the curreut season, be considered, it may be said, up to this date, Sept. 25, 1858, that this is its culminating year as compared to the past. To the people of this city, yellow fever is a question of paramount importance to all classes of society in many of its phases.

During the sittings of the convention (1845) for the revision of the new Constitution of Louisiana, a debate arose concerning the time for holding the general elections; one party contended that the elections should be held during that part of the season in which yellow fever usually prevails, so that "the birds of passage," that is, strangers who then fly from the city, being unwilling to undergo the baptism of yellow fever, could not vote, ought not to vote, having only a temporary interest and residence in the city ; while, on the other hand, orators contended that this provision would deprive half of the *resident (?)* populatiou from voting, as that proportion, it was asserted, was always absent in the yellow fever season. Thus yellow fever tinges legislation, commerce, and all the professions.

In these histories which follow, the personality of the medical attendants, more particularly as it regards others, is seldom brought into view. Science is impersonal. In many cases wherein hospital physicians have spent seconds, or minutes only, I have spent hours patiently over the patient and the cadaver. If the past be chargeable with over-medication and excessive blood-letting, I am willing to assume a part of the responsibility in these well-meant, but too often unsuccessful measures. At no former period have I practised, or even thought favorably of repeated large, much less syncopal blood-lettings in yellow fever. At the present time reliance on this method is almost entirely repudiated, but the mortality is, if I may judge, greater than ever. During the current epidemic, I have examined at intervals, a considerable portion of my notes of the treatment of this disease, and without pretending to be able to give exact figures or reliable statistical results, I might, as far as this examination has yet extended, waive, at least provisionally, my prejudices against venesections carried to syncope in the early stage of this disease occurring to robust subjects. Some of these cases will probably appear in the sequel and may challenge comparison with the existing, almost hopeless confusion in treatment which vibrates between the expectant method, opiates, quinine, calomel, jalap, citrate of magnesia, castor oil, blankets, ptisans, etc. Physicians at a distance who have written letters of inquiry, desiring to know what the established treatment is, might as well inquire how a coat could be made so as to fit the moon in all its changes.

One of these letters from an able, hopeful physician, resident beyond the limits of Louisiana, kindly addressed to myself, reads thus: "If I could get yellow fever cases in New Orleans, I would like to go down and practise a while in the city. A system of practice pursued here in two epidemics and universally approved by the people, and to a great extent adopted by the physicians, has certainly been the most successful ever pursued, and in the worst class of cases, as evidenced by every circumstance connected with these epidemics. This is no one-idea system. It is a system which I believe to be comprehensive in its application to the disease in question, and when put into operation by a variety of means and modifications, seems to me, at least, to be consistent with the most rational principles of physiology, pathology, and common sense, known. I should like to try this prac-

tice, however, on a more extended scale; and if you would be so kind as to inform me if I could have the opportunity of doing so, I would be pleased to visit the city for that purpose."

Veterans of twenty epidemics, now incredulous, once believers in systems for the cure of yellow fever, be not surprised that others think as you thought, until sad experience humbled your lofty expectations to the dust in the presence of epidemics like the present Were Hippocrates and two hundred of his most illustrious compatriots to descend from their elysium into the Crescent City to cure yellow fever without bringing some celestial drugs unknown to the two hundred New Orleans doctors, they would doubtlessly lose, if not one in two, still a great many patients in this desolating pestilence.

The numerical method, the most reliable of all tests of the comparative value of different modes of treatment, has never, so far as I know, been applied under the direction and inspection of a competent and disinterested jury of inquest. Hence men of sense not only distrust, but reject this kind of evidence emanating from private, interested individuals, who report without any authentication their great successes, omitting their greater failures in treating this or any other disease. They may say that they lose ten per cent., or none, and who can gainsay their figures, or contradict their statements? It is very different in a public hospital where concealment is nearly impossible. Thus in the Charity Hospital, for a quarter a century, about half of the yellow fever patients die. Although many are admitted in a dying state, so in like manner in private practice many are equally in a hopeless condition, and withal worse provided for as to physicians, medicines, nurses, cleanliness, baths, suitable diet, bedding, etc.

Homœopathy, which from first to last, as a system of medication, is a deliberate deception* (its cures being due to the inherent ten-

* Long ago, as well as more recently, homœopathists gave, and they still give doses in many cases as large as the regular faculty, and sometimes larger. Dr. A. Mucry, of Hanover, in *The British and Foreign Medical Review*, (Oct., 1846), says that even in Germany, "the homœopathists bear the original name wrongfully, being no longer homœopathists. The majority have even resumed a great portion of the allopathic medicines. In Germany no man of undoubted eminence has ever become a convert to the system. Only once has an instance like that of Prof. Henderson occurred, in the case of Dr. Copp of Hanau." 566.

In the volume of *Transactions of the Kentucky State Medical Society* the committee reported as follows: "It is a well established fact that they administer the medicines we usually employ, and in large doses to their patients. Facts of this kind have come under our own observation. During the prevalence of cholera in Cincinnati, in 1849, the late Dr. Latta, of that city, found some of the patients of these pretenders, badly salivated. Dr. Chambers, of Covington, saw the same result in one of their patients, whom he was afterwards called to attend. Infinitesimal doses did not occasion these effects. One of this kind of practitioners, and a very prominent one indeed, who formerly resided in this city, and had a large practice, *purchased* more quinine than any other physician in Louisville purchased or prescribed. Miasmatic diseases were prevalent at the time. In Cincinnati they gave as large doses of camphor in cholera, as any other practitioners of medicine—a treatment

dency of the economy, or to the clandestine administration of the doses usually given by the regular faculty) above all other systems gives itself the most favorable statistics of recovery. It is natural for those who deceive in physic, whether regulars or irregulars, to deceive in figures: *Falsus in uno, falsus in omnibus.*

Shortly after the great epidemic of 1853, three homœopathic physicians of New Orleans, who probably rank among the most respectable and talented of their fraternity, published a statistical statement, in the *Daily Delta,* very flattering to their school as it regards their successes in that and in other epidemics elsewhere. By the way, it is but just to say that the homœopathists (whose large clientship is neither the least intelligent nor the least wealthy in this city) seldom inundate the newspapers by advertising during epidemic excitements, their skill in, and knowledge of, the ætiology, pathology, and therapeutics of yellow fever.* This doubtful method of promoting scientific medicine, by technical essays in the political journals, though calculated to gain notoriety and win influence with the public, would be more excusable in homœopathists than others, seeing that they have no medical journals in this city.

The homœopathic report by the three physicians alluded to, appears to have been excited by, and intended as an answer to, Professor Hunt's eloquent denunciation of quackery before the Physico-Medical Society. Be that as it may, this statistical report sets forth that the homœopathic fraternity in this city, during the epidemic of 1853, treated between eight and nine hundred yellow fever patients with a loss of only one in seventeen. The homœopathists aforesaid, compare these figures with those of the Charity Hospital, where one in two give up the ghost, affirming that if many were sent to this hospital in a dying state, an equal proportion of moribund cases fell to the lot of homœopathy. They also reproduce the mortality statistics of yel-

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at war with both infinitessimalism and the doctrines of *similia.* Further facts need not be adduced. We could confidently and truly assert that no case of disease which, if left without treatment, would terminate fatally, will recover if treated by an honest homœopathist—we mean one who follows in his practice the precepts of his master."

* The code of Medical Ethics now universally recognized by the Faculty in this and many other lands, says (Chap II, § 3) that "it is derogatory to the dignity of the profession, to resort to public advertisements or private cards or handbills, inviting the attention of individuals affected with particular diseases—publicly offering advice and medicine to the poor gratis, or promising radical cures; or to publish cases and operations in the daily prints, or suffer such publications to be made;—to invite laymen to be present at operations,—to boast of cures and remedies,—to adduce certificates of skill and success, or to perform any other similar acts. These are the ordinary practices of empirics, and are highly reprehensible in a regular physician."

low fever in other places as occurring under the beneficient therapy of infinitesimal dilutions: In Rio, in 1851, as they report, the homœopathists treated 3,256 yellow fever patients; of whom fifty-six died a few hours after the first visit, having been moribund, and therefore may be excluded from the black list; of the residue, all were cured but one hundred and sixty-one; that is less than one in twenty (1 in 20·32+) died. In 1853, in Natchez, the homœopathists cured out of three hundred and forty-five cases, all but nineteen, a mortality less than one in eighteen. In Barbadoes, in 1852–3, out of three hundred and eight, homœopathy saved all but ten; that is, nearly thirty-one cures to one death. These deaths, however, are charged principally, not to the fever, but to the previous mal-treatment by the regular faculty who had "*drugged*" the poor patients before they had the good fortune to be treated by the homœopathists. Figures are produced from which it appears that infinitesimals really save all but six in the hundred. Ah! these six! If five times twenty could be cured in the hundred, then charlatans who fill the newspapers with infallible remedies, unfailing cures, would be sorely non-plussed unless they can invent a mode of curing one hundred and six out of every one hundred cases.

Now, all physicians acquainted with this malady, know that its insidiousness is such that many do not recognize it, nor feel alarmed, until the disease is far advanced, and, consequently, do not call for medical aid in time to prevent a fatal termination, should that have been at all preventable. These gentlemen assume that the number of moribund cases sent to the Charity Hospital is similar to the number of this class which they were called to treat, say one-fourth, which will, in eight hundred or nine hundred, give an aggregate mortality of two hundred or two hundred and twenty-five instead of six per cent., or even one in seventeen. It is probable that yellow fever, abandoned to its own course, will kill half, more or less; therefore, as homœopathic treatment, honestly carried out, is wholly inert,* the mortality will have been four or five hundred out of "between

*In the reproducing from the *The British and Foreign Medical Review*, the following note, there is no desire to ridicule homœopathists. The latter could easily retort by referring to the chaotic therapy extant among the regulars at the present moment:

In 1846, Dr. Balfour wrote from Vienna that Dr. Gross, homœopathist, of that city, "often contents himself with letting the patient smell the remedy, and waits four weeks or so for the completion of the cure, not even permitting a second smell." Will the homœopathists of New Orleans deny that they ever give twenty-grain doses of quinine, and correspondingly large doses of other drugs, and particularly of the very active preparations?

eight and nine hundred." However, the above manifesto cannot be disproved either by the public or the "Old School." Figures cannot lie. Indeed, the public tacitly seem to credit the flattering—O! most self-flattering reports of homœopathists, who have, it is supposed, a wealthy clientship, including many distinguished for intelligence.

A predilection to flattering statistics is not confined to homœopathy. An anecdote attributed to Prof. Warren Stone will serve to illustrate this point. Whether the story be true or not I have not the means of knowing as the Doctor has not yet returned from Europe. The story is on this wise: During the epidemic of 1853, the Doctor was asked, how many yellow fever patients he had lost? He answered that, two thousand five hundred had died, according to the official report, and as he had seen most of the physicians of the city, all of whom said they had lost none, therefore, he must have lost two thousand five hundred. This satire applies to more than one verbal statement now current, to the effect, that death and certain doctors never meet face to face in any case of yellow fever, or if they do, the former is sure to be defeated by an unfailing pill, power, or syrup, which the latter well knows how to prescribe. Nevertheless, the natural history of yellow fever is, and always was, characterized by an enormously high ratio of mortality in all places and climates where it has prevailed, and under every kind of treatment. There is nothing surprising, nothing reproachful to the medical faculty in this. The collapsed stage of cholera, consumption, cancer, and some other maladies, generally defy the utmost efforts of human skill.

Before proceeding to clinical histories, I would again remind the reader that in detailing cases an impersonal course will be adopted generally. Some of the most able observers have advised an exclusive reliance on the cases of others, in order to investigate the natural history of disease without bias and without mental distraction. A rigid adherence to this plan I have avoided as unnecessary for science and inconvenient to the investigator. Dr. John Davy, in his "*Researches, Physiological* and *Anatomical*," (2 vols. Lond. 1839,) makes the following statement concerning the method of noting and studying pathological cases: "For my own part, I must candidly confess, I have never been able to combine in myself the two characters of the practitioner and the inquirer at the same time; and that I have always found it beyond my power to watch the pulse and countenance,

and the feelings of the patient, and study, etc., in a satisfactory manner ; I have always been a mere spectator, etc. I would recommend the same plan of proceeding to most others ; lest in the ardor and interest of scientific inquiry, the patient have not that attention," etc. M. Louis has gone still further in maintaining that the making of scientific observations is a distinct trade which must be learned, not divined. Certain it is, that the observer whose purpose is wholly scientific, when relieved from the responsibility and medical management of the patient, may the more readily and impartially investigate the case, and its result. In his own cases he is an interested witness. As a counterpoise to possible biases of this nature, the cases of others are available. In omitting the names of the physicians who treated this latter class of cases, I have been governed by the best motives. These medical gentlemen may not approve of the mentioning of their names. They may have recently modified, departed from, or improved their remedial measures ; for "incompleteness, the soul of progress, runs parallel with the most extended observation." No changes, no progress. The latter implies the former ; but change may be great, while knowledge or improvement may be either stationary or retrograding. In an imperfect science, such as medicine is, the stationary periods in its history have been characterized by an ignorant dogmatism, by a superstitious deference to authority, or by the incubus of false systems.

CASES.

Convulsive form of yellow fever; one case treated from the first minute, the other from the third day; third from the third hour. Recovery.

1837. Sept. 6th. My child, Sarah, born in Virginia, aged four, resident sixteen months, healthy and vigorous, without having had previous indisposition, on the 6th day of Sept., 1837, in the afternoon, while I was present, turned pale; fell suddenly into strong convulsions; the head quickly became hot; the face flushed and purplish; the temporal arteries distended; the eyes injected, with a loss of parallelism, in their direction, as in strabismus; respiration noisy, rhoncous, and unequal; pulse irregular; one half of the body longitudinally was instantly paralyzed; the opposite side being almost constantly agitated with muscular spasms; insensibility; great difficulty in swallowing.

The child was immediately bled freely from the arm; cupped along the entire spine; had copious enemata; about half of the body was sinapised, wrapped in a blanket wrung out of warm water; ice to the head; a dose of calomel and jalap; subsequently castor oil.

These measures were carried out rapidly. Dr. M. M. Dowler was present at the attack and assisted in the treatment. The treatment began during the first two or three seconds of the disease.

In two hours the apoplexy began to subside and the senses to return. But the palsy and loss of speech continued for twelve hours.

The fever which followed was intense, and persisted for nearly a week, but subsided under almost daily purgatives with calomel, without stimulants or quinine.

In the meantime, the entire family, that is, another child and three adults, and, subsequently, two adult slaves, were attacked with yellow fever; seven cases and seven recoveries.

1838. Sept. 14th. Called to an Irishman, aged twenty, resident eleven months, sick two days. Neither himself nor his friends were aware of the character of his malady, nor alarmed, until two severe convulsions, (the only ones he ever had,) took place.

Features dull and dejected, idiotic and staring; face flushed and hot; temporal arteries excited; skin dirty yellow and cool; eyes mixed with yellow and red ecchymosed and injected spots; uneasy pain in the forehead, chest, and in the abdomen; breathing irregular, and attended with sighs; general restlessness; pulse but little affected; vomiting; tongue natural, except a little fur; torpor of the bowels. Blood-letting, cold to the forehead, a blister to the neck, sinapisms to the extremities, hot foot baths, hot bricks under the bed clothes, mercurial and castor oil purgatives.

Recovered in five days, though the strength did not fully return until twelve days after the attack. Such was the torpor of the bowels, that in some instances one pound of castor oil was taken in a single day before purgation could be effected. The stools at first like heavy mud with water, became at length green and finally natural.

1858. Sept. 15th; 9, A. M. Tom, black slave, aged about eight, born in Kentucky, resident eighteen months, rather thin in flesh and apparently delicate; never sickly; rested badly last night; had a fluid alvine evacuation; seized with the fever three hours since without chilliness; pain in the head, back-ache; skin dry, *(cutis anserina)*, and

moderately hot; pulse quick, small, frequent; eyes injected; tongue natural, except a little white fur. I directed a warm mustard foot bath; a sinapism to the abdomen, to be followed with flaxseed poultices; cold drinks; cold to the head; three powders, each cal. three grs., blue mass, three grs., pulv. gum. acac. x grs.; one every two hours.

3, P. M. Vomits; delirious; falls into a comatose sleep, eyes partly open; simple enemata.

9, P. M. Comatose; when aroused, incoherent at first but answers questions; pulse small, hard one hundred and forty-four; pain in the forehead; eyes red; skin dry, moderately hot; tongue tumid and red; convulsive, involuntary motions of the limbs, as in *chorea sancti viti.* Castor oil, extensive sinapisms, poultices, cold to the head.

16th; 8, A. M. Copious watery stools; inclines to stupor or deep sleep, often turning over in the bed; spasms gone; sweats; pulse rapid and nearly imperceptible.

17th. Improving, but very weak; treated with diet and good nursing. Left his bed in five or six days.

1848. Gretna, September 17th, 3, P. M. B. S.; recovered from congestive fever; had walked about for two weeks; had resumed his business for several days; was taken with yellow fever six days ago; treated himself with an emetic; the second day with a dose of castor oil which purged copiously; his bowels have since continued relaxed; used baths. His senses were regular until to-day, when he got out of bed and fell heavily on the floor, in a convulsion fit, frothed at the mouth, remained several hours insensible, and was speechless until I saw him; found him stupid but not altogether insensible; he answered a question or two with difficulty, but did not seem to comprehend others. Pulse about one hundred, variable; respiration easy; liver tender; skin, sallow, hot and dry; tongue tumid, smooth, retracted, quite dry, and of a dull cherry red; temporal arteries distended, throbbing; eyes faintly yellow. TR.: Cold to the head; sponge the body with vinegar and spirits often; mass. hydr. ℈ii, quin. ℈i, in sixteen pills; four every three hours.

Sept. 18th; 8, A. M. Better; slight softness of the skin; no vomiting; pulse and respiration more natural; speaks; no medicine. 6, P. M. S. quin., gr. x, in divided doses; sinapism to the abdomen.

19th, 10, A. M. Tongue moist, covered with a white fur; respiration regular; pulse soft and slow; skin moist and natural; liver bears

pressure better; external veins fallen; headache gone; eyes less yellow; quin. gr. x. in divided doses.

He recovered slowly without further medication. [In two weeks he recommenced business, as engineer in the foundry, but in a few days after, was seized with intermittent, for which I gave the usual remedies, with success. Thus in a few weeks, he had congestive, yellow, and intermittent fevers. I learned that he died of congestive fever in 1851.]

1858. Sept. 27th. I am now attending a child born in this city, aged 11 months, affected with the fever for thirty-six hours; in that time it has suffered about a dozen of convulsions. The fever seems to be a kind of hybrid, a cross, or blending of remittent and yellow fever. The mother unacclimated; the father I attended in a severe attack of yellow fever in 1853.

28th. A marked remission of the fever occurred this morning. Quinine; wine; breast; chicken-water. Well in three days.

[It is supposed that a number of these convulsive cases of yellow fever are recorded in my notes.*]

CASES CHIEFLY ILLUSTRATIVE OF THE EARLY STAGE, TREATMENT, ETC., IN PATIENTS WHO RECOVERED.

Rev. Mr. L., born in Maryland, middle aged, late of Natchez, Mississippi, for the last nine years traveling in the South; has had several attacks of bilious fever; for ten months past residing in Lafayette city, (now the fourth district of New Orleans).

Initial symptoms.—After considerable professional exercise, was taken without any premonitory symptoms, at 9 o'clock, P. M., Sept. 6th, 1842, with chilliness, flushing and heat of the face, muscular soreness, spinal pain, with uneasiness and transient pains in the forehead and temples. He immediately took a dose of powdered red pepper, without medical advice.

One hour and a half after the attack I found the skin everywhere hot, moist and injected; the face, head and neck hot, having an almost scarlatinous redness; eyes injected, hot, painful with a sensation of roughness as if irritated with sand; restlessness; slight exhilaration of mind like intoxication; ideas coherent; respiration accelerated;

* M. Robin, in his travels (*voy. dan l'interieur de la Louisianne*, 3 vols., Paris, 1807) mentions the case of a man attacked with yellow fever on the levee while eating a water melon, who fell all at once (*tout-a coup*) and expired, doubtlessly in a convulsion. This writer adopted the humoral pathology of yellow fever—a doctrine just now much in vogue with the medical faculty.

pulse quick, small, hard; "load at the heart"; slight catarrhal symptoms as cough, etc.; nausea; tongue natural; bowels regular; thirsty; temporal arteries distended, very tortuous, hard, throbbing, and nearly equal in size to the radial arteries. Sinapisms to the legs and epigastrium; pounded ice in a bladder to the forehead; gum water; hydr. submur. ℈i; massæ gr. x.

Twenty-sixth hour.—Noisy, raving delirium requiring several persons to hold him down in bed; superficial veins and arteries of the head and neck much distended; head hot; eyes prominent and red; universal, hot perspirations; respiration panting and laborious; pulse firm, not frequent; tongue natural; thirsty; vomits; dark adhesive stool. Bled from the arm about xxii℥; fainting; pulse became softer, slower and smaller. The face became pale for a time, the heat diminished; the mind quiet. The blood was natural in appearance and coagulated well in half an hour.

Thirty-fourth hour.—Mind rational; said that "last night there was a great rush of blood to the head;" this was doubtless the prelude to the delirium of which he had no recollection. Skin moist, and natural in temperature; pulse full, about one hundred; respiration easy; free from pain, inclined to sleep; tongue a little rough from papillary tumefaction, less moist than in health; abdomen and its organs free from pain on pressure, except the liver; stools copious, dark and consistent. During this period he was cupped over the liver and epigastrium and mastoids and had taken massæ ex hydr. ʒi, in divided doses portions of which he vomited; sinapisms; orange-leaf tea.

Thirty-ninth hour.—Pulse excited, head oppressed; febrile paroxysm returned; six leeches to the temples; fluid magnesia, ℥ii, hot mustard foot-baths.

Forty-sixth hour.—Respiration heaving; pulse firm; great heat; much perspiration; head hot, facial blood vessels distended; outrageous and noisy delirium, required five or six persons to hold him.

Fifty-fourth hour.—Has slept; had copious watery stools; free perspirations; respiration quiet; pulse full; senses normal; says he "felt a rush to his head but no pain last night." This sensation was probably prelusory to the violent delirium, during which he was cupped on the nape and had blisters applied for a short time to the thighs.

At the next visit evident improvement had taken place. Recovered a few days afterward.

1842. Aug. 31. The fate of the young man whose case is described below, is unknown. He went in to the hospital after having been examined; declares he was well until two hours ago, when he felt chilliness; skin hot and moist; face and neck of a cloudy or dusky hue; eyes slightly injected; frontal and spinal pains; pulse quick and resisting; breathing hurried; tenderness in the central portion of the liver when pressed; countenance dejected. Defecation regular; tongue natural.

1843. Aug. 29; 1, P. M.; a clerk, born in Germany, aged twenty-seven, resident six months, last from Mobile; has suffered repeated attacks of fever in Mississippi and Alabama within the last five years, but possesses a vigorous constitution; temperate. At the time of the present attack his health was good, excepting constipation of two days' duration; slept well last night; eat his breakfast as usual. Two hours since he began to yawn, stretch, and suffer pain over the eyes, and in the muscles; able to walk.

Second hour.—Feels a creeping chilliness; thirst; spinal and frontal pains; altered taste; tongue, eyes and abdomen natural excepting a slight tension felt in the epigastrium; respiration thirty-two; inspiration when full distressing; pulse one hundred and four, full.

Ninety minutes afterwards.—Eyes and face injected; dozes; muscles twitch; has taken no medicine; palms 103°; axilla 106°; room 88°. *Treatment:* bleed to fainting; calomel six grains; castor oil; mustard foot bath.

Aug. 30th; 11, A. M.; intelligence unimpaired, though on shutting his eyes, he "sees strange sights;" pains in the forehead and back; vomits; temporal arteries distended; tongue and abdomen as yesterday; copious dark fluid defecations including lumpy masses; skin a little moist; pulse full, 80; respiration 42 and uneasy; hands 102°; axilla 105°.

Sept. 3d. Convalescent; hand 96°; axilla 105°; on the sixth day from the attack, (having used no additional medication but two enemas and one poultice on the abdomen) he left his bed.

1843. Aug. 22d; noon; room about 88°. A sailor aged 30, born in New Hampshire, resident one month, of good constitution, stout and muscular, slept well last night until 3, A. M., when he awoke with chilliness which still continues.

Ninth hour.—Intelligence natural; mind excited, (his ship sailed

two hours after his attack); pain above the eyes ; soreness of the muscles ; tongue pale, broad, moist; load at, and acidity of the stomach; abdomen natural; eyes faintly injected,and morbidly sensible to light; face, neck, and breast, flushed—a slight pressure with the hand causes this red flush to recede, and makes a distinct but momentary white imprint; pulse quick; respiration natural; hand 100°; axilla 106°. *Treatment:* V. S.; cal. eight grs.; hot foot bath; castor oil; cup epigast.

Aug. 23d; 3, P. M. Flushing increased; eyes injected; pulse and respiration hurried; no headache; no vomiting; tongue natural; inclines to slumber; hand 98°; axilla 105°; cups, baths, spongings.

Aug. 24th; 2, P. M. No natural sleep, except dozings; erysipeloid redness of the face and neck; skin moist; respiration quiet, but quick; pulse soft, quick, and variable; vomits mucosity and water; tongue pale, a slight whitish fur on its dorsum; muscular force feeble; on arising for a foot bath he fainted, and became temporarily blind; hand and axilla each 102°; cold to the head.

Aug. 25th; 1, P. M. Mind dejected; strength diminished; appetite, tongue, respiration natural; flushings less marked; pulse soft, quick, and variable; hand 93°; axilla 99°.

This man's case continued with little variation until the 4th of Sept., at which time his eyes were a little yellow, his muscular powers still feeble; pulse small, 85; respiration slight, 17; skin moist; tongue natural; hand 92°; axilla 97½°. Had used opiates, quinine, cups, serpentaria, four grs. blue mass. Cured Sept. 28th.

1843. Sept. 25th. A cabinet maker born in Boston, aged 27, resided one year at Cincinnati, three months at St. Louis, whence he came to New Orleans where after walking daily in the streets exposed to the sun for three weeks, was taken at 10, P. M., yesterday after a plentiful supper with pain above the eyes; symptoms like intoxication (though he never drinks even wine); heat in the head; chilliness; pains in the legs; vomiting; thirst.

Thirteen and a half hours after the attack.—Pain over the eyes; flushing of the face; the temporal arteries distended, tortuous and throbbing; pulse full, 110; respiration 20; inspirations somewhat painful; resonance natural; skin moist; tongue clean, but rather dry; thirst; urine scanty; costive for two days; (during his south ward

progress, his defecations had been unnaturally frequent and semi-fluid.) The eyes somewhat intolerant of light, their pupils natural; conjunctivæ excessively injected with red, inclining to pink color, the vessels multitudinous, minute, and tortuous; the general expression of the eyes animated; hand 102°; axilla 104°; perineum 105°; hand against the temple 105°; foot bath; cold to the head; drinks; cups.

Sept. 28th. Convalescent; hæmorrhage from the scarifications that had been made in cupping.

1843. Oct. 6th. A carpenter, aged 21, born in Maine, resident two weeks, large, muscular; attacked *sixteen hours since* with chills, frontal and spinal pains. Present state: Face, neck and breast highly flushed; pain in the forehead; eyes minutely injected; tongue and dental margins white; free perspiration; pulse full, large and soft; respiration and senses natural; hand and axilla 106½° each. Blue mass, quinine, foot baths, chicken water.

Oct. 8. Convalescent. Hand 86°; axilla 97°. Recovered a few days after.

1843. Oct. 14th. *Fourteenth hour.* A german, aged 31, long resident in Cincinnati, eight days in New Orleans; skin generally flushed, particularly in the face and neck; eyes prominent; conjunctivæ injected; corneæ brilliant; tongue reddish and dry; gums whitish; vomits; sweats; pains in the forehead, back, limbs and abdomen; pulse full, large, slow, soft; senses unimpaired; hand 95°; axilla 102°. Recovered slowly.

1843. Sept. 7th; 3½, P. M. A Canadian, steamboat-man, three years in the South; first fever here; aged twenty-four, late from St. Louis, resident ten days.

Twenty-eighth hour. Chilliness; muscular soreness; vomits; tongue white; abdomen supple; coughs; dull sound and pain below the left clavicle; pulse full, soft, 68; eyes injected; frontal pain; senses natural; hand 100°; axilla 103°. *Treatment:* Yesterday bled to fainting; cold to the head; enema; foot bath; bled to day to fainting; cupped; seidlitz. Recovered in a week after.

1842. Aug. 31st. J. C., born in Ireland, aged twenty, seaman, last from Boston, resident in New Orleans two months; sixteen hours sick. He had worked as usual the previous day, fixing sails on ship board. In the night he had chilly sensations; sleeplessness;

frontal pain, as he expressed it, "sufficient to burst his forehead;" skin moderately hot, and of a brick red on the face, neck, and hands; back ache; muscular pains; tenderness in the central portion of the liver; respiration easy; pulse regular, except in the temples where the arteries which were enlarged as it were, throb violently; forehead extremely hot; minute injection of the eyes; tongue natural; vomits; moderate thirst; bowels regular; castor oil; cupping. Convalesced on the fifth day.

1848. Aug. 25th, 10½, A. M. A steamboa-tman, born in New York, aged twenty-seven, resident at intervals for two years; healthy; remained during the epidemic yellow fever of 1847.

Thirty eight hours since, was taken with frontal, spinal and general muscular pains; next day he had a chill, took a dose of salts, but has been costive more than two days.

Present state.—Nausea, but no vomiting; pains continue; senses natural; skin dry, flushed; abdomen soft; respiration quiet; a slight white fur on the tongue; pulse large, regular 98; hand 103°; bend of the arm 103½°; axilla 104°. A foot bath; quin. ℈i, with hydr. sulph. nigr. grs. x, in six pills.

Aug. 27th; M. Convalescent; has taken hydr. sulphuret. grs. x, in grain doses; a dose of castor oil; lemonade frequently. Liver firm and painful on pressure; strong inspiration gives pain; tongue furred with white in the middle; respiration 16; pulse full, soft 50. Senses natural.

1843. Sept. 7th, 3, P. M. An Irish laborer, aged thirty; late of St. Louis; resident six months; for two years occasionally resident; first fever during this period; general good health.

Forty-eight hours sick.—Was suddenly affected with chills, muscular pains, and hot skin; was bled twenty-five ounces, took fifteen grains of blue mass, was cupped twice, had a foot bath. Eyes injected; tongue tumid, with a white fur at the base; vomits; abdomen soft; face flushed; pulse corded, 80; respiration quick but quiet. Seven days after, convalesced.

1843. Oct. 26th; M. An Irishman, aged 25, resident 1 year ; sick five days; vomits small quantities of dark liquid blood; comatose; eyes prominent, half closed; vividly injected, pupils mobile; respiration heaving, wheezing, noisy; pulse regular; tongue dry, red, tumid, coated with yellow fur; free perspiration; hand and axilla each 106½°.

Recovered in eight days after, contrary to the expectation of several physicians.

1843. Nov. 4th; M. A German, aged 32, steamboat-man, resident eight, and sick five days, taken with chills, frontal, spinal and muscular pains. Treated with general and local blood-letting, quinine and opiates. *Present state:* Respiration easy; pulse soft, small, 130; pressure of the abdomen painful; hand 99½°, axilla 103°. After considerable hæmorrhage from the throat, he recovered in ten days.

1843. Nov. 4th; 1, P. M. A flatboat-man, aged about 20, resident seven days; treated with blood-letting, purgatives, and lemonade: senses and excretions natural; sleepless; perspires; tongue white; abdomen supple; face and neck flushed; eyes prominent and injected; respiration easy; pulse quick, large and gaseous; hand 100°, axilla 107°; perineum 106°.

This man's case, simulating intermittent, began with frontal, spinal and muscular pains, chilliness, and hot skin; the two following days he had chills, but the symptoms of a mild yellow fever predominated. Recovered in eleven days.

S., born in Illinois, aged 21, steamboat-hand, resident one week, entered the hospital Sept. 16th, 1841, sick four days. On the 21st of September, his skin was natural except a dirty yellow color; eyes a little injected; tongue and respiration healthy; pulse small and corded; senses regular; hopes of recovery sanguine,—says that he has bled from the nose, during two days, one gallon of blood; the nurse confirms his statement.

Sept. 27th. Discharged. Three days after, he had a relapse. I met him in the street, returning. He had been chilly; skin, now, warm; pulse small, quick and irregular; face flushed; pain and oppression at the stomach and in the chest. This man recovered. [I state from memory only, that he was treated with large doses of quinine.]

B., born in Maryland, aged 24, last from Covington, Louisiana, resident in New Orleans one and a half year, a plasterer, took the fever Sept. 7th, 1841. Treated himself with castor oil; had much purging and vomiting. 11th. Mild symptoms of yellow fever. The next day his pulse and respiration nearly natural. 14th. Copious sweats; debility; scarcely any defecations for several days; skin and eyes very yellow. Treated with porter and broth. 18th. Case the same. 23d.

Yellow, feeble, and in bed. 26th. Scarcely any improvement. 30th. Cured.

W., born in Norway, aged 26, resident two years, became sick Sept. 1st, 1841. Labored one week after, feeling more or less pain all the time in his forehead and bowels. On the 12th, his skin and eyes were yellow. 13th. He insisted that he needed an emetic. 15th. Talked incoherently, his general symptoms growing worse. 16th. Nearly insensible; hiccups; orange hue of the skin, with much heat. Sept. 18th. Convalescent. Oct. 1st. Cured. Treatment: liberal diet; brandy, cups, foot baths, elixir vit., quinine, sinapism, two enemata.

1839. Aug. 14th. C., recently from New York, aged about twenty-five, seized on board the steam boat Hail Columbia, this morning, with slight chills, pain over the eyes, and in the back; the pain in the forehead, soon after became a diffused head ache; delirium followed. At 10 o'clock, A. M., I visited him; senses confused, skin hot, face flushed, eyes injected, pulse excited but gaseous. Two general blood-lettings and one free cupping; active mercurial purgations during two days, were followed by convalescence on the third day.

1839. Aug. 27th, was called to G. R. and wife, born in Germany; stout; resident in New Orleans one year; aged from twenty to twenty-five; taken in the night, with violent pains over the eyes, also in the back and muscles; vomiting; red eyes; hot skin; flushed face; copious hæmorrhages from the nose; hard jarring pulses. They were treated with blood-lettings, copious mercurial purgations, sinapisms, blisters, quinine. Both recovered perfectly in less than a week.

1839. Sept. 19th, 11, A. M.; called to see S. C., aged twenty-five, born in New York, resident in New Orleans one season, artisan in an iron foundry, of good constitution, sick two days, during which he took calomel, castor oil, etc. Pulse excited, but compressible; skin hot; face flushed; eyes injected and yellow; throbbing of the carotid and temporal arteries; frontal pain; back ache; tongue clean, etc. During the day, he was bled, (about twelve ounces), cupped on the nape and over the stomach (about eighteen ounces); had foot baths, cold to the head, mercurial and other purgatives; sinapisms to the abdomen; small repeated iced drinks. At dark, the febrile symptoms relaxed; sweats in abundance followed, together with sleep. Moderate purgation for three days, completed the treatment; on the fifth day he left his bed. He had a slight salivation.

Eleventh hour. 1847. Aug. 5th; 11, A. M. Irishman, aged 17, resident 3 years, extremely effeminate, was for the "first time in his life" taken with fever (which was not preceded by chills) eleven hours ago. Having been constipated, he took a dose of castor oil, which had failed to operate. Pain in the forehead; eyes prominent and injected; face and neck flushed; skin wet with perspiration; vomits; mouth, pulse and respiration nearly natural; hand and bend of the arm each 103°, axilla 103°. Bled 12 oz. from a small orifice; the flush receded, the pulse became smaller, softer and quicker; the pain of the forehead undiminished. In fifteen minutes after the blood-letting, the hand gave 101½°, the bend of the arm 101¾°, the axilla 103½°. Cured in seven days.

1847. Aug. 10th. A man occasionally resident for six years, a sufferer from intermittents, was affected forty-eight hours since with pain in the forehead, oppression of the stomach, and weakness. Took a purgative, was bled from the arm, which relieved the pain; skin dusky; pulse quick and jerking; eyes uninjected; tongue white; gums externally as if covered with a thin white film; hands 100°, axilla 106°. Recovered in two days.

1841. Sept. 19th. A large, muscular negro man, 30 years of age, belonging to T. B. Smith, of this city, was seized with all the usual symptoms of the yellow fever—with violent pains in head, back, limbs, etc. I abstracted from the arm 60 ozs. of blood, and in twelve hours had 20 ozs. drawn by cups from the nucha, and 20 ozs. from the epigastrium—making, together, 100 ozs. of blood taken in the first twelve hours of the attack.

He convalesced on the fourth day.

[This case is taken from the case book of the late Dr. Hester. He appears to have left on record but two brief cases.]

During the now prevailing epidemic, I have attended eight slaves, quite black, one belonging to the Commercial Bank, resident fourteen years; none were bled; all but one were purged with castor oil once or twice, were sponged with spirits, vinegar, etc.; cold to the forehead. The heat generally declined in twenty-four hours. Small, but few doses of calomel, blue mass, quinia, and morphia, were given. Of each of the first three named medicines, the whole number of patients did not probably consume one drachm, say six or eight grains each on an average; of morphia, perhaps not a half a grain each; mustard

poultices, simple enemata, etc. In about forty-eight hours there was, in almost all, great prostration; chicken soup, gruel, ale, porter, and in one case brandy, were given. One, pregnant, sickly before the attack, became alarmingly weak, and had, for ten days, slight hæmorrhages from the tongue and gums; no abortion; *now in statu quo.* Of the residue, the average confinement to bed was about five or six days. All recovered.

1841. Nov. 3d; M. *Self treatment*, by a Tennessee raftman; T. M., aged 35; says he has been in New Orleans and Carrollton thirteen days, working hard with his raft; health good until his attack of yellow fever, which was sudden and was ushered in with a chill, frontal and spinal pain, and "an aching all over," high fever, etc.; took fifty grains of calomel at once, and in twelve hours after a dose of castor oil; he took at intervals two more doses of the latter, which discharged at least one gallon greenish, brown, and black matter, "which removed all pain from the head and back, broke the fever; never had such relief in all his life." The next day he had severe cramps, with a moderate fever. The day following (yesterday) came to the hospital; at night became slightly delirious. Present state: rational; skin yellow, not hot; eyes much injected; pulse quick and soft; breathing easy, tongue moist, a little furred with white; urine copious. Convalescent. He complained, however, for several days of vertigo, and constipation, but regained his health in a week.

1842. Sept. 9th; *fortieth hour;* called to see Capt. K., born in New York; aged 31; resident in and near New Orleans eight years, excepting an absence of ten months at Galveston, and absences while voyaging to the latter; was taken the evening before last with the usual symptoms of yellow fever, for which he took an infusion of senna and salts yesterday morning; vomited the medicine; took calomel and jalap which purged "a dozen of times."

Sept. 9th; 5, P. M; pains over the eyes, in the limbs and back; face and neck flushed; skin a dusky red; eyes injected; respiration easy; tongue pale, moist, with white and yellow fur; temporal arteries distended; pulse rather active. Bled four ounces; fainted; the frontal pains relieved for a few minutes but returned; cold to the forehead; sinapisms to the legs and abdomen; cal., blue mass, quin., *aa.* gr. x. 8, P. M.; action of the pulse increased; repeat the dose as above; to be followed with castor oil.

Sept. 10th, 7, A. M.; slept some; oil operated largely; perspires; tongue cleaning; pulse fuller, slower, larger; repeat the cal., quin., etc., at noon; ice to the forehead; sinapisms.

Sept. 11th and 12th; improving; porter; quinine and blue mass pills; a dose of castor oil.

Three days after he resumed his command; about which time, I learned that the mate of his vessel died of yellow fever.

1843. Aug. 26th; M. H. C., born in England, aged 21, resident two years, laborer, sickly, last from Liverpool, taken with chills on the evening of the 24th, after working; had vomiting; weakness followed, without pain; has slight muscular tremors; pulse quick and soft; tongue large, and whitish on its surface; respiration and senses regular; eyes a little injected; face and neck flushed; hand 95°, axilla 103°. Treatment: purg. enema; ol. ric.; cups; cold to the head; drinks. Recovered in a week.

1843. Aug. 26th; 11¾, A. M. E. T. V., resident 6 weeks, aged 18, steamboat-man, sick since Saturday, 15th August. Yellow fever treatment began Aug. 22d. Bled to fainting; cal. gr. 6; foot bath; cold to head; bled again to fainting the same day; two cuppings, each 20 oz. The residue of the treatment, at different times, sinapisms, foot baths, liq. calcis, poultices.

Is a little stupid at night; respiration easy; pulse full, regular; has no pain except in one thigh; tongue thick, a little white on the surface; hand 102°, axilla 102°. Aug. 30th. Sitting up; no yellowness; convalescent.

1843. Aug. 27th; 1½, P. M.; air 87°. T. B., born in Ireland, aged 30 resident 11 days; sick 5 days. Was taken Aug. 24th; no chills; had frontal and muscular pains; hot skin; no vomiting; took no medicines; defecation regular; urine scanty; tongue broad, moist, and a little yellow and white on the surface; pulse, respiration and abdomen natural; slept last night; makes but little complaint, except of weakness; skin cool; hand 98°, axilla 101°.

Aug. 28th. V. S. *ad deliq.;* 64 ozs. of blood were taken before fainting took place; cold spongings; enema; porter; anodynes; sinap.

29th. Lemonade iced; porter.

Aug. 30th. Looks weak, cholera-like; no yellowness, no heat, no redness of the eyes, no flushing, etc; sleeps well. September 1st. Improves; no yellowness. September 3d, is convalescent. 4th. Better. Oct. 3d. Cured.

CASES DIAGNOSTICATED YELLOW FEVER.—SOME DOUBTFUL.—RECOVERIES.

1848. Sept. 16th; 11, A. M.; 4, P. M.; V. M., born in the city of Palermo, (Sicilly), seaman, aged 24, last from Philadelphia, resident four years, six days sick. Had been engaged as a fisherman, exposed to the sun; his comrade had just died of sunstroke, and M. supposed himself to have been "struck with the sun;" skin hot; complains chiefly of pain in his head and chest, on the 14th became insensible and comatose; quiet, free from muscular agitation; seems to suffer general paralysis, the limbs lying quietly in any position as they may be placed; skin sallow; eyes prominent, up-turned, lids partly closed, pupils contractile; liver firm, a little tumid, pressure upon it caused an inarticulate moaning; respiration 36, a little noisy, slight exaggeration of deep sleep; both lungs excepting their apices, gave dull sounds on percussion; pulse 126, firm. Hand 103½°; bend of the arm 103½°; axilla 105°. His physician pronounced him incurable, as I learn. Treatment: sponging with vinegar.

Sept. 18th, M. Symptoms the same with the following exceptions: pulse slower, larger, harder, fuller; respiration slight, but not noisy; the dulness somewhat increased towards the pulmonic apices. The eyelids are open, the pupils somewhat dilated; he directs his eyes staringly as if he saw; is still speechless; skin extensively hot; does not vomit; lies motionless on his back.

19th. M. Is better; cannot speak; opens his mouth when requested; tongue tumid, red, dry; no vomiting, respiration natural; pulse regular; skin cool and dotted with mosquito petechiæ; intelligence improving.

22d. Convalescent; speech restored; pulse small, regular, weak, tongue, respiration, pulmonary resonance, and senses natural; says he has resided in Louisiana ten years; the last four chiefly in this city where he had the yellow fever during the last year. This man did not regain his health until a month afterward. [*Sub-acute insolatian? Cerebro-pulmonic congestion?*]

1848. Sept. 16th, M. P. M., born in Brazil, aged 28, formerly a whale-man, lately a steamboat-man; resided seven years in Massachusetts, where he has a family, skin darkish or brown; hair long, coarse, straight, black, like the Indian's; last from St. Louis; resident in New Orleans one week, sick twenty-eight hours; taken with a chill, pains in the forehead, stomach, back, joints; costiveness;

senses regular; dejected; pains continue; vomited and had another chill to-day; respiration 28, uneasy, painful, with cough; pulse contracted, hard 110; eyes prominent, painful externally, red with injection, suffused with tears, staring wildly, oscillating rapidly, as if the muscles of the globes spasmodically affected; tongue, gums and abdomen natural; thirst; sleepless; hand 102⅛°; bend of the arm 103½°; axilla 104°. Treatment: cups to epigast.

Sept. 18, M. Convalescent; skin cool, complains of headache, sleeplessness only. 19th Sept.; well. [*Remittent ?*]

1848. Sept. 16th. A. S., a Dane, aged 23; last from Hamburg; resident one year; large and muscular; dejected; was taken six hours ago, previous to which his health had been good; worked as usual yesterday, and slept well last night; first symptoms: a slight chill followed by pains in the forehead and limbs, with hot skin; no vomiting. *Sixth hour:* no urine, no defecation to-day; pains continue; a muddy redish flushing of the skin, particularly of the face and neck; temporal arteries seemingly enlarged, hard, with a stroke nearly as large as the radial; eyes prominent, highly injected, glistening, tolerating light; pupils natural; tongue thick, whitish; respiration and pulse nearly natural. Hand 103°. During a blood-letting of one pound, lasting five minutes, the heat arose, in two m. to 103½°; one m. 104°; two m. 104½°. In half an hour after, the hand 102°; the axilla 105°.

The blood was nearly as red as arterial, in twenty minutes was completely coagulated, a few large drops of serum forming on the surface; the color still uncommonly red. The face and eyes were more red and injected, though the pains were diminished after v. s.

Sept. 18th. Dischared cured of "intermittent." [*A mild case of yellow fever ?*]

1848. Sept. 22d; M. F. K., aged 17, orphan boy, born in Germany, cabinet-maker; resident two months; taken three days ago with fever *unattended with chills* frontal, spinal, and muscular pains, without vomiting; took no medicines; urinates; defecates. *Present state:* Stoutly made, free from emaciation; muscular force diminished much; senses little changed; inclined to dullness; face flushed, eyes injected, pupils natural; volume of the tongue augmented, dry, coated with a black crust in the centre; thirsty; spits bloody mucus; acute abdominal sensibility, especially in the region of the liver; respira-

tion painful, unequal, imperfect, 42; pulse small, soft, variable 132; each lung dull except the upper portion. Hand 104°; bend of the arm 106°; axilla 106°. Sinapism to the epigastrium.

24–5. Had a chill yesterday, and one to-day, followed by hot skin. Pulse slower, tongue clean, etc. Convalescent.

1843. Sept. 4th, 11½, A. M. C. C., born in Ireland, aged 29, resident nine years; absent recently at the Lake where he worked ten days in the water; sick five days with intermittent as was supposed; was seized with frontal, spinal, and muscular pains extending to his knees; general prostration; respiration laborious, 45; pulse small, 130; eyes free from injection; tongue pale and pointed; no vomiting; defecates; urinates; hand 102°; axilla 109°. Treatment: lemonade. On the two following days this man appeared to be nearly well, excepting debility. [His case was diagnosticated as yellow fever.—*Intermittent?*]

1843. Aug. 22nd; 10, A. M.; C. W., born in France, aged 30, resident seven months; yellow fever for twelve hours; headache, etc;* bled twenty ounces; blood falling on the thermometer in the basin gave in five minutes, 98°, in 5 m. 100°; stationary. In an hour afterward, the hand gave 105°, the axilla 104°. 23d. W. says he is better; two days later convalescent; took a dose of castor oil; hand 98°, axilla 100°; 28th, discharged cured.

1843. Aug. 19th to 25th. J. W., born in N. Hampshire, aged 18, flatboat-man, last from Louisville; resident one week; took no medicine before entering the hospital. Treatment: bled to fainting; cupped; purgative enemata; sinapized foot bath; chlor. oxyd. sod.; gum water. Recovered in a short time.

1843. Sept 1–6. D. M., born in London, aged 26, resident ten months. Treatment: bled twenty ounces in the morning and sixteen ounces in the evening; ice; anodyne enema; castor oil; cataplasms; sinapisms; morphia; porter; quinine. Recovered.

1843 Sept. 2–6. J. W. born in Wales, aged 23, sailor, resident three weeks. Treatment: bled thirty ounces; cups; foot bath; sinapisms; enema; blue mass fifteen grains; blood-letting directed to twenty ounces; fainting from eight ounces; cups; phos. calcis. Convalescent on the fifth day; but recovered slowly.

1843. Sept. 9–11. C. S. born in Germany, aged 29; resident (as

* In this and the next half dozen of cases which are intended to indicate the treatment, symptomatic histories will not be copied.

steamboat-man to the Balize) eighteen months; bled (twenty-second hour after the attack) to fainting; foot bath; enema; same day again bled to fainting; foot bath; enemata. Convalescent; well, in less than a week.

1843. Sept. 9–11. J. M., born in Ireland, aged 25, steamboat-man; last from Bayou Sara; bled the first day to fainting; footbath; cupping; castor oil, poultices; seidlitz. Convalescent in less than a week.

1820. Sept. 3–7. (From the *Rcp. Phys. Med. Soc.*). Mr. M'Ghee aged 24; bled twenty ounces; *limonade cuite* with crem. tart., and sulph. magn.; cold affusions; cal. ℈i; castor oil ʒii; cont. *limon.;* stimulating clysters; cold to the head and epigast.; calcd. magn. ʒii. Convalescent on the 4th day.

1837. It has been said that a physician who is his own doctor has a fool for a patient. To this charge I am in a degree amenable, in as much as the physician in attendance, Dr. M. Morton Dowler, while my whole family was prostrated with the yellow fever, was himself undergoing the same New Orleans baptism.

On the 8th day of September, 1837, after having visited upwards of sixty patients affected with the epidemic of that season, I was taken with this malady, about dark, while walking the street, two squares distant from my office. My first symptom was debility. I was obliged to ask the aid of a gentleman's arm in order to support myself home. A dull aching in the forehead, limbs and back came on. After a few minutes' conversation with patients in waiting at my office, I went to bed from which I did not arise until a week had elapsed. During the night, the frontal pain increased, the skin became moderately hot; the stomach oppressed, attended with vomiting. During my sickness my understanding was but little affected, even when I was almost speechless. I was able, sometimes, to assist my physician with advice, concerning my family, a part of whom was taken just before, and the residue soon after myself. All recovered, under modes of treatment somewhat different.

Calomel, morphia, blisters, hot foot baths, ice to the forehead, sinapisms, a dose of castor oil and a small cupping, were the remedies which I used. On the fifth night after the attack, and ten or twelve hours after the fever left me, a clammy state of the skin, with coolness came on; my pulse became fainter and fainter, and at length,

nearly imperceptible; my breathing had nearly ceased; a sensation of suffocation, not of pain, induced me to make efforts to inflate my lungs, but my strength was, for a time, altogether insufficient. I was perfectly conscious of my situation. I supposed then, as now, that a very little additional depression of the vital powers would have deprived me of life, without any pain worth mentioning. Warm mustard baths to the extremeties, with warm plasters of the same over the chest, frictions, London porter, etc., were used with the happiest effects.

I will here add, that while the two doctors Dowler, whose treatment was then as much alike as the two Dromios, were down with the fever, Mrs. D., undertook to treat herself with large doses of calomel, jalap, castor oil, salts, senna; sinapisms, and two large blisters. She made a quicker recovery than any of the seven. Such was Old Physic, *anno*, 1837.

1838. Fatal Sporadic Case.—I will now give a mere synopsis of a case, the original notes of which occupy over twenty pages. Whatever may be said against the energetic medication pursued, I still think that the intensity of the disease and some of its features, are among the most extraordinary I ever witnessed.

F. T., born in Messina, Scicily, aged twenty, of vigorous constitution, of sanguine temperament, clerk to an extensive jewelry establishment in Chartres street, resident in New Orleans three months, requested my services, at noon, Sept. 2nd, 1838. For two days previously, he had suffered from giddiness, pains over the eyes, backache, vomiting, hot skin and costiveness, for which he had taken several doses of castor oil, from which he had but one thin, black stool.

Sept. 2. Universal heat of the skin, with dusky erysipelatous redness of the face and neck; eyes prominent and blood-shotten; throbbing, with distension of the temporal arteries; pain from temple to temple, above the orbits, not extending to the globe of the eyes or brain; tongue natural, except a slight whitish fur; suffers but little from pressure of the abdomen; respiration, easy; pulse, resisting; thirst; vomiting; manner anxious and hurried; senses regular.

About eighteen ounces of blood being taken from the arm rapidly, faintness, vomiting, abatement of the frontal pain, and a recession of the redness of the skin took place. The color, consistence and coagu-

lating power of the blood, natural. Mercurial purgation; sinapisms to the abdomen; ice to the head. During the night, copious black vomit stools, like fine soot and coffee grounds.

3d. In the morning, the heat of the skin was nearly natural; costive; full pulse; the frontal pain and throbbing returned the moment cold was withdrawn. Cupping from the nape of the neck one pound during the afternoon, with relief; mercurial purgatives; enemata; mustard foot baths; ptisans.

4th. Slept; abundance of black vomit stools, with a heavy, black sediment, and mucosity. Tongue, natural. Two blisters to the ancles, at dark. Foot baths.

5th. Same kind of stools, amounting to several pounds. Sinapisms to the abdomen; gummed drinks; two mercurial pills; restless night; sighing; unequal respiration. Abdominal and arterial pulsations.

6th. Bowels constipated; great quantities of urine; tenderness of the abdomen; profuse bleeding from the gums, which was checked towards night, by galls in powder, and in infusions. Poultices to the abdomen; castor oil during the evening; discharged black vomit matter, like ink, with a heavy sedimentary powder, insoluable in water. Perspirations came on at night; small doses of sulph. quinine.

7th. Stools like the last; from three or four pounds. Febrile odor extremely offensive, especially, that in the breath; hæmorrhage abated; pulse vigorous. Slept imperfectly. Towards night the breathing became irregular, and laborious, the stools suspended; skin dry; ptisans of Virginia snake root, sage and gum; warm bricks under the bed clothes; perspirations followed; blister over the stomach; chicken water; quinine mixture; foot baths.

8th. In the morning the hæmorrhage became alarming; fifteen to twenty ounces; urine three to four pounds, nearly colorless; skin moist, yellow; pulse small, tense, 100; somnolency; bowels costive; muscular power prostrated.

9th. Skin and eyes quite yellow; hæmorrhage abated; stools small, and black as usual, without any bilious tinge; slight incoherency of ideas, increasing in the night. Pulse and respiration frequent; heaving of the chest, and sighing. Features sharpened, sad; restless; tongue coated with a black fur; weakness; blue mass, calomel, quinine; creta; pounds of black vomit matter, discharged during

the night; vomiting. Extensive scrotal excoriations, which were treated with washes, olive oil, starch, etc.

10th. Tongue began to clean along its edges; gums natural; rested; pulse full and corded; breathing, and senses improved; urine about two pounds; tenderness of the abdomen; perspirations; bowels torpid; medicines continued; followed by castor oil. Inunctions with mercurial ointment, in which quinine and powdered red pepper were incorporated; had during the day a small chylous stool, for the first time.

11th. About three pounds of urine; bowels constipated; blistered surfaces recently dry, and of a dark red color, now became moist and paler. From lying constantly on the back, painful excoriation of the skin over the sacrum had taken place; this was relieved by propping the body in such a manner as to remove the pressure to the sides, alternately; mercurials and quinine; tapioca acidulated with lime juice.

12th. Stools thin and dark without black vomit matter; integument of the scrotum, still much inflamed; urine about two pounds and a half, saffron colored; pulse full and strong; blisters secreting well; chicken and rice water diet.

13th. No stool for twenty-four hours; tongue natural; expectorated for the first time, freely, a fine golden colored, thick mucosity; three to four pounds of highly colored urine, with sedimentary matter; pulse full; skin yellow and warmer than natural; seidlitz. In the evening, stools dark, and for the first time tinged with bile; mercurials.

14th. Urine about five pounds, very yellow, without sediment; stools small, dark, mixed with yellow and green, after castor oil; yellow expectoration abundant; coughs; has not complained of pain in the chest; sleep and senses natural; pulse full and corded; temperature rather elevated; strength and spirits much improved.

15th. Passed about five pounds of urine of a deep yellow color; perspired freely; mouth and tongue natural; changes from side to side without difficulty; slept well; has a good appetite; improving in strength and in other respects.

16th. The enormous excretion of urine continues; slept well; perspires.

He eat freely of eggs, chicken, etc.; some hours after, the skin became hot, the pulse excited; had slight vomiting; bowels constipated; castor oil, which discharged copious, natural, semi-solid stools.

17th. Urine but little diminished; stools yellow; mouth dry; pulse excited; external veins prominent.

18th. Urine about two pounds, which greatly augmented during the evening, assuming a high, almost reddish color.

In the evening the fever reäppeared with great violence; heat pungent; pains over the eyes and in the legs; arterial throbbings especially in the temples, etc.; bowels constipated; calomel, to be followed with castor oil.

19th. Scanty natural stool; urine about one pound during the previous night, but the excretion was now suspended during the remainder of the day; pulse small, hard and frequent; respiration hurried and gasping; sighing; vomiting; skin dry, hot; great heat of the face and head; ideas incoherent.

In the evening raving delirium; tried to get out of bed; universal muscular spasms or tremblings; heart and arteries throb violently, agitating the chest and abdomen, and apparently sometimes the extremeties; mouth and tongue parched. Cold to the head; sinapisms; aperient; enemata; cups. In a few hours the delirium, heat and rapidity of the pulse declined; but in the night, the symptoms returned with redoubled intensity; brain oppressed; violent convulsive spasms affected one half of the body longitudinally; the other side paralyzed; eyes agitated and divergent in their direction; the tongue when protruded turned towards the convulsed, non-paralyzed side; the jaws became immovably fixed; veins collapsed; bowels constipated; insensibility.

20th. Died.

FATAL CASES.

The following I have translated from the French. It is one of the six model cases reported by the *Phys. Med. Soc.* This case which occurred thirty-eight years ago, is a type of scores of cases which now, *anno*, 1858, daily pass to the realms of death.

1820. Aug. 21st. I visited this evening Mr. L., Capt. (of the ship Alexander) resident fifteen days; the Capt. has very often voyaged to the ports in the Antilles; aged 53, born in France; sanguine tempermanent; face highly colored [reddish]; walking about his room greatly agitated; very loquacious; pulse feverish; tongue humid and white; lassitude; pains in the knees; very violent headache; with-

out appetite; all of which he maintained were nothing to his mental sufferings. He said he wanted no medicine. Nevertheless, I advised acidulated drinks, emollient enemata, with sinapisms to the legs.

Second day of the disease.—The same lasitude and pains; intense headache; redness of the eyes and face; pains in the back and epigastrium; nausea; pulse large, soft, intermittent, accelerated; tongue whitish; skin a little humid and burning with heat; no thirst; bloodletting fourteen ounces from the arm; for drinks, cream of tartar, with nitre, in sugared water.

Third day.—Same symptoms; frequent vomitings, at first, grayish and afterward black; pulse softer and slower. I apprised the patient that in three days he must die. Treatment: anti-emetic potion of carb. pot. and citric acid; sinapisms to the legs.

Fourth day.—Morn.; sleepless night; headache diminished; appearance of astonishment *(facies d'étonnement)*; eyes red; yellowness around the lips; epigastrium painful on pressure; pulse large, irregular, low; suppression of urine; both the vomitings and stools, black.

Evening.—Abdominal pain less marked; hiccup; unavailing attempts to urinate; stools bloody.

The fifth night.—Greatly agitated; several stools containing much pure liquid blood; mind calm and vigorous; abdominal pain; thirst; hiccup; fainting; and, at 4, P. M., death, on the 25th of Aug. 1820. I found it impossible to make a post mortem examination.

No. 1. The distinguished lawyer and writer, Sampson, took the fever the same day and died at the same time with the French captain. The following are some of the items of his medication: twenty ounces of blood from the arm; cal. fifteen and jal. twenty grains; two pounds of blood; jalap twenty grains; crem. tart.; castor oil one and half ounces; snake-root; peruvian bark; calomel and turpentine often; cal. and musk; "*champagne for common drink*"; blister; calfive grains every two hours; frictions with mercurial ointment repeatedly; turpentine teaspoonful doses; whereupon, "the patient resolutely refused any more medicine." He died two hours ahead of the Capt.

No. 3. Rev. S. Larned, treated with the Sampson medication, and died on the fourth day.*

1841. Sept. 20–29. P. M., Irishman, aged 21, a laborer. During

* No. 2. Mr. Brown, began with venesection *four pounds*; next day twelve ounces; blister; third day cob-web, three grains, every two hours. *Recovered.*

eight days he was treated with two blisters, several mercurial inunctions, fifteen grains of blue mass; quinine; elixir of vitriol, spirits of camphor and of nitre; porter; nitro-muriatic acid, sponging. Symptoms, two days before death: skin hot, dry, yellow; respiration quick; cough; frontal heat, pain and arterial throbbing; tongue broad, moist, and of a natural color; pulse quick and corded; answers a question with intelligence, but quickly falls back into a half sleeping state, attended with quiet or whispering delirium; moderately emaciated. 29th, died.

1841. Sept. 16–22. L. A., German, aged 26; sick several days; muscular; large; skin yellow and moderately hot; breathing regular; hiccup; pulse gaseous and irregular; tongue healthy; has no fixed pains; senses regular.

Five days later.—Skin very hot and yellow; lies on his back; mouth open; tongue dry; teeth encrusted with foul, black matter; eyes yellow; breathing regular; pulse regular but hard; moans; no abdominal tenderness; no delirium. Treatment: foot bath, porter, blister, Dover's powders, ten grains of calomel, elixir of vitriol, prunus Virginiana, bi carb. sod., brandy. Died.

1841. Sept. 10–15. H., Irishman, aged 25, last from St. Louis, resident one week; went to a fire on Commerce st., where he got wet in a shower; September 10th, took quinine, on the 12th, his senses and functions being almost natural, except frontal pain, noise in the ears, and dizziness. On the 13th, after his discontinuing the quinine, the sounds in the ears disappeared; the pulse being gaseous, had two enemata, but says he has had no defecation for two days. 15th. Died on the fifty day of the fever.

1841. Sept. 3–18. M. F., German baker, aged 36, resident one month, was attacked with yellow fever September 3d; on the 7th, he entered the hospital, where he was treated with porter, cups, footbaths, mercurial frictions, extract of barks, blister, wild cherry-tree bark, carbonate of ammonia.

9th day. Skin hot, and of a dirty yellow color; breathing and pulse regular; eyes, red, yellow, dull; muddy and yellow hues, with a stupid expression of the face; sometimes changing to that of fright, and intense pain; delirium; got out of bed, carrying his bedclothes with him; but is obedient when spoken to sharply; is always restless; a blister on the abdomen has produced dry inflammation, and is becoming gangrenous; foul, dark matter on the teeth; tongue natural.

Sept. 16th. Symptoms aggravated ; pulse gaseous and irregular ; trembles; moans; features expressive of agony.

Sept. 17th. Trembles from head to foot; pulseless; tongue dry and foul; intense agony stamped on every feature.

18th. Died, after an agony of six days, most of which time he appeared to be dying. The pain, during all this period, appeared to flash like lightning over the entire body, at short intervals, while the countenance mutely, but strongly expressed the deepest anguish mingled with sadness, fright, and a bewildered consciousness.

1841. Sept. 14. T. C., born in Ohio, aged 22, steamboat-man, resident three weeks. Second day of his fever; diarrhœa; vomiting of black vomit mixed with brown; scanty urine; hot skin. Treatment: cupped; took carb. soda, snake-root, quinine, an enema. Died on the fifth day.

1839. July 30th. I was called to an engineer, aged 25, resident, chiefly in the South for the last four years; robust and muscular; ill for six days; black vomit for forty-eight hours; treatment hitherto by Thompsonian doctors. 4, P. M ; pulse natural; breathing somewhat laborious ; power of swallowing lost ; copious nasal hæmorrhage ; vomiting of a black and brown fluid as thick as molasses, in large quantities without straining or effort; universal tremors of the muscles, with slight subsultus tendinum; bites the spoon strongly; jaws clenched; mind torpid; stupor, without raving; the head and trunk hot; the extremities cooler than natural; the skin moist; was dotted on exposed parts with mosquito-petechiæ or extravasations; much discolored everywhere, with a dirty yellow hue, except upon the dependent parts, which sustained the weight of the body, where large, livid spots existed; eyes yellow. Sinapisms to the extremities; ice to the head.

7, P. M; pulse full, 80; breathing laborious; eyes projecting, widely open, fixed in an immovable stare, without winking; spasmodic jerkings of the muscles, a pendulum-like vibration, causing the body to move from side to side; total insensibility. Died, seven hours after; the body soon after, became intensely yellow.

1842. Aug. 27th. W. H., born in Germany, aged 19, last from Mobile, resident five months; fifth day of his malady, sad, vacant, restless expression of countenance; low, muttering delirium, whispering the name of a female almost incessantly for several minutes;

turned from side to side; tried to get out of bed; free from emaciation; muscular and fat; had thrown up much black-vomit upon his person and bed clothes; appeared to suffer nothing from pressure upon the abdomen which was free from distention; muscular agitations or trembling; eyes yellow and half closed; mouth open; sordes on the teeth; tongue slightly reddened at the tip; the skin naturally fair, had a faint yellow tinge, and a healthy temperature; the face discolored with large dull scarlet spots; respiration easy, but quick; the pulse nearly imperceptible in the wrists and instep. Died next day; having been treated with cups, morphia, quinine, a foot bath and a blister.

1841. J. D., born in Scotland, aged 25, resident ten days. 15th; sick twelve days; senses regular; complains of "being sleepy all the time;" says he rests well at night and his fever has lasted only eight days; skin moist; pulse slow, regular and gaseous; eyes red; pupils dilated; tongue dry; vomiting slight. 16th. Black vomit; copious bleeding from the nose; skin moist. Treatment: castor oil; sulph. quin.; cal. twenty grains, with opium one grain; cal. 10 grains, opium half a grain; porter; foot bath; a blister; acet. plumbi and cal., each eight grains. 19th. Died.

1841. Sept. 12–13. M., aged 25, a stout man, resident one year, last from N. Jersey, sick two days. Treatment: grog, morphia, a bath, sponging, twenty grains of blue mass, and a blister. He had, the day before his death, an idiotic countenance, noisy respiration, a pulse of one hundred, a dusky yellow skin, black matter on the teeth and tongue, talked both sense and nonsense incessantly; though tied, he presented an example of perpetual motion.

1841. Sept. 12–13. —— young man, face and neck flushed, as in erysipelas, with heat and a dusky yellowness of the entire skin; pulse natural, except gaseousness; temporal arteries enlarged, prominent, and throbbing; breathing unequal, sometimes natural; tongue dry; tied by his four extremities to the bed posts; screams loudly, as if frightened, attempts to get out of bed. Treatment for three days; blisters, morphia, quinine, camphor juleps. Died on the next day.

1841. Sept. 19–21. M., Irishman, aged 22, resident one year; fourth day of his fever, and two days before death, had, besides the usual symptoms, a double pulse, after the preliminary, vigorous stroke, there was a weaker secondary or back stroke, less forcible, which

followed immediately; black vomit for several days. Treatment: quinine, snake-root, carb. sod. cups, enema.

1841. Sept. 25–8. B., born in France, a saddler, aged 28, resident six months, sick six days; breathing and tongue natural; eyes and skin yellow; pulse gaseous. 28th; pulse regular, and hard; respiration laborious and noisy; black sordes upon the gums and teeth; coma, insensibility, and death. Treatment: cups; blue mass, thirty grains; a blister; chlor. ox. sod.; porter.

1839. Sept. 4–7. J. F. M., aged 20, late of Texas, a carpenter, of good constitution, sick twenty-four hours, required my services at 2 o'clock, P. M., September 4th; universal heat; frontal pain acute; eyes and face red and flushed; arterial throbbing in the temples, and forehead; pulse strong and jarring; tongue red; nausea and vomiting; he says, he "feels as if coals of fire were in his bowels." Mind dejected and anxious.

5th. Watery purgation; subsidence of the frontal pain; abdomen free from tenderness. Thinks himself almost well. Fever increased at night, with delirium, restlessness and vomiting.

6th. No urinary secretion; heat of the skin diminished; pulse gaseous; eyes injected and yellow; the face of a dirty yellow hue; restless, not deranged; desires to be changed from bed to bed; passed a little urine in the night; had mucous stools, mixed with black vomit matter like coffee grounds; paroxysms of difficult breathing.

7th. Has fits of difficult respiration and loss of speech; belches; pulse quick and gaseous. Intellect but little affected; skin somewhat yellow. Died at five o'clock in the evening, after about four day's illness. Treatment: calomel, castor oil, blood-letting from the arm; sponging; mustard and bran poultices; gum water; blisters to the ancles, aperients chiefly blue mass; cold to the forehead.

F., born in New York, aged 19, steamboat-man, entered the hospital September 27th, 1842.

Oct. 5th. Noon. Person stout, robust, free from emaciation; senses natural; says he left St. Louis nearly three weeks since; three days before and three days after his arrival in New Orleans, he had a daily chill followed by fever, for which he took calomel and castor oil, which operated freely. [Dr. * * * who attended this case regarded it as a pure intermittent, as he assured me; the patient having become convalescent in a few days after admission, got out of bed too soon and

after walking about less than hour, became almost instantaneously yellow, accompanied with other severe symptoms of yellow fever.] Slight spinal pain; eyes yellow and red, bearing a full light without pain, pupils natural; inclines to shut the eye-lids; skin cool, moist, and dotted on exposed parts with mosquito petechiæ; tongue clean, slightly reddish and dry, gums spongy, teeth covered with sordes; thirst for cold drinks; no vomiting; no hæmorrhage; urinates; defecations thin and black; flatulency; respiration imperfect, a forced inspiration gives no pain; pulse soft, small and quick; sleep of the preceding night disturbed with dreams, without delirium; chief pain from a blister over the stomach. Oct. 2nd, cupped; 3rd, calomel and blue mass, each 5 grains; 4th, blister; carb. soda; 5th, magnesia.

During the night of the 5th, black vomit came on, and next day he expired tranquilly, his senses not being deranged to the last moment, not apprehending any danger as long as he could speak.

1843. Aug. 4–10. G. P., German, aged 25, sailor. August 4th, foot bath; cups to epigast.; cold to head; enema; 5th, cup mastoids; cold to the head; sinapisms; iced gum-water; s. quin., twenty grains in enema; sinapisms; 6th, s. quin. ℈j. every two hours in enemata; sinapisms; phos. cal. ʒij, in mucil. gum arab. ℥iv; tablespoonfull every two hours; bicarb. sod. ʒij; syr. morph. ℥j, mucil. ℥iv; tablespoonful every two hours; blister epigast., six by eight in.; 7th, quin. ʒss. in enemata, every two hours; iced porter; blisters to the legs.

Aug. 10th. Morning. Condition: Skin moist, cool, dotted with mosquito petechiæ; eyes injected and faintly yellow; tongue natural except a little white fur at the base; vomits his drinks; hiccups; pulse regular but contracted; respiration easy; pains in the limbs and back; quiet; senses normal; healthy fæcation yesterday; now discharges claret colored blood only. Evening: black vomit; delirious, picks at the bedclothes; pulse feeble; skin cool, soft, moist; eyes redder and more yellow. Died.

Aug. 30–Sept. 3. J. S., a German, aged 22, resident four weeks, late from Savannah; second day of the fever, bled sixteen ounces; foot bath; third day, elix. vit.; syr opii; fourth day, poultice; cal. ℈j; sulph. magn.; enema. On the second day, coma; pulse small and excessively rapid; respiration loud, puffing, with distention of the cheeks; eyes closed, yellow; skin a lemon hue; two violent convulsions; raves at night. Died, three days afterward.

1843. Aug. 26–30; 1¼, P. M. J. M., born in Massachusetts, aged 36, laborer, last from Havre, resident two months, sick five days; taken on the 20th without chills, after having worked in the water four hours; had pain in the top of the head; pains in the back; chilliness followed with vomiting and fever. Present state: face, neck and eyes a little red; tongue whitish on its surface; respiration unequal; abdomen, senses, etc., natural; thinks he is better. Hand 100°; axilla 102°. Treatment: Aug. 25. Hot mustard foot bath; cold to the head; bled to fainting; then ten cups; anodyne enema; sinap. 26th. Bled; purg. enema; poultices; ice. 27th. Sinap. cal. gr. ii; sub. carb. sod.; anodyne enema. 28th. Purg. enema. 29th. Purg. enema; quin. gr. ten. 30th. Sinap. Died.

1843. Aug. 29th–31; 11½, A. M. A. R., born in Germany, laborer, aged 28, last from New York, resident eight days, sick four days; taken without chills; had frontal, spinal, and muscular pains, vomiting; had chilliness second and third days; took a purgative; was cupped. Respiration quick, 40; vertigo; pulse soft, equal, 75; was bled to fainting, twenty-four ounces twenty minutes since; urination, defecation, and senses regular; countenance dejected; thirst; tongue a little dry and a little discolored by a slight yellowish and white fur; eyes uninjected; no flushing of the skin or yellowness. Treatment: v. s. *ad deliq.;* mustard foot bath; ol. ricini; the latter in three hours produced two copious liquid, black, coffee-colored stools. At 12, M., twenty minutes after v. s. when the blood (about twenty-four ounces) was begining to coagulate at the circumference, the thermometer was immersed in it; in five minutes it arose to 99°; in ten it was 97½°; in ten to 95°. The blood scarlet red, coagulated, but was not cupped; in one hour the clot began to lose hold of the sides of the bowl; in two hours was well coagulated, elastic, contracted, with considerable serum around its margins. The scarlet color was external only; considerable frothiness remained on the surface. In three hours the serum augmented as in healthy blood.

The temperature of the hand (which was hard from labor) gave in twenty-six minutes 104°; the axilla 107° nearly. Evening; spine cupped twenty ounces.

Aug. 30; 11½, A M.; general improvement; little pimples breaking out on the lips; sleepless; no yellowness; or vomiting; pulse soft, quick, weak; respiration quick, attended with an occasional sigh.

Treatment: chlor. oxyd. sod. Hand 96°; axilla 102°. Became very yellow; no black vomit or hæmorrhage. Died Sept. 1st.

1843. Sept. 3d; 2½, P. M. J. W., born in Germany, aged 21, resident two days; steamboat-man, last from Louisville, sick five days; rather emaciated; skin a little yellow, dotted with musquito petechiæ; eyes yellow, half closed; pupils dilated; comatose; respiration noisy, moaning, 40; pulseless; copious anal hæmorrhage; bled last night from the orifice made in his arm, Aug. 31st, when he was bled to fainting; then cupped. Sept. 1st. Enema; foot bath; 2nd, cupped freely; phos. cal. ʒi, in mucil. gm. acac. ʒvi: spoonful doses. Axilla 98½°; hand 88°. Died the same evening at 5, P. M.

1843. Aug. 30, M. J. P., born in Denmark, aged 41, last from Savannah, sick nine days; resident sixteen months. I had seen him several times without taking notes of his case; a strong muscular man. Now totally insensible, deep coma, loud, stertorous respiration; sighing as if immersed in cold water; eyes yellow and injected, suffused with whitish exudations; pupils natural; pulse imperceptible; skin yellow, and covered with perspiration; limbs motionless. Axilla 102°; hand 95°. The breath appearing comparatively cool the thermometer was held nearly touching the lips so as to receive on its surface the inspired and expired air; the mercury fell to 93°; room about 85°. Died the same night. Treatment: 28th, cup nape ʒxii; epigast. ʒxii; sinap. foot bath. 29th, cup nape ʒxx; mustard foot bath; enem. cathart.; sinap. epigast.

1847. Aug. 4–9. H. H., born in Stockholm, sailor, aged 26, resident four months, last from Liverpool; bled twelve ounces from the arm; sinapized; enema colycyn.; cold to the head; sponged with warm vinegar and water; iced lemonade; iced milk; decoct. rad. rub. villosi.; starch and laudanum injections. Second day of the fever: Skin bronzed; respiration, pulse, tongue, gums, nearly natural; hand 103°. Third day: Quiet; a good deal of stupor of mind; bronzed skin; pulse large, slow, full; no vomiting; tongue tumid, except the tip, which is red and pointed; the dorsum is white; gums have a white apthous efflorescence; hand 101°; popliteal 101°; axilla 103°. Died Aug. 9th, 11, A. M. Cadaver very yellow.

1853. July 21st–23d; 11, A. M. C. S., aged 28. Second day of the fever: Intellegence impaired, but answers questions; uses the bowl to receive his copious and frequent gushes of black vomit; at-

tempted to drink, but fell back in a convulsion, and, for a time, appeared to be dying; stout, muscular, and lean; general muscular tremors; whispering motions of his lips; eyes prominent, red, yellow, and glistening; pupils contracted; mouth open; gums, fauces, and tongue dark, red and smooth; a few musqueto petechiæ; face and neck of a dusky, purplish red, with yellowness in places; chest a faint yellow; respiration 16, hard and convulsive or jerking; pulse small intermitting, 160; inclines to sleep; both lungs dull sound towards the base, particularly in the left; in the cardiac region, he says, is the seat of the chief pain; abdomen supple. Hands 100°; bend of the arm 102½°; axilla 104°.

Treatment: July 21st; cal. thirty grains, opium iii grs., ipicac twelve grs. in twelve pills; one every two hours. 22d. Infus. ulmi. 23d, ice water. Died at mid-night.

1853. July 14–26. [The following case and a good many other cases were treated by an able physician, very much after the hydropathic method, with wet cold clothes, etc.; but with no satisfactory results, according to the physician's opinion; he also, gave porter, iced milk, etc.] Ninth day of the fever: (One day before death.) Bend of the arm 101½°; axilla 102½°. Somewhat emaciated; gums and teeth covered with a black sordes; mouth widely open, corners depressed; tongue red, smooth, retracted and tremulous with convulsive motions, which latter affected the lips, fingers, and arms, the eyes, and eyelids; drinks with difficulty; speechless; pupils small; breath offensive; eyes yellow but closed; skin yellow; fists clenched; respiration irregular, noisy, 36; intelligence almost completely gone; moans. Two hours latter the skin was growing cool; and a cold, clammy sweat was exuding. His whole aspect repulsive; his countenance indicative of pain.

1858. Sept. 14th; 10, A. M. J. S., born in Bavaria, aged 23, resident two years; a clerk, but recently has gone out into the open sun in the afternoons to peddle; muscular; general good health; has been drinking freely, especially when taken sick; slept well as usual until 6, A. M., when on rising from bed, he was seized all at once with great debility, pains in the forehead, back, and limbs, and became suddenly unable to turn in bed; had no initial chill.

Fifth hour.—The above described symptoms continue; slight chilliness; hot copious perspirations; eyes injected, kept closed; tongue

tumid, moist with increased redness; thirst; muscular system agitated; disinclined to answer questions; mind dejected; face much flushed as is the entire skin; whole surface very hot, about 110°; breathing uneasy and quick, sometimes panting; pulse quick, concontracted, soft about 110; urinates, general capillary congestion of the skin. Cup nape and spine sixteen ounces; sponge with whisky and venegar repeatedly; apply mustard cataplasms to the legs and anterior walls of the trunk, to be followed with flax-seed poultices; pounded ice in a beef's bladder to the forehead; iced drink; three powders of ferrocyan. quin. and sub. mur. hydr., each about ten grains, two hours apart.

5, P. M. Has vomited several times; two natural stools; pulse larger, about 110; thirst diminished; respiration quiet; mind calmer; skin moist and less congested; heat declined; bend of the arm 105°. Castor oil to be followed, if necessary with enemata; after purgation, powder, two grains cal., three ferrocyan. quin., two grains dover, one-fourth gr. morph.

A severe case, but is there any thing in this diagnostic description peculiar or indicative of severity?

15th, 10, A. M. Free evacuations from the oil; took the powder at 10, P. M. last night; takes the other now; slept some; thirst and heat less; otherwise little change.

3, P. M. Pulse 110, full; heat considerable; perspires; head, back, and limbs painful; tongue slight yellow fur; thirst; three stools; urinates; cold to the head still; chicken water.

8. P. M. No change; take one-fourth gr. morph.

16th, 9, A. M. Heat about the same; urinates; bowels quiet; pulse firm, about 110; no marked change. Sago; drinks; sinapisms; flax-seed and hop poultice to the abdomen.

1, P. M. No material change; urinates very *abundantly;* drinks; sago and gruel diet.

7, P. M. Heat continues though diminishing. Quin. sulph. ℈i; morph. sulph. gr. iss; acid. sulph. arom. ʒi; syr. simp. ℥vi. M. F. Solut.; give one fourth for enema; to be repeated at midnight if sleepless.

17th, 8, A. M. Heat, pains, etc., greatly diminished; take a table spoonful of the remaining mixture every two hours.

1, P. M. Nausea and vomiting; urinates much; pulse 88, soft; skin moderately hot; take of the solution two table spoonfuls for enema.

9, P. M. The same; tongue swollen and congested about the frænum; hiccup.

18th, 9, A. M. Weak; gruel; chicken broth; sparkling Catawba wine; an enema of the above mixture $1\frac{1}{2}$ oz.

4, P. M. Thinks himself quite cured; said, I "might visit *once* more." In the night became restless; took an enema of the above mixture, and slept a while quietly.

19th, 11, A. M. Restless; anxious; retches; hiccups; urine diminished; peculiar offensive febrile odor; pulse 80; respiration quiet; senses regular; drinks; chicken water.

5, P. M. Eyes yellow and injected; heat above the natural standard; nausea; great restless; pulse regular; hiccups; skin dry, tongue tumid, ecchymosed, red, and dry.

20th. 10, A. M. Condition the same, urinates very copiously-rested badly; was somewhat delirious; subsultus tendinum; picking the clothes; answers questions, but inclines to a semi-comatose stupor; chicken water.

5, P. M. Eyes yellow; inclined to sleep; is delirious; pulse about 75, soft, spits blood; hiccups.

21st. No change, except greater prostration; more wakeful; mind clearer.

Evening. A neighbor gave him a large dose of castor oil, on his own responsibility; copious purgations of blood, black vomit, and a clear albuinoid fluid, estimated at a gallon; great debility; stools involuntary; London porter freely.

22d. Unchanged except the gums, tongue and lips which are coated with a black sordes; senses dull; intolerable odor; pulse soft; eyes yellow and injected; skin a dirty yellow; porter.

23d. Sinking; porter.

24th. Died at 6, A. M.

(To be Continued.)

ART. V.—*Letter from* WARREN STONE, M. D., Prof. of Surgery, University of Louisiana.

PARIS, August, 1858.

MR. EDITOR: Agreeably to your request, in your last, I send you such professional items as I think may be worth reading, and if you think proper you may print them. My time has been too limited in each place to make observations and draw conclusions of my own, and I have endeavored to obtain the result of the experience of others whose experience is large, and whose authority is high, on some subjects not yet entirely settled, or upon which the profession is not entirely agreed. It is due to the members of the profession here, to say, that they are extremely courteous and affable to strangers, and afford every facility for inquiry and observation. It is not so easy, however, to get a positive or an unbiassed opinion as I expected.

The practice of resection in the lower extremities is still continued in many of the London Hospitals, and it is natural for those who practise it, to sustain the principle. I have always doubted the propriety of these operations except under very favorable circumstances, and as no such favorable circumstances have existed in any cases of diseased joints that have come under my care, I have never made the operation. The favorable cases that were shown me in the Hospitals of London, confirmed me in my opinion. The unfavorable cases, of course, had passed from observation. I did not have precisely the proportion of the favorable and unfavorable cases, but it was acknowledged that resection in the large joints of the lower extremity did not afford the same security to life as amputation. It is proper, however, to say that the operation was not strongly urged, even by those who practised it, but they dwelt upon the great importance of saving a limb, and of conservative surgery. I did not see a case where the limb, after resection, was as useful as a well adjusted artificial one would have been, and then it is much more liable to get out of order and vastly more difficult to mend.

In passing through Stevens' Hospital in Dublin, Mr. Collis, the surgeon, called my attention to one of the successful cases of resection at the ankle joint. The operation was not made by Mr. Collis, but it was evidently well done, for the wound healed well, and the patient was able to walk very well for a year; but the part was rather tender, and finally the bones became diseased, and he came to the Hos-

pital two years after the resection with caries and fistulous openings. I could not get the exact history of this case; but he appeared to be what I would denominate a favorable one, that is, he did not appear to be a scrofulous subject.

The most favorable case is that in which the disease is the result of synovial inflammation, suppuration, and ulceration of the cartilages from the surface, leaving the heads of the bones healthy, with the exception of the carious surface. This appeared to be such a case, and if such a bone could not endure the weight of the body after its natural articulating surface had been removed, what prospect can there be in a case where the disease commences in the cancellated structure of the head of the bone and ulceration of the cartilages commence from the bone, and when the bone cannot be healthy? In such a case, the limb may be preserved at some risk to the life of the patient, and the destructive influence of the disease may be arrested; but why this risk of life without a prospect of adequate gain? or why deprive the patient of the prospect of a more complete cure and a better locomotive apparatus, furnished by art.

Mr. Syme of Edinburgh, who has done very much towards establishing this operation, told me frankly in a conversation on the subject of resecting, that it would not do on the lower extremities, and that he had given it up. No one has had better opportunities for arriving at a correct conclusion on this subject than Mr. Syme; and, perhaps, there is no one more competent to draw conclusions from experience. With the upper extremity it is quite different, as no valuable substitute can be supplied for a lost arm, and any use of the arm, however small, is valuable. Mr. Syme amputates at the ankle joint, in preference to making Chopart's operation, even when the os calcis and astragalus are sound, as the operation through the tarsal bones makes a bad stump. I saw him make the operation in a case where the foot had been amputated, leaving the astragalus, calcis, and I think, the scaphoid bones. The patient had never been able to use the stump well, and finally, it became tender and painful, and amputation was necessary. The operation was made by first making an incision from one malleölus to the other through the sole of the foot down to the bone, and then the soft parts were dissected from the os calcis so as to form a flap of the tough granular tissue of the heel; a similar incision was made in front, and the limb disjointed. The malleöli,

together with the articular surface of the tibia, were sawed off, and the flap applied to cover the end of the bone. A case was brought into the Amphitheatre on whom the same operation had been made nine months before, and the stump was perfectly sound, and apparently as insensible to pressure as the actual heel.

The operation for the radical cure of inguinal hernia as modified from Gerdy was considerably in vogue in London, and I took some pains to ascertain the result. Mr. Spencer Wells has made a favorable report of quite a number of cases, but I was not able to see him. Mr. Lawrence, at St. Bartholomew's, has made several operations, and he told me that although he retained the instrument longer than is recommended, the plug of inverted skin came down in a short time; in some cases in a few days. Mr. Brooke, of Westminister Hospital finding that the plug was not retained, very sensibly concluded that it must do more harm than good by keeping the original canal dilated during the process of inflammation which is produced by the instrument, and proposed to use the instrument (or a modified one) to carry the needle through the original canal so as to excite inflammation, but without inverting the integument, or making use of it as a plug. The impression of dispassionate observers is, that this operation may effect a cure in recent cases of hernia, when the obliquity of the canal has not been disturbed, in such cases as a well adapted truss often effects a cure, but that it is of no use in old cases where the obliquity of the original canal is changed into a direct opening by the approximation of the two rings. The truth is, like all other operations which have been instituted for the radical cure of hernia, from the time the royal stitch was practised to the present time, it will prove so near a failure, that after the novelty of it has lost its effect, it will go into disuse. Novelties are as eagerly sought for on this side of the water as with us, and for the same purpose. Novelty brings notoriety, and notoriety is just as near akin to fame, and just as valuable here as with us.

The subject of ovarian tumors being one on which the profession is divided, I have taken some pains to inquire into. Mr. Baker Brown, in his work on the surgery of females, has an interesting chapter on ovarian cysts, and speaks of the iodine injections, and refers to Mr. Simpson's experience which is rather encouraging. I conversed with both of these gentlemen on this subject, but they have not added materially to their published experience. Mr. Simpson, of Edinburgh,

has injected as much as eight ounces of the tincture of iodine of the Edinburgh preparation, which is stronger than ours, and never has seen any serious inconvenience. He considers that in cases of a simple cyst, the effusion may generally be arrested by the injection, but in multilocular cysts it fails. There is no experience in the repetition of the injection, as would be required undoubtedly if there were more cysts than one ; but it is reasonable to suppose that each cyst, where there are more than one, is as curable as the single one. If a moderate proportion of these cases can be cured by injection, the treatment ought to be adopted in preference to extirpation. I consider that extirpation is only justifiable in these cystic enlargements of the ovaries, for they will enlarge in a majority of cases so as to distress the patient very much, and if tapping is resorted to for relief, the patient (who may otherwise be in very good health) will soon be exhausted in a majority of cases by the drain, and in such a case extirpation might be justified.

Dr. Trousseau, who is probably the best authority in Paris, has used injections effectively, and thinks it will be found to succeed in cases where the fluid is clear. He punctures with the trocar, and introduces through the canula a gum elastic catheter which he leaves in for the purpose of inducing adhesion of the cyst to the abdominal walls, and to afford a discharge to any fluid that may accumulate in the cyst. The injection is repeated after the cyst has contracted down, if the fluid continues to be effused, he injects four or five ounces.

Dr. Trousseau also injects the cavity of the pleura in cases of empyema. The matter is discharged with the trocar, and the compound tincture of iodine diluted with two parts of water, is injected, and the patient placed in various positions so as to diffuse the injection over the whole diseased surface, and then it is withdrawn and the opening plugged with a wax bougie, so as to keep it open for the discharge of matter, and for further injections, which are generally necessary.

Dr. Francis Churchill is attracting some attention by his treatment of tubercular affections by the use of the hypo-phosphites. I was much pleased with the man ; he was very affable and explained to me his views of their action, his method of using them, and the manifest effects obtained, and invited me to his Dispensary where he attends

three times a week, and prescribes gratuitously. He has, he told me, about eighty cases in attendance at present. I saw a large number of these cases, and am satisfied that he does not deceive either himself or others with regard to the nature of his cases. All I saw were cases of phthisis, and most of them either in the second or third stage. He does not pretend to cure such cases generally, but he believes that in most cases, he can arrest the tubercular depositions, and improve the resources of the system, and the result depends upon the nature and extent of the previously existing disease. When the tubercular deposit is circumscribed, the thing may soften down and form a cavity which may heal, or the cavity may remain and contract slowly, without any serious inconvenience. He thinks in some cases tubercle is absorbed. He uses now the hypo-phosphite of soda, which he thinks better than that of lime, and he gives it to an adult, in doses of from five to twenty grains once a day, about the time of the morning meal. He considers that the immediate effect of the remedy is stimulant upon the nervous system, and that it indirectly acts upon nutrition, and increases the blood. As a general rule, he says, patients feel better and stronger very soon after commencing the use of the phosphite, and after some time, more or less plethora is induced. By plethora, he means a relative condition, and not what is generally meant by plethora in a full habit; the lips and tongue lose their anæmic appearance, and become rosy; the cheeks present more color than is usual in such cases, and the face, sometimes, becomes a little puffy, and it is not unfrequent for more or less hæmorrhage to occur either from the nose, bowels, or lungs. When this is the case, he omits the medicine for the time, and sometimes cups or applies a few leeches to the arms. He thinks that a persistence in the medicine under this condition, may favor softening. After the apparent plethora subsides, he resumes the medicine in smaller doses.

I have, of course, had no time to follow out his cases to verify his assertions; but quite a number of cases that I saw, presented the appearance of what he called plethora, and some had had slight hæmorrhages from some mucous surface. He has a gentleman under his treatment whose history I am acquainted with, and whose friends are not likely to be imposed upon, and they told me that the effect of his medicine was precisely what is described above. I examined this gentleman, and found Dr. C.'s diagnosis correct. The physician of the

family was present, and he acknowledged a decided improvement in flesh and color. Dr. C. gives no particular directions as to diet, but advises exercise in the open air, and such wholesome food as they have been in the habit of using. To the poor that he sees at his Dispensary, he gives no special directions as to occupation, leaving these to the unaided influence of the hypo-phosphite. In order the more completely, probably, to test its influence, I remarked to him that if the remedy had the effect of increasing innervation and the blood-making power, it would be very valuable in other but analogous conditions. He replied that this was the case, but he confined its use to tubercular patients for fear it might be said that he was making a panacea of it and impair confidence in it. This is simply what I learned from Dr. Churchill of his method of treating consumption. There is no mystery or quackery in it. It can be easily tested without any hazard to life. He published a book on the subject, but the copies are all gone. He will soon publish another edition, with additions, and has promised to send it to me.

I think this remedy worthy of a fair trial; for, if it has the effect which he sincerely asserts, even in a much less degree, he believes it is very valuable. If a system can be established that will, in a large majority of cases, arrest the tubercular process, it will confer a greater benefit upon the human race, than any discovery since that of the vaccine by Jenner. The pathologic and therapeutic laws of tuberculosis are as well understood at the present time, as those of any other chronic disease, and they are quite within the comprehension of the common educated mind, and if agents can be found that will fulfill the therapeutic indications, there is no reason why the tubercular disease should not be, in most cases, arrested in the onset and before serious lesions are produced. We have, in the improved system of diet, in the use of cod-liver oil, and in the system of active exercise in the open air, which are now generally adopted, established a mode of treatment or management which is quite successful, when adopted before serious mischief is done, and if the hypo-phosphites are found upon trial to have the effect which Dr. Churchill thinks they have, they will be a valuable addition to our present stock of therapeutic agents.

Medical men of distinction are more honored here than with us, and so of scientific men generally. Dr. Trousseau was recently made a

Commander of the Legion of Honor, and M. Robin, the great micrologist, has been recently decorated with the Legion of Honor, and elected to the Academy of Medicine.

WARREN STONE, M. D.

ART. VI.—*The Influence of Honorary Titles and Rewards upon the Progress of Science, and the Public Good:* By BENNET DOWLER, M. D.

PROFESSOR STONE, in his interesting letter, which forms the preceding article, alludes to the fact that European governments encourage men of learning more than our own, instancing several physicians whose contributions to medical science have been lately rewarded by promotions to the Order of the Legion of Honor; hence I feel warranted in, or as good as pardoned for, making a few remarks on this and some kindred topics, which, if not strictly medical, are indirectly so in their import in common with general science, and the means of its extension.

The Legion of Honor, which includes many physicians, is neither a patrician order nor a hereditary rank in the family; it confers neither peculiar privileges nor exclusive civil rights. If not antagonistic to that pseudo-aristocracy which is founded on mere wealth, or "on a little brief authority" which chance bestows at the annual elections, it has not with either any necessary connection.

The organic laws of this Republic wholly interdict nobility. The founders of the government viewed this institution as it existed in the mother country, consisting of Dukes, Marquesses, Earls, Viscounts, and Barons, created at pleasure by the crown, and favored with peculiar rights and privileges denied to others. Thus every Peer of the Realm whether wise or otherwise, may take a seat and legislate in the House of Lords, being free from arrest in civil cases, and amenable for even treasons or felonies, only to that privileged house; his titles and dignities rarely, if ever, given as a reward for scientific merit, are not only perpetual during life, but transmissible to his posterity.

The Legion of Honor is virtually a democratic institution. The

great French Revolution, so fatal to hereditary aristocracy, had scarcely completed its sanguinary orbit, when Napoleon, then First Consul, proposed to establish an Order of Merit in France. Of all his measures this was one of the most beneficient. It encountered at first the severest opposition.

The expediency of creating this Order was debated and to a great extent opposed in the sessions of the Council of State. But finally, when, in 1802, the ballot was taken, the vote stood, yeas 14; nays 10. In the Legislative Body, there were 166 yeas; 110 nays. In the three Chambers of the Government, numbering three hundred and ninety-four, the majority in favor of the measure amounted to no more than seventy-eight.

Before the law passed, at one of the preliminary meetings of the Council, May 8th, 1802, Napoleon said: "I do not think that the French love either liberty or equality. They are what the Gauls were, proud and frivolous. They have but one sentiment—*honor*. It is this sentiment that we must feed: the people must have badges of distinction." On another occasion he said, that "the cross of St. Louis had been a powerful stimulus to the soldiery to perform prodigies of valor. Money was worthless in comparison to it; it was preferred to mountains of gold."

During the debates upon the propriety of establishing the order, Cambacérès maintained that "these distinctive marks had not been proscribed by the republics of antiquity. Regnault de St. Jean furnished a very acceptable argument, in the assertion, that the newly framed government of the United States had just completed its republican institutions by creating the Order of Cincinnatus."

"The law was not put in execution for two years or more owing to the necessity of procuring six million francs for the preliminary expenses. M. De Lacépède, the naturalist, was made the Grand Chancellor of the Order.

"The institution of the Legion of Honor," says an author little friendly to Napoleon, "was one of the most daring, and it must be admitted in view of the character of the French people, and the actual condition of affairs, one of the most fruitful and beneficent acts of Napoleon's reign. In contributing to arrest confusion and consolidate society, to substitute the distinctions of *merit* for the divisions of *caste*, to conciliate faction, and promote effort in various paths of glory, it

deserves to rank above the concordat and on a level with the civil code. Historians whose opinions have won regard, have pronounced it altogether the first, and most productive acheivement of his genius."

The Republicans and Bourbonists, the wits and the students in the University, ridiculed without measure the new Order. But Napoleon declared all would be ambitious in the end to obtain admission into it. Even Monge, Berthier, Thibaudeau, and others who had denounced it, and an host of the most learned, accepted the cross with intense satisfaction, as Lagrange, La Place, Lalande, Bossut, Fourcroy, Haüy, Parmentier, Hallé, Geoffroy St. Hilaire, Cuvier, Anquetil, Danse, De Sacy, and others.

This Order has, I believe, five divisions: *chevaliers* (an unlimited number); *officers; commanders; grand officers*, and *grand crosses*. It is conferred alike on the humblest and highest citizen alike for civil, scientific and military services of a distinguished character generally of twenty-five years' duration. This period applies more particularly to times of peace, as during war a brilliant exploit may be immediately rewarded by admission into the order. Virtually it is conferrable by the Sovereign, at pleasure. Louis Phillippe bestowed it upon a multitude, so that it amounted to 30,747 during his reign. This royal prodigality was caricatured in the Paris *Charivari*, which represents the King as firing upon the mob with an enormous mortar loaded with the crosses of the Legion of Honor. The number of members according to a recent author, is less than seven thousand, being divided into sixteen cohorts, each cohort containing seven grand officers, twenty commanders, thirty officers, and three hundred and fifty chevaliers; the whole Order, therefore, would comprise six thousand six hundred and twelve members.

"Four years after the institution of the Legion, every prejudice against it had disappeared, and all classes of society displayed the utmost eagerness to be admitted to its ranks. No station, no capacity, was above the ambition of holding a position in an Order of Merit so truly national and universal. On his return from Moscow, Napoleon detached his own cross from his breast, and gave it with flattering words to Corvisart, his confidential physician." Louis XVIII decreed the maintenance of the Order, and gave the badge to Arago, Biot, Guizot, Villemain, Cormenin, Didot, Vernet, Chateaubriand, Nodier, Lamartine and others.

It is not here intended to dwell on the beneficial tendencies of this Order of Merit in regard to medical science, nor upon the expediency of establishing a similar organization in the United States, where if any where, the principle of individual merit, genius, originalty, useful scientific research and discovery, should be recognized, encouraged, and honored, not as a family, but as an individual distinction and reward.

With regard to the creation and the beneficient effects of this French Order of Merit, Sir A. Alison, (who is by no means partial to French institutions) says: "This event proved that Napoleon had rightly appreciated the true character of the revolutionary spirit. The leading object in the revolution was the *extinction of castes, not of ranks; equality of rights, and not of classes; the abolition of hereditary, not personal distinction.*"

The marvellous sway which Napoleon exercised over the army, Alison attributes to his habit of seeking out and distinguishing merit and talent amoung the private men or inferior ranks of the army, the distribution of eagles to the regiments, crosses of the Legion of Honor to the most deserving, and the instant promotion of extraordinary merit on the field of battle. The highest offices were open alike to all who distinguished themselves.

In the American Republic all titles of honor not interdicted by the organic law, are as much in use as in any other country. Probably no country has as many generals, colonels, majors, captains, excellencies, esquires, LL. D., M. D., D. D.; one is called his lordship, several his grace; there are legions of honorables, and a multitude of his honor, which is the title of all who can manage the popular or other votes so as to be elected judges. Foreigners reproach Americans who visit monarchical countries, as being more eager than others, to gain admission into the presence of Royalty.

In Imperial France, plain Mr. is thought good enough for almost every one not wearing a crown, while titles of this Order do not confer peculiar privileges, nor create, in the eyes of the law, civil inequality. In the imperial government of Brazil, titles of nobility are not heridetary.

There never descended to the tomb purer patriots, more unselfish men, than Washington, the President, and the army officers of the Revolution, the founders and members of the Order of Cincinnatus.

It was, however, at least, in one point of view, contrariwise to the whole tenor of their lives both in principle and practice, seeing it inaugurated an order as "immutable," which enacted as a fundamental law, that its title and medal shall be hereditary in "the eldest male posterity, and in failure thereof, the collateral branches, who shall have an ORDER by which its members shall be known and distinguished, which shall be a medal of gold, of a proper size to receive the emblems, and be suspended by a deep blue ribbon, two inches wide, edged with white," etc. This anti-republican blunder appeared to friendly crowned heads so evident, that the King of Sweden, Gustavus Adolphus III, would not allow such officers of his as were in the American army to wear the Order of the Cincinnati, a proceeding which Washington criticised. It was, however, a friendly voluntary order, not a legal or chartered institution. Feeble at first, it is probably now dead or dying.

An Order of Merit sacred to distinguished services in science, art and arms, should not only repudiate the hereditary principle, but be secured against the control of mere politicians who might easily prostitute it for election purposes; or, failing in that, denounce it to the people as aristocratic. Such an order by stimulating research and beneficial deeds by honorary rewards, is for the public a positive good, "without money and without price."

In Great Britain, learning and letters, arts and sciences, improvements and discoveries, have seldom, perhaps never, been rewarded with a peerage, or other title except the lowest in the scale of nobility, namely, knighthood. In France, a simple order of individual merit, open to the humblest, costing the treasury nothing (no rich estate to give dignity to the title) has for more than half a century been tried, and has proved to be an object worthy of an honorable ambition, fruitful in promoting scientific research and noble conduct, the practical and utilitarian results of which redound to the public and not directly to the order itself.

That such an organization in a republican government is altogether impracticable, is a foregone conclusion neither tried nor proven; if it be practicable, it might to some extent act as a counterpoise to the undue influences which the demagogue and the man of mere wealth exercise in society.

In modern times, the Executive Government of the American Re-

public, seems practically to distrust and repel men of literary character and scientific attainment. It was not so in the days of Franklin and Jefferson. A statesman is a politician; a politician is not generally a statesman. Among literary men as well as others, there are doubtlessly some whose want of practical knowledge and administrative ability, should exclude them from responsible places and offices in the government. Napoleon, who, in many of his Bulletins, placed his title as Member of the Institute before his title of General, had little love for merely speculative authors; but Bourriene explains this apparent dislike thus: "It was less the effect of prejudice than a necessity of his character. Time is required merely to read, and certainly to appreciate a literary work, and to Napoleon, time was so precious, that he would have gladly shortened a straight line."

ART. VII.—*Masked and Traumatic Yellow Fever; a case by* MR. THO. H. CASTLETON; *two cases by* B. DOWLER, M. D.

1858. Charity Hospital, June 22d. Henry Rixe, patient; Dr. Dirmyer, Visiting Surgeon; Tho. H. Castleton, Reporter. Henry Rixe native of Prussia, age 56 years, laborer; arrived in this city directly from France. He had been in the city three weeks before admitted to the Hospital, June 22d. He was received for erysipelatous inflammation of the right hand, caused from a severe bruise while working on ship.

He was a strong and muscular man for one of his age; complexion was quite fair and rosy; enjoying perfect health with the exception of the above named disease.

He remained in the ward under treatment for erysipelatous inflammation, and was improving as fast as could be expected under the circumstances; when on the morning of the 29th July, while paying the usual visit, he complained of a slight headache, and nothing else; had no fever; skin cool and moist; pulse natural, soft and pleasant, pulsating about fifty-eight per minute. I gave him a mild purgative and cooling drink. I saw him again at noon; he expressed himself much better; no headache; no fever, etc. About 3 o'clock, P. M. of

the same day the nurse of the ward came to me, and informed me that said patient was worse. I immediately went to see him. I found him with high fever, headache; pain in the back, and a little delirium, and also some pain about the epigastric region; tongue quite natural; pulse somewhat quickened, strong and full. His bowels had been well moved by the purgative given in the morning. I ordered him a mustard foot bath; orange leaf tea; also Haustis quiniæ of the House, to be given as soon as perspiration was well established. The fever lasted from six to eight hours.

The Haustis quiniæ acted like a charm; as soon as the fever left him, he went to sleep, and rested well all night. Patient was much better in the morning, entirely free from fever, no headache, no sickness at the stomach, no pain; tongue was not at all coated, but natural; pulse quite pleasant; no redness of the gums, but on the contrary they were quite pale and anæmic; noticed a little delirium or unsteadiness about the man, but supposed it was caused from the quiniæ. Nothing was given him this morning, but lumps of ice. He remained about the same until 4, P. M., when I noticed, while passing through the ward, that a change had taken place. I found him with fever, but not so high as the day before; complained of no pain in any part of the body; stomach quiet, etc. I gave him a mustard foot bath; ice in lumps; liq. ammoniæ acetatis ℥i; tablespoonful every two hours. The fever soon left him, with no bad symptoms. I saw said patient again that night at 9 o'clock; found him sleeping; I had then great hopes of finding him well in the morning; but, to my great astonishment, I found him dead.

I followed him to the dead house, where a post mortem was made. We found nothing to account for the sudden death, when it was suggested by the Visiting Surgeon, to examine the stomach, where we might possibly find something that would clear up the obscurity which had enveloped this case. When the stomach was opened; it was found to be filled with black vomit. This fact removed all doubt or suspicion and obscurity connected with this case.

Yours, respectfully,

THO. H. CASTLETON, Res. Student.

DR. DIRMYER.

Black vomit in a consumptive; masked yellow fever. 1841. Oct. 24th. A man dead a few hours from consumption; body in an extraordinary

state of general emaciation; the left lung almost entirely infiltrated with tuberculous matter, where not totally disorganized by extensive excavations; tubercles in the right lung; the mucous tissue of the stomach and bowels softened, the stomach which was greatly attenuated, contained black vomit. A distinguished physician, the late Dr. Luzenberg who assisted at the autopsy, maintained that this man died of yellow fever; a severe epidemic of which was then subsiding.

Traumatic Yellow Fever. 1842. Sept. 20th–Oct. 20th. V. S., born in Germany, aged 26; was bled from the arm about the 15th of Sept.; on the 20th, having entered the hospital, with his arm greatly swollen and sloughing, (phlebitis); great debility; tongue dry, and red, etc.; was treated with camphor, morphia, etc. under which he evidently improved; the enormous tumefaction of the arm disappeared, but the limb was stiff; the patient much emaciated; skin yellow. On the 5th of October, the skin and eyes were intensely yellow; on the 7th the skin was rather hot; pulse jerking, but soft, breathing uneasy; headache; 13th; skin warm, intensely yellow; pulse corded; black vomit, (as the nurse affirms); 20th, died.

I believe my MS. notes contain other examples of what I venture to call Traumatic Yellow Fever. The reader is referred to the first volume of Baron Larrey's Military Memoirs, from which it appears (*anno*, 1800) that out of six hundred troops wounded at the seige of Cairo and the taking of Boulaq, (Egypt) *two hundred and sixty died of yellow fever.* He expressly says that "*it attacked none but the wounded!*" He gives a good description of the disease, its febrile symptoms, including yellowness, hæmorrhages, suppression of urine, etc. The French soldiers, at first attributed the fever to poisoned balls discharged by their enemies, but were soon undecieved by the irruption of the yellow fever in its malignant form, killing about three in every seven of the wounded! Larrey does not say what ratio the mortality bore to the number of cases; although none but the wounded contracted the disease, he does not say whether all of these took the disease; but in any case, it must be admitted that here is a traumatic yellow fever endemic which the clerk of the Charity Hospital of New Orleans (who gives figures honestly) will call severe.

Yellow Fever maskd with apparent Death. In the Medical Museum*

* This Journal was edited by the learned and venerable Prof. Coxe, of Philadelphia, whose scholarship is equalled by very few medical men of this or any country.

for 1805, Dr. John Rush of Philadelphia, reports a most extraordinary case of resuscitation from apparent death in a yellow fever patient. The account is given with circumstantiality six years after its alleged occurrence. It claims to have occurred in the presence of many yellow fever patients among the crews of two government vessels (in which the disease was prevalent) not to mention the citizens of the village, Marcus Hook, on the banks of the Delaware river.

There appears no rational motive for deception on the part of the narrator. The account was published while the parties that must have witnessed it were still alive. It has not been contradicted. Still, however, the statement is not free from improbability. It is improbable that a recovery will take place after black vomit which this patient had; or rather, it should be said that recovery from black vomit *was once* deemed almost like recovery from apparent death. It is not so now. Numbers now recover after that phenomenon; while writing this page, Oct. 8th, 1858, I have just paid the last visit to a patient convalescent from black vomit.

It is somewhat marvellous that after an apparent death of four hours' duration, the patient should for two hours more swallow brandy in large doses without breathing, more especially as the narrator declares that nothing like a fit or convulsion occurred.

The following is a summary of this case: James Clark, aged 19, seaman in the Government ship Ganges, with many others, was seized with yellow fever in its "malignant form," Sept. 7th, 1798. He was treated for three days with "strong doses" of calomel, brandy, ether, laudanum, mercurial inunctions, and was bled twenty-four ounces.

On the morning of the fourth day he began to throw up black vomit, and, at noon, expired: "upon paying my second visit to the tents at 4, P. M.," says Dr. Rush, "I saw the body of Clark lying in a coffin apparently lifeless. On closely examining it, I observed the pale yellow [of the skin] had changed to an orange, interspersed with purplish spots: neither pulse nor motion of the heart were perceptible; nor was respiration discoverable on the mirror which was held before the mouth. Upon a more minute examination, I felt (or thought I felt,) a slight warmth about the epigastric region,"—whereupon the doctor directed warm ashes to be poured into the coffin and a gill of very strong brandy toddy to be given every half hour. A quart of

brandy had been taken by 8, P. M. At 11, P. M. the patient complained of the hot ashes; he was uncoffined; wine sangaree was substituted until day light, when he refused to take any more, and called for food; at day light the doctor* found him propped up in bed taking soup.

ART. VIII.—*Speculative and Practical Researches on the supposed Duality, Unity and Antagonism of Nature and art in the cure of Diseases:* By BENNET DOWLER, M. D.

Nature is the existence of things so far as is determined according to general laws, or it is the first internal principle or foundation of everything which belongs to existence or the effectivity of a thing. The existence of things in themselves we cannot cognize.—KANT.

Two works* whose fundamental doctrines are subversive of what always was, now is, and ever will be, denominated practical medicine by the great majority of the medical faculty, having been recently republished and highly commended in this country, duty, if not inclination, requires that those doctrines so pregnant with weal or woe to the well-being of society, should be scrutinized and fearlessly adopted or utterly condemned according to their merits. Let justice be done though the heavens and the doctors fall together.

When Voltaire for the last time visited the theatre, an admiring auditory almost suffocated him with complimentary bouquets. Journalistic bouquets or laudations have been heaped too nearly the smothering point upon the two authors alluded to, boding no good to the faithful *in Æsculapio.*

There are higher aims, nobler ends, than the short-lived acclamamations of critics and the notoriety gained by attacking self-evident truths and adopting a bigotted skepticism. In setting aside the landmarks of the past and the existing experience of the great body of medical men, there is a startling boldness, a brilliant novelty herein, which rivals the philosophical school of Berkeley and Hume, which taught that there was not a particle of evidence to show the existence

* John Rush's name is in the Catalogue of Graduates of the University of Pennsylvania, for 1804, having written a thesis "on the causes of sudden death, and the means of preventing it."

* Professor Bennett's "Clinical Lectures," 1858, and Sir John Forbes' work "on Nature and Art in the cure of Disease," 1858; both published by the Messrs. Wood: New York. See catalogues in this Journal.

of matter. Byron remarks of the good bishop who adopted this dogma, that

"When Berkeley said there was no matter, it was no matter what he said:"

In speculative science there can be little harm in such a fancy, but it is far different in practical medicine, when the question stands out at the bedside in the most salient form, help! help! or I perish? "Is there no balm in Gilead? Is there no physician there? Why then," etc.

Not to cure a disease, if it be curable by art, is not a whit better than mal-practice and is not justifiable by any rational theory whatever.

A journalist, how much so ever he may be aware of his liability to form erroneous opinions and prejudiced judgments, is not only justified but sometimes required to dissent from or oppose principles and practices, which, though sanctioned by great names, appear to him dangerous to the well-being of society. Influenced by no unworthy motive, impelled by a sense of duty rather than by choice, he may hope that generous minds will excuse and forgive well meant efforts even though they may not be free from imperfection and mistakes. Here indifferentism is more blamable than dogmatism. Silent acquiescence in that which is deemed not only erroneous in theory but highly detrimental in practice, is scarcely consistent with allegiance to truth and the public good. Truth, however, is great and will prevail "and a true verdict give" in the face of undue laudation and unjust criticism. Errors are comparatively harmless when they originate with the worthless and ignorant—a class to which Professor Bennett and Sir John Forbes are related only as antipodes.

Professor Bennett, as will be seen in this paper, not only opposes antiphlogistic treatment, (as blood-letting) even in the most acute disease, but affirms such treatment only tends to prolong the malady or prevent recovery. Sir John Forbes' stand-point is, to use his own words, that "at least *all active remedies* must be abondoned;" in plain English *all* remedies must be abandoned for what remedy so ever is *inactive* is not a remedy.

How have these doctrines been received in Great Britain? For, notwithstanding the Declaration of Indcpendence (July 4, 1776), it is probable that the acquiescence in these doctrines abroad will be followed with a like reception at home. Such is already the case.

In the face of such a system which virtually proclaims the existing medical profession worse than useless, expediency, blamable though it be, would enjoin silence. But instead of this, some are half or altogether willing to receive these views as quite orthodox: judging by the medical press, the latter class would seem predominent. In the Glasgow Medical Journal, (July, 1858), Dr. J. A. Marston says: "Professor Bennett's papers are an admirable expression of the general and dominant feeling among the profession upon the employment of venesection as a remedial measure. A weak point exists in his definition of inflammation; for, although it may be correct to say that there is a certain abnormal condition of the blood, blood-vessels, and nerves of a part inflamed, it is *not proven* that exudation must also be present to constitute inflammation." The editors of *Ranking's Abstract*, (July, 1858), write thus: "We do not hesitate to say that our sympathies, both in pathology and practice are with Dr. Bennett."

Professor Bennett, as an original investigator, has done good service in the cause of science; and Sir John Forbes, as well as the former, has proven himself to be an able writer, consequently their recent works above alluded to, might be expected to possess, independently of their intrinsic merits, strong claims to acceptance in advance. If teachers, writers, and journalists consider these representations of existing pathology and therapeutics just, they should have the manliness to adopt and reccommend them, so that their pupils and readers may at once know how the land lies, and how they should steer their course so as to avoid stranding upon the shoals and rocks of old Physic. On the other hand those who either distrust these doctrines as *not proven*, or think them false and injurious, should endeavor to prevent them from rising into public credit and influence.

Molière long ago said, there was not a more amusing piece of mummery extant, than that of one man attempting to cure another. It has been a standing jest from time immemorial, that the art of physic consists in amusing the patient until Nature cures the disease. These pleasantries are, however, solemn axioms underlying the therapy of Drs. Bennett and Forbes. Indeed, the works of both might as well have been entitled what the latter is, "Nature and Art in the Cure of Disease."

Nature, however, is a term more equivocal, being applied to more subjects and objects, and in a greater number of senses, than any

other. Its name is legion. It is the parent of a numerous progeny of fallacies, inaccuracies, inconclusive ratiocinations, and scientific controversies.

Nature in disease is a metaphorical or imaginary personality, poetically endowed with knowledge, volition, and superior power. The poets have taken a still wider range than pathologists, seeing that they metamorphose Nature to suit every phase of the imagination, including mythological gods and goddesses, and fixed Fate ; a theory contrary to Pope's, who says of Nature's God :

"He binding *Nature fast in fate*,
Left free the human will."

"But," says Bacon, "man is the minister and interpreter of Nature." True. Being her minister and interpreter, while she is an unintelligent necessity, he often puts her in the right way, and controls her afterward, as the sequel may show more fully.

The presumed antagonism between either physiological or pathological Nature and Art, is entirely erroneous both in theory and practice. Suppose Sir John Forbes were to write a treatise on Nature and Art in Astronomy; or, on Nature and Art in Moral Philosophy; or, on Nature and Art in Government ; or, on Nature and Art in Geology ; or, on Nature and Art in Chemistry, or on Mechanics, and Agriculture, etc., could he show that these factors must be antagonistic, or that the interference of Art, or, to use the accepted tautological epithet, "*active* interference," is, and must be, not only useless, but positively detrimental ?

In one point of view Nature and Art are dual,-two in one and one in two. The inherent tendency of the animal economy and the drugging process of Art, may coincide and mutually agree in their common result, the restoration of health ; in another point of view, Nature is disease, decline, death. When Nature gives one the ague, the doctor fights her with quinine, and generally conquers by *active* treatment ; but Nature at last takes off both doctor and patient, by old age, or otherwise.

The brilliant phrase that pathology is deranged physiology, is metaphorical, and cannot in any case be literally applied. In so far as disease exists, it is the antithesis of health, and *vice versa*. The expression is, in one point of view, as happy as it is suggestive, reminding the pathologist that he should be a physiologist, and that he should

know the laws of both, in order to know departures from either. Without affirming the supremacy of the *lex parsimoniæ*, or that Nature in Disease or in Health always attains her ends by the shortest and most direct means, it may be assumed, indeed it is an axiom in philosophy, that the same causes produce the same effects, and that no more causes than are necessary to account for the effects, must be admitted. Hence, if the laws of physiology tend to the preservation of health and well-being, there can be no need to call in Diseased Nature, which is altogether antagonistic, much less to endow disease with sanative functions, positively contradictory to the essential, inherent character of disease itself. If disease be qualitative, the whole of its existence consists in its perpetual antithesis to, and secession from, pure physiology or the health-status. Take away all of its qualities, and disease is not. The therapy of what is not, is nothing.

Experience teaches that often unintelligible words and sophisms are apt to mislead both the professional and public mind. Some diseases are curable by neither the "autocracy of Nature" nor the druggings of Art. The dualism of Nature is a fundamental study. Her attributes of morbidity must be opposed; her health-giving attributes invoked and honored.

The theory which claims for Nature an exclusive jurisdiction as surgeon and physician, implies, indeed, or virtually teaches, that the medical man is not only a squatter, but a tresspasser without color of title, and should be ejected from her premises forthwith, unless he be contented to be a merely passive observer, which he must be, if active medication be interdicted. This theory is calculated to discourage the cultivators of science, while it encourages laziness, ignorance, deception, and the neglect of the sick; as the latter are, according to this theory, already in better hands than man's. Why, then, should any one study or toil to acquire a knowledge of the principles and the practice of medicine, agriculture, trades, or arts, if Nature will do all in the best manner gratuitously?

The postulate gravely put by Sir J. Forbes, (and Prof. Bennett also,) that "at least all *active remedies* are to be abandoned," virtually interdicts all treatment whatever. A medicinal agent without action is at once the maximum of absurdity. Antiphlogistics, or stimulants, may be right, or they may be wrong, but they can be neither unless active. For what thing soever is not active, is wholly inert, and therefore not

medicinal. The proposition, freed from all pedantry, fallacy, and disguise, is that medication should be interdicted. With this proposition, polypharmacy and the exudation theory have no necessary connection. The most ultra phlebotomist, or heroic prescriber of drugs, never denied that blood-letting or doses may not be too actively and largely employed. Diet may be given in excess, but if it be not active, it is no diet at all for the individual.

The fundamental argument of Drs. Bennett and Forbes may be thus stated: Nature in Disease, or Diseased Nature, cures Disease; the healing Art, as now practiced, is antagonistic or contrary to Nature, and, therefore, this Art is not only not curative, but positively injurious or fatal. This fallacy is repeated very often, and in various forms, but with little or no reliable data from which indisputable conclusions favoroble to the authors' theory, can be legitimately deduced.

In *The Glasgow Medical Journal*, (July, 1858,) Dr. J. A. Marston, an admirer of Dr. Bennett, says, in a paper on Fever, that "a non-professional man, reading the various papers which have appeared relative to the blood-letting controversy, however high an opinion he might form of the abilities and argumentative qualities possessed by the faculty, would certainly reverse the common axiom, and say that in a multitude of counselors there is distraction. He would perceive, at least, that bleeding was out of date with all, but the why and wherefore would puzzle him. Nothing is more true, than that anything may be proved by statistics. To watch, rather than to count, is a philosophical expression of what, I take it, is the general feeling upon this head. Pneumonia is particularly ill-chosen, from the difficulties attending it, for drawing any accurate conclusions from statistics of this disease. Generally, it differs widely, as seen in public hospitals, from the same disease witnessed among other classes, as the agricultural." * * * * * * * *

Some years ago, Dr. Forbes, in the *British and Foreign Medical Review*, of which he was the able Editor, announced that physicians were wofully incompetent in reasoning powers, and that he intended to write on medical logic, which he did, under the facetious title of "YOUNG PHYSIC"—a treacherous performance, which killed his Journal, and illustrated the charge he brought against others, that is, incompetency in reasoning. This attack, however, did not kill Old Physic. Hence he has returned to the charge in the year 1857.

In the July No., 1845, of the Journal mentioned, he says, "that taken as a class medical men are not distinguished for their qualifications for correct observation," etc.; and that he "intends ere long to give the public a work on Observation and Logic in Medicine," which is at length given, *anno*, 1857, under a different title, "*Nature in Disease.*" "Taken as a class," if "medical men" can in this book discover "correct observation and logic" as highly characterized; they are more fortunate than the writer of these lines.

In the number for January, 1846, which contains an elaborate paper on Homœopathy, the learned gentleman thus sums up the charges in his indictment against the regulars: "That in a considerable proportion of diseases, it would fare as well or better with patients, in the actual condition of the medical art, as more generally practised, if all remedies, at least *all active remedies, especially drugs*, were abandoned. No systematic or theoretical classification of diseases, or of therapeutic agents ever yet promulgated, is true, or anything like the truth; none can be adopted as a safe guide in practice." In a part, see the whole, in its prelude, the book of 1857.

In his late work Sir John, says, of "the indirect method, that it completely recognizes the autocracy of nature; it is not only useless, but injurious to attempt to suppress morbid processes by strong measures," etc.

The indirect method which completely recognizes the autocracy of Nature in curing Disease, swept off by small pox one thirteenth of the human race and generally disfigured the residue during many long centuries until of late, when Jenner, rebelling against this "complete Autocrat," robbed Her Majesty of the right divine to govern wrong, thereby freeing millions every year from the deadly grasp of autocrat aforesaid. Anæsthetics have almost abolished pain hitherto imposed upon man and beast as inseperable from certain necessary processes for relief from disease. This autocrat had from the morning of the creation fired her thunderbolts into honest peoples' houses and bodies, until Franklin disarmed and controlled her with a rod of iron.

If Nature be a complete Autocrat and have a will of her own, Art often defeats her purposes. If nature makes a hare-lip, art closes it —a blinding cataract, art restores vision—a deformed pelvis, art

makes the cæsarian section or the dissection of the child—a wrong presentation in child birth, art rectifies the mal-position, and so on.

"The drugging practice!" "the drugging system!" "the drugging doctors!" These potent and reproachful terms now much in vogue, are neither witty nor wise, inasmuch as all medicines are drugs. Those not contented with the drugs known, and those who want better bread than can be made of wheat, buck-wheat, corn, and rye, should discover substitutes having better claims to the right of way through the Œsophagean Istbmus. It is not the drugging but the unskillful drugging of a patient which should be condemned.

Whether full living and stimulation in inflammatory diseases, and "the feeding of fevers" are prescriptions less applicable to the American populatien than any other people let others determine. Cheap whisky, and an abundance of bread, pork, beef, and the like might be supposed to create a phlogistic diathesis, admitting antiphlogistics in a population whose diseases are remarkably acute as compared with the predominating, prolonged, and adynamic epidemics of European typhoid, typhus, etc. How speculative so ever these suggestions be, there can be little question, if antiphlogistics be, in a certain class of maladies, curative, that from the extreme difficulty of justly appreciating the therapeutic import of remedial measures, strong popular biases against them, must greatly impede or thwart the physician's plan of cure, if opposed to popular opinion, especially in regard to blood-letting, an open procedure, in which there can be no concealment as in writing a prescription which may contain the very medicine, which, if known, the patient would sometimes refuse to take.

Admitting in certain acute inflammatory fevers and local phlegmasias, that stimulants are hurtful and antiphlogistics useful, and supposing the public mind to be neutral in regard to these plans of treatment, it will be easier to convince these neutrals that stimulants and tonics are to be preferred, that the patient being already weak, should be strengthened, not weakened by blood-letting, purging, etc., and, moreover, if this logic, however pernicious, can be fortified by the authority of great names, new books and illustrated editions, the conviction is apt to be complete, the conclusion indisputable. The history of medicine abounds with popular delusions which have been transmitted from generation to generation in defiance of the clearest refutations of science. To sanction and propogate the unproven

dogma that in no disease, however inflammatory, can blood-letting act otherwise than by prolonging or aggravating the malady, is a very grave question in a moral or medical point of view. Should it after all prove to be a false theory (it certainly is not a proven one) its victims must fall daily in hecatombs.

If it be admitted that of late diseases have fallen from their former high sthenic state to a low asthenic one, no one can say that the former diathesis will not be speedily rehabilitated, but, in this eventuality the former systematic venesections carried to swooning, *coup sur coup, à la Bouillaud,* will scarcely be renewed, because there is a deep conviction that they never should have been practised as a general or routine treatment. If the total abandonment of blood-letting by the present generation should take place, as some imagine and wish, owing to the alleged debility of the human constitution, it is probable that the next generation will attribute this conduct more to a debility in the logic than to a debility in the human economy of their predecessors. For, admitting that diseases, especially epidemics, are less inflammatory now than they were a quarter or half century ago, some cases may and do form exceptions, in which antiphlogistic treatment is still the best; nor is there any reason to think that it ever was, is now, or will be, otherwise.

It is but a few years since some of the most eminent surgeons upon the eve of great operations attempted to anticipate the inflammation incidental to such operations, by venesections. Thus M. Lisfranc, in his *Précis de Médecine Opératoire** (Paris, 1845–7) recommends blood-lettings before surgical operations in all cases in which the operation itself from its nature will be attended with but slight hæmorrhages only, affirming that in plethoric cases one or two venesections should be always practised.

The excessive, progressive, and aggressive system of swooning blood-lettings in yellow fever was based on the assumed pathology of the disease, namely, solidism, a local inflammation of the stomach and bowels. A contrary, though as yet unproven hypothesis is by far more prevalent, that is to say, yellow fever is considered a humoralistic disease, or a disease of the blood, and, hence, a general disease.

* T. I. 33–4. This great surgeon, who, if he loved his *confreres* little, seems to have been loved less by them, died before the third volume of his valuable work on surgery had been printed. Its practical character and able criticism entitle it to an English dress notwithstanding the exuberance of surgical literature extant.

This, however, is a question of greater difficulty than appears just now to be conceded in the prevalent system of humoralism. For, were it proven that the humors are changed in all of our maladies, the question arises, did not these changes originate in the solids? Is not the morbidity of the humors the effect of a decrease or increase of fibrine, lymph, etc., antecedent in the solids themselves? Exudation is no more the primary condition of inflammation than suppuration, ulceration, or gangrene.

It is not easy to see how the microscope can be as yet a criterion for either antiphlogistic or stimulant medication. The microscopic examination of a fluid or a solid, presents a certain appearance; but the question arises, can therapy be deduced from this appearance?

Have the micrologists ever written a single prescription for the cure of yellow fever founded upon a constant, peculiar alteration ascertained by the microscope in the fluids or solids of the yellow fever patient? Has the microscope aided in even the diagnosis of this disease, or in showing the differentiations between the blood of those sick of this fever and those who are in perfect health? Is not the physician thrown back upon experience, just as much as if no microscopic appearances had been noticed in the premises?

Dr. Bennett's fundamental criterion of inflammation is *exudation.* This latter is, perhaps, in every instance the effect of recognizable antecedent disease, not the primary, but the secondary, tertiary, or even ultimate termination of a series of morbid phenomena. Will Dr. Bennett affirm that a semi-membranous exudation is the incipient symptom of croup, or that such an exudation is desiderated, as curative, or that no effort should be made to prevent, arrest or expel it by antiphlogistics or other active measures? Does he suppose that this exudation must never be interfered with being really curative, notwithstanding the impending danger of suffocation? Experience, or as he calls it, "blind experience," does not warrant the conclusion that antiphlogistics by arresting the exudation either prolong the disease or enhance its dangers to life.

Some of the Bennett antiphlogistic party, appeal to rheumatism as the type of all that is inflammatory, and as a sure test of the perniciousness of blood-letting. The fallacy of this mode of reasoning is not difficult to discover. Blood-letting, however useful it may be in some diseases never has been assumed to be a panacea for every

malady of an inflammatory nature. Rheumatism, however acute, is precisely one of the diseases in which blood-letting is least curative, while on the other hand, the asthenic variety is seldom cured by tonics, stimulants, or the salts of potash. If not the most dangerous, it is one of the most persistent and uncontrollable types of inflammation, be the treatment what it may. Here is an example of dualism in Nature. The physiological force and morbidity fight with each other, while Art is often little more than a spectator. In many cases it runs on for a long time, and is neither a good test for antiphlogistic nor stimulant treatment. Yet even in this disease medication is of great value in mitigating symptoms, as pain, sleeplessness, etc. Were cancer proved to be sthenic, or asthenic, it does not follow that either antiphlogistics or stimulants would cure it.

As a therapeutic *experimentum crucis* or test, croup is preferable to pneumonia, because it is more readily diagnosticated by its general symptoms, by the expulsion of inflammatory exudations, and by the ocular demonstrations of dense or membraneiform exudatious expelled from the trachea which are seen lining its mucous membrane in cases wherein tracheotomy is performed to prevent immediate suffocation; an operation which has sometimes been successful in saving life. These exudations are, however, secondary, never primary elements of inflammation. Ophthalmic inflammation is, in many of its varieties, as in conjunctivitis, cornitis, iritis, etc., a better test than pneumonia, because its phenomena are open to ocular inspection during its progress as are its destructive terminations, in exudations, opacities, ulcerations, etc., while on the other hand the effects of antiphlogistic treatment are as it were directly subject to ocular examination, without a veil between the doctor's and the patient's eyes. Who has ever practised antiphlogistics and never saw the diminished redness, diminished tumefaction, diminished vascularity, diminished turgencence, diminished heat, diminished exudation, etc., in ophthalmias?

Should the learned masters of the Healing Art succeed in convincing the Faculty not only that antiphlogistics, but "all active medication," that is, all medication whatsoever, is injurious in pneumonia and in every other malady, will not the people fall back upon these active remedies, and the more so because the people are appealed to in the recent attempts to show the utter uselessness, not to say mischievousness of the medical profession? Sir John Forbes says, his book "is

calculated to convey to educated and well instructed people of all classes such information respecting the nature of disease and the true characters and powers of the medical art, as they would be capable of comprehending and appreciating." What is the sublime truth which he wishes to popularize, which the people, however, never will believe? It is that all medication, "at least all active medication must be abandoned!"

If the doctors abandon bleeding, purging, puking, sweating, stimulating and the like, the people will not. If after all it be not proven that these remedies are always injurious; if it be proven that in the most skillful hands they are curative, what would be the consequences, if Sir John should succeed in his utopian crusade against "all active medication?" A revolution will have taken place which its aberrant originators, who shall return to the true faith and practice will find too strong for them to control. The evolution and termination of a revolution generally extend far beyond the limits which its projectors contemplated. It will then be found difficult to reinaugurate remedial measures, however useful, when popular opinion shall be fixed against them.

But they have adduced no reliable evidence to prove that medicinal agents skilfully directed, whether against itch or intermittent, cholera or syphilis, fevers or inflammations, should be abandoned as being not only not curative but mischeivous; they have not proven that disease left to its natural course, uncontrolled by artificial treatment, is best for the sick, nor that this natural method of cure is the supreme result at which true medical science has arrived, after the experience of several thousand years.

Sir John Forbes, does not, however, write for the people exclusively. Although he promises to republish "Young Physic," he says that his work on "Nature and Art in the cure of Disease," which, after fifty years' practice he has just published, will be his last: "I cannot help being impressed with the feeling of solemnity which naturally accompanies any act that is to be the last of its kind. And in this mood I would fain regard the present work in the light of *A Legacy to my younger brethren.*"

While Sir John utterly rejects the principles of homœopathy, he is virtually the purest homœopathist, a better one than can now, perhaps, be found. Pure homœopathy is to all intents and purposes non-

medication, which is precisely his theory. When, in 1845, he wrote his work entitled "Homœopathy, Allopathy, and Young Physic," he based his conclusions on homœopathic statistics which gave, by far, the highest ratio of cures. Hence he concluded that inasmuch as the infinitesimal dilutions and attenuations could have no manner of curative power, Nature cured. Hence all active medication is worse than useless. Were the premises true, that is to say, the statistics and the infinitesimal treatment true, his conclusions must be admitted. But the statistics are false. The homœopathists are false homœopathists. These propositions could be supported by reliable evidence for many years extant, did time and space permit.

It may be sufficient to allude to the evidence furnished in the month of October, *anno* 1858, contained in *The American Homœopathic Review*, edited by R. G. Perkins, M. D., and H. M. Smith, New York. These truly orthodox editors say "they are in favor of *high attenuations*." They call this Review "an organ of the Homœopathic school;" and publish the Official Proceedings of the Fifteenth Annual Meeting of the American Convention or Institute of Homœopathy, which opened at New York, June 2d, 1858, with a large number of Delegates from many States of this Republic. On the second day of the session a scientific discussion, apparently the only one that occurred, took place on the treatment of intermittent fever.

Dr. Hempel maintained that this disease, in North-Western Michigan, "could be cured only by *Arsenicum, in doses of one-tenth or one-fifth of a grain, to be repeated every two or three hours.* In the course of the disease, after giving *arsenicum*, he sometimes observed that œdema of the lower extremities would make its appearance instead of the expected paroxysm. He regarded this as simply a feature of the disease, and not a medicinal aggravation; and this opinion was confirmed by the fact that the œdema yielded to a few more doses of the same remedy.

"Dr. Donovan agreed with the gentlemen who preceded him, that *quinine* could not be relied on as curative in the treatment of intermittent fevers. He thought homœopathists committed a very great mistake in going back to this treatment. He admitted that quinine given in *sufficiently massive doses* would usually break the chill.

"Dr. Watson said that he had heard the remarks of the gentleman who had preceded him, (Dr. Donovan) with the greatest pleasure, from the fact that his own experience in the treatment of this disease

had been entirely at variance with that just related. When he first commenced practice, he had used the high attenuations, but they entirely failed him. The only remedies on which he now relied in the treatment of Fever-and-Ague were *Arsenicum, Carbo vegetabilis, Cedron, Ipecacuanha, Nux Vomica, Quinine, Nitric acid, Sabadilla, Tartar-emetic* and *Veratrium.* He administered *Belladonna, Opium,* or other remedies, whenever they seemed indicated as intercurrent remedies for any complications of the disease.

"Dr. Ward had some experience, and had often failed in these cases. He, at one time, used *quinine,* but from failures, and disastrous consequences arising therefrom, he had abandoned its use altogether. He now used *arsenicum* with the best effects—it is now his main remedy. But in some cases he found unequivocal advantage in the use of *natrum muriaticum.*

"Dr. Clary had cured many cases with the remedies generally employed in homœopathic practice; but in other cases the results were unsatisfatory. It seemed to him that we need a wider range for the selection of remedies.

"Dr. McManus said the matter of the *attenuation* of the remedy was not in his opinion one of much *importance.*"

These extracts represent the *animus* of the whole body with some slight dissents as to the rejection of high attenuations. This being the only scientific discussion reported, it may be assumed that it is the official exposition of the faith and practice of American Homœopathy.

Here are "massive doses" truly! The *fifth of a grain of arsenic every two or three hours," four or five grains per diem!* The standard dose with the regular faculty is the twelfth of a grain.

One of these doctors says high *attenuations entirely fail;* another that they are of *no importance,* while a true homœopathist ought to maintain that the efficacy or potency of a remedy is augmented in an infinite progression by an infinite subdivision; the decillionth part of a grain of charcoal is an authorized dose, but may be augmented in potency by dividing it *ad infinitum!* Dr. H., is, in one respect, a consistent homœopathist, for he cures the *œdema arsenicalis,* by giving more arsenic.* "*Massive* doses of quinine break chills!"

* Some years before his death, the late Dr. Hester (who was officially connected with the Board of Examiners for granting licenses to practise in the Eastern District of Louisiana) informed me that an educated homœopathist, having applied for license, was asked upon his examination, how he would treat a patient suffering from arsenical poisoning? The homœopathist replied, "I would give him more arsenic." License was refused. Dr. H. had subsequently an interview with the gen-

These are they who rail at the heroic doses! A decieved public stand by clapping their hands and huzzaing for homœopathy and doses infinitely small!

This same Dr. Hempel, who is a voluminous Homœopathic author, the Hahnemann of America, six or seven years ago used and recommended the thirtieth attenuation. The mathematical calculation for the fifteenth attenuation was attempted by Prof. Simpson,* of Edinburgh, assisted by others. They found that sixty-one worlds of sugar or water as large as the earth, would be required to dilute, even that low attenuation so as to get Dr. Hempel's dose duly prepared, divided and potentized, starting with one grain or drop. All the planets and all the space within their orbits, if filled with water, would go but a little way towards dilution so as to get this, "*the best dose*," that is the thirtieth attenuation of a grain or drop of medicine, which requires a mass 140,000,000,000,000 times as large as the whole planetary system. Now (*anno*, 1858), Dr. Hempel gives as "the best dose" in intermittent, the *fifth of a grain of arsenic*, every two or three hours, at the rate of four or five grains per day, which would probably kill most persons beyond the limits of Michigan, as two or three grains have been known to prove fatal, in other latitudes. That such follies involving the health and lives of millions should be accepted as truths, "must give us pause."

In alluding to these topics, a desire to engage in polemics has no influence. The public good, the cause of truth, the honor and usefulness of legitimate medicine, are motives altogether sufficient for the occasion.

The editors of *The Homœopathic Review*, in an article on the "Yellow Fever at Staten Island," New York, (1858,) give the homœopathic treatment of that disease, thus: "*Arsenicum*." In the last stage, "*Arsenicum, Lachesis, Nitrate of Silver*, and *Sulphuric Acid*"—nothing more, nothing less.

The editors of the Review, in their Introductory announce the following postulate, which may throw some light upon the narrow limits

tleman while crossing the lake, and inquired of him privately whether he still really believed in the propriety of the treatment aforesaid, and was answered affirmatively. Herein he was perfectly consistent with the fundamental laws of Homœopathy. The introductory article of the Review above quoted, (Oct. 1858) announces "that the doctrine of *similia similibus curantur* is a common ground upon which all can meet. The editors of the Review consider the homœopathic law unexceptionable, and look upon it as the foundation of all science in medication. The uncertainty of experiment attaching to the therapeutics of other schools is nearly absent under this dogma."

* See Simpson on Homœopathy, p. 291, *et seq.*

of the ratsbane treatment of yellow fever: "If the drug is similar in its pathogenesis to the totality of the symptoms in any given curable case, that drug is certain to cure in some one or other of its potencies. If a drug fails to cure, it is generally because the practitioner has failed to find the true similimum or its appropriate preparation. The field for *experiment* in the homœopathic practice is therefore extremely narrow, and the chances of failure are correspondingly small."

This Review also gives the mortality statistics of several homœopathic physicians, who have practised extensively in yellow fever epidemics in 1853–4–5, showing a mortality of only five and a fraction in the hundred. It is upon such homœopathy and such statistical evidence that Sir John and Prof. Bennett sing *Ios* to Nature, and *Dies iræ* to Art in the cure of Disease. No yellow fever practitioner, can, among twenty severe epidemics, name one, in which not more than five per cent. of the sick were actually moribund and beyond the reach of either arsenic or lunar caustic, *before medical aid* was asked.

"We usually praise with a view to be praised," said Rochefaucauld. Drs. Forbes and Bennett have virtually, and in the strongest manner, praised homœopathists by accepting their *own self-flattering statistics* of cure as the foundation of their logic adverse to Art and favorable to Nature. The homœopathists who give "massive," nay, poisonous doses of "arsenicum," etc..* must be amused at the credulity of Sir John and Prof. B. Nevertheless, the editors of *The American Homœopathic Review*, are not ungrateful; being praised themselves by the two high Priests of Nature, they return the compliment as follows: "The really great minds in the old school have pronounced themselves in favor of *permitting Nature alone to cure diseases*† with but little or no interference from the practitioner. The whole allopathic world is thus on the brink of homœopathy. This fact is perhaps not apparent to the adherents of the old school, but it cannot fail to be evident to us, who stand on higher ground and have a corresponding wider range of vision. To our illustrious founder belongs the honor of striking

* Nevertheless the editors say, "it is certain that the discovery of the *power of attenuated remedies* is one of the greatest that medical science has ever witnessed." B. F. Joslin, M. D., LL. D., in his report to the American Institute of Homœopathy, says that "mere *clinical experience* is a squandering of time, leading to the frequent reconstruction of systems."

† Nature's cure of an intermittent is not to be admired, being slow and uncertain, while often her sequelæ, as general anæmia, swollen spleen, dropsy, and a broken constitution, are more dreadful than the original malady. The same may be said of syphilis, and not a few other diseases amendable to the healing art.

the heavy blow which scattered the confused heap of valuable facts and silly theories pertaining to the allopathic school."

Thoroughly machiavelian in purpose, the able "physician to the Queen's household" and virtually Secretary of State to Her Most Morbid Majesty "the Autocrat of all the Diseases," craftly says, "homœopathists, who, in point of fact, if they adhere rigidly to the original Hahnemannian dose, *do literally prescribe words and not things. Of all the examples of Autocracy of Nature in curing diseases, supplied by the records of medicine or by its actual practice, there is none which, in point of extent or in force of evidence, can compare with that furnished to us by the new school of practice known by the name of* HOMŒOPATHY. Since the establishment of this system, now more than thirty or forty years, an *immense number* of the sick in all civilized countries have been *treated according to its precepts and practice; that is (according to the opinion of the* BEST JUDGES, in which opinion I ENTIRELY CONCUR,) nominally by drugs, but actually left to the resources of Nature, or at most aided, it may be, by regimen and faith. The practice of HOMŒOPATHY, now so widely spread, is the MOST PERFECT as well as the GREATEST example of Expectation that ever existed in the medical world. INDEED, NO OTHER FORM OF IT CAN BE REGARDED AS QUITE PURE. The homœopathic remedies, so-called, are utterly inert, and incapable of influencing the body, in any of its organs or functions, whether in health or disease. This, to me, is a demonstrated fact. It is only in the practice of its followers that we can find an entire abstinence from all drugs of possible power. It is, therefore, from this source that we must draw conclusions," etc. 145–6, 148–9, 245–6–7.

Sir John Forbes has for many years devoted himself to homœopathic statistics of cure. The evidence which he has thus gained he calls the purest; nay, the only complete and perfect route into the realm of Nature in disease and into the very secrets of her all-wise therapy. But there is a better route to the Natural History of disease than the homœopathic. There is a large class of patients, particularly in epidemic seasons, who enter the hospitals, or ask medical aid at their dwellings, in the advanced and moribund stages of their maladies, in which no active medicine whatever had been taken, and in which none will be prescribed by the skilful physician. The observer, who is not bent on "the pure evidence of homœopathy" alone, will find a few skeptical physicians who treat the most acute diseases with gum-

water, lemonade, ptisans and the like, which do not interfere with the natural course of disease, thereby affording the data for the Natural History of Disease uninfluenced by Art. But, according to Sir John, homœopathic statistics alone are pure, perfect, and reliable. It is homœopathic Nature which Sir John adores. Allah is great, and Hahnemann is his prophet.

Prof. Bennett, in the same way quotes homœopathic statistics against the antiphlogistic treatment, saying that "no reasonable medical man can suppose homœopathic treatment to be anything else but inert."

Sir John Forbes having written a book against "all active medication," based on the complete superiority of homœopathic evidence, finally damns homœopathy thus: "It is melancholy to be forced to make admissions in favor of a system so utterly false and despicable as Homœopathy." (248.) Equally "false and despicable" is his or any book based on the supreme purity, truth, reliability, and "force of evidence" derived from such a source. Homœopathists and all others should exclaim with Lady Macbeth,

"Out, damned spot!"

If Homœopathy "is utterly false and despicable," the devotees of such a system must be either fools or knaves; if fools, they are incompetent even to diagnosticate diseases, and *à fortiori*, incompetent witnesses; if knaves, not to be believed on oath.

Thus under the pharisaical pretence of devotion to "the complete Autocrat," "Nature in Disease," Homœopathy is damned, and "Allopathy" is doubly damned.

Eventful æra! Professor Henderson, of the Medical School of Edinburgh, teaches Homœopathy openly, while his co-Professor Bennett is more prudent, and fights the regulars under the flag of Antiphlogistics. Sir John virtually hoists a flag with the motto: "*Delenda est Cathargo.*" "*A legacy*" indeed!

Will Young Physic claim under this, the last will and testament of one thitherto deemed rich in intellectual treasure, a gift not only worthless, but worse? Would it not be better to be disinherited altogether than to accept such a bequest? The following codicil taken from Rochefoucauld's maxims should be added as explanatory of the devise: "Men love to contradict their general character; dislike being thoroughly understood; they will not be a thing whose behaviour on any occasion the most careless prophet with certainty foretell."

When the ague, cholera, croup and the like, invade "the Royal Household," does Sir John *prescribe* Nature, and *proscribe* physic? Would not Victoria Regina give up her crown rather than adopt Young Physic's presumptuous and sciolistic dogma of non medication in her "household" a dogma professedly based on Homœopathic "provings?"

Art. IX.—*Yellow Fever of* 1858 *in New Orleans.*

In the July number of this Journal was reported at length the case of Catherine Maxwell, who was attacked on June the 16th, while residing on Barrack street, Second District, (one square from the Third), and who died at the Maison de Santé, on June 30th, of undoubted yellow fever. The case of Edmund Cooke, was also reported as suspicious. He was attacked on June 13th, and died at the Charity Hospital on July 3d. It is recorded on the books of the Hospital that his death was caused by yellow fever, and that he was the first admission of this disease in 1858. From the previous history of these cases, both were supposed to have been of local origin. Neither of them, so far as I can learn, served as a focus of infection; both did come from that portion of the city where the disease first prevailed.

On June 20th, three Italians were attacked by this disease. They were admitted to the Charity Hospital on the 23d, and on the 25th, a fourth Italian; all of them died between June 25th and July 2d. They were seamen on the brig Rosalie, the mate of which brig, was also attacked and died about the same time. This brig Rosalie sailed from Palermo on April 4th, and landed at Post 21 (Second District,) on the 10th of June. She had made no intervening port, and had had no sickness on board during her voyage.

About the 20th, the day the three seamen were attacked, she was towed up to Post 52 (Fourth District.) The disease was no doubt contracted prior to this at Post 21. At this same Post, lay the R. C. Wright, which arrived on June 5th from Rio Janeiro, and at Post 19, a short distance below, was the M. A. Stevens, arrived on June 4th from Havana. At both these ports, yellow fever prevailed at the time of their departure. At Post 21, was also the Pizarro from Stonington, arriving on the 17th of June, and at Post 22, the W. H. Stuart from

Liverpool, arriving in this city June 14th. The yellow fever did not appear on either of these vessels, but a seaman on the bark Flight, which also laid at Post 22, died at the Marine Hospital on July 14th of this disease. Farther, in the four different wards of the Hospital where the four seamen from the Rosalie lay sick and died, there were confined during this time, from June 23d to July 2d, twenty unacclimated patients, eleven of whom, had been in New Orleans one year or less. None of these were attacked with yellow fever while in the Hospital. One of the twenty was discharged on the tenth day after his exposure to the disease, the balance remained some three weeks and more, affording ample opportunity for the contagious property of the disease to manifest itself. These facts would seem again to confirm what preceding years have proved, that if yellow fever be contagious, it is so under peculiar circumstances *only*, something else being required besides exposure to the sick.

At Post 33 (Third District), some five squares below Post 21 where the Rosalie lay, were moored two vessels, the Elizabeth Ellen, Captain Staigg, and abreast of her, the Independence, Captain Eustis. The latter had been in New Orleans several months from New York. Immediately in the rear of these two vessels, lay the Trumbull, Captain Smith, and the F. B. Cutting, Captain Lyons, both of these had also been sometime here from Northern ports.

The Elizabeth Ellen lay at St. Thomas some two months, during which time, yellow fever prevailed severely. On May 18th, she sailed for New Orleans with eleven passengers. On the 24th of May, one of her crew died; he is reported by the Captain, to have vomited blood, but from consumption. This vessel arrived at quarantine on the 4th of June, was visited and fumigated, then allowed to pass landing in New Orleans the same day. Captain Staigg stated that on the voyage, he had had some fever which was followed by slight juandice and abscesses, but that it was not yellow fever, which he had had before; with the exception of this and the seaman who died, there had been no sickness on his vessel. His mate and stewardess confirmed this testimony. However, Captain Eustis asserted that Staigg had informed him that his disease was yellow fever. Captain Healy, since dead, had heard from (I believe) Staigg himself, the same thing, and a reliable person now residing in the city, testifies that a member of Staigg's family living in daily *intercourse* with him, asserted that his fever was yellow fever.

Farther, the mate prior to his testimony to the Board of Health, informed a physician of this city that a case or cases of yellow fever had occurred on the Elizabeth Ellen before her arrival in New Orleans. Still farther may be added the following testimony:

"Mr. Alexander Wolff is a native of St. Thomas, and was a passenger on the ship Elizabeth Ellen on her trip from St. Thomas during the month of May. He has seen yellow fever, and learned from the captain that one of the crew died on the voyage, he did not see him, but understood that he had vomited blood and black matter. He died hard, screaming and in much agony. About six or seven others of the crew were sick, but recovered. They complained while sick, of headache and pain in the back. The captain was sick with a high fever, and delirious for twenty-four hours. His face was yellow and red during the fever, but yellow during convalescence, when also he had boils about his person. The captain's son was also sick with fever. None of the passengers were sick, they were chiefly from St. Thomas and Rio Janeiro.

[Signed] A. Wolff."

New Orleans, October 1, 1858.

Whether Staigg or any of his crew had yellow fever or not, six days after his arrival, that is, on the 10th of June, the daughter of Capt. Eustis, living on her father's vessel, the Independence, was attacked with fever and died on the 22d. On the 14th his son was seized with fever, dying on the 20th; on the 24th, his nephew, who recovered; prior to the 27th, his seaman, Thomas Mervins, who died July 1st, "black vomit found in the stomach." About this same time, (between June 20th and July 1st) Captain Smith of the Trumbull, also Captain Lyons of the F. B. Cutting, (the proximity of these vessels to the Elizabeth Ellen and the Independence will be born in mind), as well as his mate and daughter, were taken sick, the two first dying; also, Captain Healy was attacked with fever while going North and died on the fifth or sixth day at or near Cairo, and is said to have thrown up black vomit. This last victim was a friend of the others, and had assisted Smith, Lyons, and his mate, in transferring the body of Captain Eustis' son, after decomposition to considerable extent had occurred, from an ordinary to a metallic coffin. On June 29th, a seaman from the F. B. Cutting was admitted into the Marine Hospital, dying

July 7th, "black vomit found in the stomach." On July 6th, another seaman from the same vessel was admitted with yellow fever who recovered.

Here then, we have eleven cases of suspicious fever, originating upon those three vessels which lay in immediate contact with the Elizabeth Ellen recently arrived from St. Thomas where yellow fever was prevailing. Of these eleven cases, I have medical authority for stating that Enstis' nephew was not a well marked case of yellow fever, that Captain Lyons' daughter was not yellow fever, that black vomit was found in the stomach of the sailor from the Independence, as also the one from the F. B. Cutting; another sailor on the latter also had yellow fever. Thus disposing of five out of eleven, we come to the six cases which it is denied were yellow fever. These are Enstis' daughter and son, Captain Lyons and his mate, Captain Smith and Captain Healy. The last for the present, rests upon the rumor that he died with black vomit. The other five are the most important, as the decision that their deaths were caused by yellow fever, would make them the first of the season.

Notwithstanding the opinion of a physician to the contrary, who saw some or all of them, (but I believe attended none of them throughout,) I think these five did die of yellow fever for the following reasons. Those who saw them agree that they had fever, that they died from the second to the twelfth day, and that their bodies after death were yellow, the depending parts black from congestion. Among the witnesses to this effect is the undertaker, who in growing old, has seen thousands of yellow fever victims, and adds his voice to the general rumor which assigns their death to this disease. Farther of the eleven cases, eight died, two of them with black vomit in the stomach, these two having been attacked with their disease at the same place, and about the same time as the others. Now what other febrile disease existed in New Orleans in June last, of which out of eleven *unacclimated* persons attacked, eight died? And in what disease does such mortality generally occur in our summers?

Since writing the above, the following letter has been received which confirms, in the most positive manner, the opinion previously expressed. Did our friend's notes require any apology, which they do not, I would state that when written they were not intended for publication.

"U. S. Marine Hospital, New Orleans, Oct. 4, 1858.

"*Dear Doctor:* In reply to your note of the 21st, I have to state that Dr. Ker, [principal Surgeon, U. S. Marine Hospital,] informed me this morning, that the son and daughter of Captain Eustis, of ship Independence, were undoubted cases of yellow fever, as were also the cases of Captain Lyons and the mate of the F. B. Cutting, and Captain Smith of the Trumbull. He does not think the daughter of Captain Lyons had yellow fever.

"Leonard Eustis was brought to this Hospital from the ship Independence on the 24th June. At the time of his admission, he had no fever, but seemed to be convalescing, complaining simply of general debility. I cannot, under the circumstances, pronounce it yellow fever. The first cases admitted this year, of this disease, were as follows:

"Thos. Mervins, seaman on ship *Independence*, admitted June 27th, died July 7th. *Black vomit found in the stomach.*

"John Robinson, (f. m. c.) seaman on ship *F. B. Cutting*, admitted June 29th, died July 7th. *Black vomit found in the stomach.*

"Geo. Miller, seaman on ship *F. B. Cutting*, admitted July 6th, discharged July 16th.

"John McIlroy, seaman on ship *Revenue*, [Post 39, Third District,] admitted July 7th, died July 16th.

"Hugh McIntosh, seaman on *Bark Flight*, [Post 22, Second District,] admitted July 12th, died July 14th.

"The next two cases were deck-hands on the Towboat *V. H. Ivy*, and were admitted on the 10th of July, and both died of *black vomit.*

"The number of discharges of yellow fever cases was, in August, 23, deaths, 21. In September, discharges 29, deaths 19.

"Hoping the above items may prove serviceable to you in your investigations,

I remain, yours, etc.,

J. Winchester Breedlove,

Resident Physician, U. S. Marine Hospital."

On July 1st, another death of this disease is reported to have occurred at 29 Dumain street, about half a mile above the Elizabeth Ellen. On July 6th, F. Collins died at the Charity Hospital—he came from the E. P. Stringer, Post 48, Third District, some distance below

the Elizabeth Ellen. On July 2d, a death is reported on Delord street, and from the 7th to the 19th, Dr. Bensadon saw four cases, also in the First District. These five were all near the shipping, and nearly two miles above the Elizabeth Ellen.

Such are the details in regard to the origin of yellow fever this year. That those entertaining different theories on this subject, should interpret them differently, is to be expected. So much is certain, that the disease made its first appearance upon or near the shipping, for even Catharine Maxwell was but five squares from the levee, and about seven from the Elizabeth Ellen; that the very first cases were attacked on ships adjoining a vessel from an infected port; and that the disease prevailed first in that district of the city where the Elizabeth Ellen lay. Now, it behooves the absolute contagionist, who believes the Elizabeth Ellen was the "*fons et origo mali*," and that she or some other vessel from an infected port conveyed the disease to the Rosalie, to explain why the disease was not likewise contracted on the Pizarro and Stewart, and why none of the twenty unacclimated patients confined in the same wards with the four Italians from the Rosalie did not become infected? And will not the non-contagionist admit that the eight deaths out of the eleven fever cases which occurred on those vessels, which alone were in *immediate* contact with the Elizabeth Ellen recently from an infected port, was a singular circumstance, and a most remarkable *coincidence*?

The disease which thus began in the Third District of this city on June 10th, attained its maximum mortality the week ending September 12th, when our cemeteries received 472 new occupants. Its victims will number probably 4500, thus causing a greater loss of life than any epidemic which has ever devastated New Orleans, except that of 1853. And it may be doubted if the mortality in proportion to the total number of cases has ever been surpassed, for I do not believe the latter can be estimated at more than treble the former.

Although the mortality increased every succeeding week until its climacteric, yet so slight was its increase and subsequent decline, that it may be considered to have been stationary from August 22d to the present time, October 10th, an unusual, if not unprecedented duration. This is no doubt attributable to the great number of unacclimated strangers, (several hundred have arrived in a single day) who, rushing in to fill the vacant places of the dead, had no fear of shaking our

grim lion's beard. Their temerity has been chastised, and far different places awarded many of them, from those they hoped to occupy.

The daily mortality has also been peculiar in its irregularity. One day often almost doubling both the preceding and succeeding. In this as in other years, children of every age, and born in the city, have been attacked, and some have died; and persons who have passed many summers here and through several epidemics, have paid their tribute to the disease this season, among others, a driver of the Charity Hospital's hearse, who had resided eleven years within its walls. As usual in our epidemics, all have seen many remarkable recoveries, and many most singular deaths. The type of the disease is termed by some adynamic, others say, call it what you please, it is still identically the same old yellow fever familiar to them for many years, the type of which is determined by peculiarities of constitution, etc. It deserves mentioning, that at the beginning of the epidemic, there was observed a singular disappearance of flies, and that the black tongue has not prevailed, I believe, in Louisiana, although reported to exist among the cattle, etc., of adjoining States.

The following places have been visited by yellow fever this year: Matamoras, Brownsville, Houston, Galveston, New Orleans, Pass Christian, Biloxi, Mobile, Savannah, Charleston; and Plaquemine, Baton Rouge, Woodville, Natchez and Vicksburg. It is hoped that our subscribers in these places will furnish us with the facts bearing upon its importation or local origin.

In the past six years, New Orleans has been laid waste by four terrible epidemics, which have carried to their graves some 18,000 of her inhabitants. These plain, but terrible figures, are a sufficiently eloquent description of the woe and anguish inherited by the survivors. The men of heart, the men of money, the ignorant, and the learned, all have their attention directed to the two great practical questions: How can yellow fever be excluded from our city? When once admitted, by what remedial measures can we diminish its mortality? True, *wise* men have sprung up this year as before, to respond to these questions most satisfactorily to themselves. They can exorcise this yellow devil, both keep him afar off, and cure all or nearly all of those he may attack. Notwithstanding all which, the destroyer comes like the waves over the throne of the impotent Xerxes. And notwithstanding all which, their *autographs* can be found with the sextons, and neither

do their statues adorn our public squares, nor their busts fill a niche in any of our halls of learning.

But what reply do we receive from those noble, earnest students and thinkers, who, like their professional brethren of every nation and in every quarter of the yellow fever zone, have made these questions a subject of profound study—a study prolonged by successive generations, through some four centuries? These men have lived with it, slept with it, drank its vomit, sipped its urine, inoculated themselves with both, and have brought all the resources of science to bear upon it, meteorology, chemistry, microscopy, etc.; and still we doctors write and wrangle; yellow fever comes and goes as of yore, and if the hospital and army statistics of the past are reliable, the mortality by this disease is as great now as formerly. We have, as yet, but one remedy in our control for these evils, and that was not discovered this century—*to remove the unacclimated.*

Still, in the elucidation of those phenomena which may ultimately lead to great practical benefit, much has been accomplished. Among these, may be mentioned, that it has been established *beyond doubt*, that cases of yellow fever do originate in New Orleans, and that the disease, if ever, is certainly not *always* contagious. In regard to the former, it is thought by some, that these sporadic cases do, in fact, owe their origin to germs originally imported, which, however, have lain dormant for one or more years. Ought not those conditions which cause yellow fever suffice to give vitality and efficiency to these germs? If so, Tchonpitoulas street, where the disease first appeared last year and principally prevailed, should have given this year some evidence in favor of this idea!

Recent investigations into the abnormal conditions of the urine of yellow fever patients promise us assistance in diagnosing the disease, and may also be regarded as another forward step. Besides its uniform acidity in the first stage, and certain other chemical and microscopic peculiarities, it has been found albuminous, with few exceptions, at some period in the course of the disease. La Roche "unites in opinion with those who believe that it rarely happens that a patient suffering from the yellow fever has not albuminous urine." Dr. Blair tells us that albumen usually appears in the urine the second or third day, sometimes the first, sometimes not till black vomit has appeared. He has also observed its

successive appearance and disappearance on different days. Where the disease was aborted by quinine, which Dr. Blair administered freely, the urine was not albuminous, and croton oil given early, retarded its appearance. He farther states, that sometimes the albuminous deposit appeared in specimens after cooling, when both heat and nitric acid had failed to render it perceptible, and that the albumen never presented the same white appearance as in Bright's disease. Dr. Blair did not find the urine albuminous in either intermittent fevers, small pox, rheumatism, pleurisy or pneumonia. However, it should be borne in mind by those trusting to albuminaria as a means of diagnosing yellow fever, that Dr. Geo. Johnson, of London, has demonstrated its presence in typhus and typhoid fevers, and farther, Rokitansky writes, that: "Albumen is discovered in the course of numerous diseases, both accompanied by and unassociated with renal disease. In many acute diseases, albuminous urine is secreted with an excess of lithic acid, and lithate of ammonia. Albumen is sometimes found with sugar in diabetic urine; it always occurs in hæmorrhage into and inflammation of the urinary passages, in hyperæmia, nephritis, etc. It is found to a large amount in Bright's disease of the kidney, frequently mixed up with blood-globules, or hæmatosine. Its presence is demonstrated by milky turbidity of urine, by the urine foaming when air is blown into it, by coagulation of the albumen on the application of heat, the addition of alcohol, or nitric acid, etc."

A more accurate diagnosis has a direct bearing both upon the treatment, and the introduction of this disease. Upon the former, and the means adopted by our State to prevent the latter, I shall venture a few words.

The great mortality in proportion to the total number of cases, will not lead us to exaggerate the importance of the remedial measures applied by the profession at large. In fact, there have been a very great number of cases amenable to no treatment. For these, as well as for that larger class of cases, who, beyond all doubt are benefitted by medicinal agents, every variety of treatment has been tried; from syncopal bleeding to foot-baths and ice in lumps. It would be difficult to mention anything which has *not* been tried, except transfusion, the experiments with which in other places have been far from encouraging. Many relying chiefly on some potent drug have yet not disdained to resort to those remedies applicable to particular symptoms

and conditions; others have pursued what is termed the expectant, or the *rational* system. Some of the representatives of both systems tell us of the very small number of certificates they have issued. An erroneous means, however, of arriving at an accurate comparison of different treatments, as was illustrated by a physician in 1853, who, on summing up the certificates as stated by his various friends, came to the conclusion that the responsibility of some seven thousand deaths out of a mortality of eight thousand, rested on his shoulders alone! Whether the various measures resorted to, have been only different means of arriving at the same end, is no doubt to some extent true, and certainly they have proved that a majority of patients will recover under every variety of treatment.

In the catalogue of medicines administered this year, is that too much praised, *too much abused* remedy, quinine. As an old and faithful friend who has served us long and well, I should feel guilty of moral cowardice to offer no word in his defence, now when some of his quondam friends have deserted him, and his ancient foes show him no quarter. Knowing how little weight either my opinion or experience carries with it, I shall avail myself of another's assistance, whose name commands respect wherever it is known. Dr. Warren Stone wrote in 1854, and all he wrote then, has been equally applicable since. "My experience is, that all systems of active perturbating treatment are about equally successful, or rather equally unfortunate. Another system has for its general features the expectant plan, with the addition of the use of quinine in the early stage.

"Although this may be termed a system of treatment, there are a variety of methods of administering quinine. Some give it in large quantities, without qualification or preparation; some endeavor to obtain a remission, which can almost always be done if the patient is seen early, and some are very timid in its use, and would do better not to give it at all. I have used it for the last seventeen years, and have made but little alteration in my method since 1837. If I see the patient early, or in the forming stage, I am in no hurry to administer the quinine. The stomach is often full; the patient may vomit, or may require a dose of oil, or something to evacuate the bowels, which can be given with a hot foot bath, and afterwards the quinine, in quantity sufficient to put the patient fully under its influence, and this may be done either by a single dose or in two or three divided doses given in a

short time; I prefer a single dose if the stomach will bear it. Twenty grains of pure quinine is enough to put any patient under its influence, and I am satisfied that enough is better than more, although I do not believe in the evil effects of quinine, which some have, in my opinion, unjustly and injuriously accused it of. It is unfortunate that men will endeavor to sustain their own peculiar systems by detracting from, and throwing odium upon those of others, instead of resting upon their own merit. There can be no doubt as to the great value of this remedy in this disease. Its mode of operation will better explain its value. If given early, it relieves the excruciating pains even of the head as completely as in the case of a distinct intermittent neuralgia, diminishes the injection of the conjunctiva, promotes perspiration to profusion, prolongs the sweating stage in a large majority of cases, until the disease subsides and maintains the integrity of the nervous system, so that the different functions will be but little disturbed. The expectant practitioner will say that can be done in a majority of cases without quinine. I grant it, but quinine, in addition to the expectant treatment, will save ten per cent. more in my opinion. In 1837, I had four wards in the medical department of the Hospital; the patients were admitted in regular turn to each physician, and of course, in the course of a month the cases must have been about equally favorable, and ten per cent. was the difference in result. I even divided my own wards two and two, until I was so well satisfied of its advantages that I felt, that I was not doing my duty to neglect to give it in a fair case, and only continued my observations upon doubtful cases, or cases advanced in the disease. The true principle is to administer early in the disease, sufficient quinine to place the patient under its influence; say from fifteen to twenty-five grains, according to the case, and repeated after some hours, so as to keep up the influence gently. I formerly was in the habit of giving with it either blue mass or a few grains of calomel, but finding, as a general rule, no condition requiring it, I have dropped its use. The case should otherwise be simply managed; cups, either dry or scarified, or sinapisms, to relieve the pain of spine or head, may be useful. I generally prefer sinapisms, as it disturbs the patient less to apply them. Cold drinks, as a general rule, but occasionally in the latter part of the disease, drinks as hot as can be taken, have a fine effect upon the stomach. If the attack is severe as it is subsiding, the patient sometimes becomes restless, and com-

plains of a burning sensation in the stomach, indicating the acid secretion that always precedes and accompanies black vomit. This may be relieved by the admistration of alkalics, with very minute doses of opium or morphine, not more than a twentieth or thirtieth of a grain of the latter at a time, for patients are extremely susceptible to its action, and a small quantity may produce stupor.

"Many of the cases, I have said, have nervous delirium in the last stage, or rather as a sequel. In 1847, most of the fatal cases were of this character, having passed the stage of black vomit, which is the natural termination of all fatal cases. This must be anticipated, and anodynes administered moderately before the patient is so renovated as to render them dangerous, and stimulants resorted to earlier than would appear necessary from the evidence of mere exhaustion. I have said that quinine should be given early, at least as soon as the cold stage has passed, but I never give it in the second or last stage. Some give it after the disease has subsided, and the patient ought to be left alone. Some are too timid to use it in the early stage, when it ought to be used, and finding ordinary means fail, resort to it when I would not; and some give it in unnecessary and unwarrantable doses from beginning to end. Is it strange that quinine has its opponents? I am as strong an opponent of such use of it as I have seen, as any one, but because a thing may be abused, should it be discarded? I do not pretend that quinine cures, or cuts short the disease, but it favors the natural termination, and renders cases simple, that without it would be dangerous. Some cases I have said terminate in twenty-four to sixty hours, under the expectant treatment. With the early use of quinine, a great many more cases would have this termination. These cases, however, have to be kept quiet the usual length of time, for if they are allowed to get up they become faint, or if they are guilty of imprudence, the disease terminates in black vomit and death, showing that it takes a certain time for the specific poison to work its effects.

"The epidemic of 1853 was the severest in character ever known, and more cases were fatal in their nature, in spite of any management; and all kinds of extravagances were committed in treatment, and many conservatives (perhaps honestly) seized upon quinine, and held it up as the cause of many of the symptoms that belong to the severity of the disease. It was as valuable in the proper stage last year as

it ever was, but there were more cases in which all means failed to produce the usual favorable effects than ever known before. Many claim the merit of originating the use of quinine. I used it in two or three cases in 1835, but the epidemic of 1837 was the first opportunity I had of giving it a fair trial, and the principles upon which I use it were established that year. I have given my views of the main features of the nature and treatment of yellow fever, without entering into the many minute details of nice management of critical cases which my feeble powers of description would fail to make plain, and which must be learned at the bedside. One of my objects in view, was to give my feeble voice in support of a most invaluable remedy which has, in my opinion, been unjustly assailed, and the faith of the public shaken in regard to its use."

Upon the means adopted by this state, to prevent the introduction of yellow fever, there are few arguments which are not threadbare, but the statement of some *facts* may not be *mal apropos*. Quarantine was established in Louisiana in 1821, and continued until 1825. In 1821, there were seven admissions of yellow fever to the Charity Hospital reported. In 1823, there was a severe epidemic, which, in the language of a distinguished contagionist, "was introduced from Pensacola by the way of the Passes and the Lake." There are two deaths of yellow fever reported as having occurred at the Charity Hospital in 1823. In 1824, there was another severe epidemic, which, by the same authority, "was introduced by the towboats." In 1825, quarantine was abandoned, to be resumed again by an act passed March 15th, 1855, in which year, a severe epidemic occurred, which, however, began, I believe, before the quarantine was in working order. In 1856, there were 74 deaths by yellow fever; in 1857, 200; the disease was supposed by some, to have been imported from Havana via Mobile. In 1858, a terrible epidemic again prevails, which the future historian may assert, "was introduced from St. Thomas via the quarantine station." Such are the quarantine facts of this State, and if we travel North for similar items, they will be found not a jot better. Quarantine existed in Philadelphia throughout all her epidemics. New York had four epidemics before the establishment of quarantine, and has had ten since; furthermore, there once occurred in New York an interval of twenty-two years without yellow fever and without quarantine.

From all which facts, it does not necessarily follow that yellow fever *may not* be imported, but it is impossible to avoid the logical conclusion that, either this disease cannot be imported, or, that the laws establishing quarantine regulations, and the execution of these laws, have thus far been totally inefficient, and therefore injurious to the interests of this community. With such facts gathered from past experience, what can we hope for the future? New Orleans will, in a short time, be connected by a day, or a very few days travel, with every seaport in the Southern States; and before a great while, we must expect to hear of the introduction of yellow fever via Memphis and the Mississippi River, or down the Jackson and Great Northern Railroad, or via Opelousas and the Great Western Railroad, etc., etc. If the frightened constituents of our Solons insist upon paying for this sort of *protection*, we have still a right to expect of their wisdom, a satisfactory and *consistent experiment*.

To conclude these cursory remarks upon this endless subject, we have compiled and appended two tables which may be of some value for present and future reference.

YELLOW FEVER EPIDEMICS IN NEW ORLEANS.

There is authority for stating that cases of yellow fever occurred in New Orleans in 1769 as well as in 1791; but the first unquestionable epidemic did not occur until its population was about 6000 in 1796. The yellow fever again prevailed in 1799, 1800, 1801–'4–'9–'11–'12. 1817–'19, violent epidemics; 1820–'22–'24, epidemics; 1825–'27–'28, mild epidemics; 1829–'30, epidemics; 1833, violent epidemic; 1835, epidemic; 1837–'39–'41, violent epidemics; 1842, mild epidemic; 1843, epidemic; 1847, violent epidemic; 1848–'49, mild epidemics; 1853, great epidemic; 1854–'55–'58, violent epidemics.

To the above may be added the following facts frequently referred to: In 1807 and 1808, the embargo was in force, there was no yellow fever. In 1809–'10–'11, there was no embargo, but yellow fever in 1809 and 1811. In 1812–'13–'14, there was war, yellow fever in 1812 only. In 1815 and 1816, neither war, embargo, nor yellow fever.

From 1821 to 1825 there was quarantine, and epidemics in 1822 and 1824.

In 1823, the yellow fever prevailed at Fort Smith, Arkansas, and Natchez, Mississippi. There were only two deaths reported at the

Charity Hospital of New Orleans this year, one, August 23d, the other, September 11th. The first case in Natchez was on August 12th.

In 1844, there was a severe epidemic in Woodville, Miss. The first case in New Orleans this year, was attacked on July 20th. The first case in Woodville, on July 12th.

March 15th, 1855, quarantine was again established; yellow fever epidemics in 1855 and 1858.

Since 1796, there have probably been some cases of yellow fever every year, certainly so, since 1817.

MORTALITY STATISTICS OF YELLOW FEVER, 1844—1858.

Year.	May.	June.	July.	August.	Sept'ber.	October.	Nov'ber.	Dec'ber.	Total.
1844	.....	.....	1	4	54	.....	.....	.....	148
1845	.....	.....	.....	.....	.....	.....	.....	.....	2
1846	.....	.....	.....	1	8	118	33	.....	160
1847	.....	.....	74	965	1100	198	12	10	2259
1848	.....	4	33	200	467	126	20	.....	850
1849	.....	.....	.....	8	188	396	137	8	737
1850	.....	.....	5	60	33	4	.....	.....	102
1851	.....	.....	.....	.....	.....	.....	.....	.....	16
1852	.....	.....	2	8	91	198	105	11	415
1853	3	40	1406	5189	1135	165	28	4	7970
1854	.....	.....	29	532	1234	490	131	7	2423
1855	.....	5	382	1286	874	97	19	7	2670
1856	.....	.....	.....	14	40	16	4	.....	74
1857	.....	1	1	1	8	98	82	8	199
1858	.....	2	132	1140	1825	.....	.....	.....	.....

The figures in this table, for the years of great mortality, are by no means perfectly reliable until 1855. They are, however, the best which can now be obtained. The error lies in fewer deaths having been reported than actually occurred. To 1847–'53–'54, doubtless, several hundred should be added.

For the completion of 1858, the reader is referred to the monthly reports in this and succeeding numbers.

STANFORD CHAILLÉ.

ART. X.—*Yellow Fever in Mobile, A. D.*, 1858.

MOBILE, 29th Sept., 1858.

DR. STANFORD CHAILLÉ: *Dear Sir*—I received, two days ago, your letter requesting me to give you the facts respecting the origin of yellow fever in Mobile this season, to which I give a hasty response,

containing the most reliable information I can obtain on the subject.

On the 3d of August, Dr. Woodcock was called to see a boy named Perret, on New Hampshire street, west of Franklin, with what he regarded as a bilious remittent fever, and, after attending him four days, left him on the 7th, apparently convalescent. On the 13th, Dr. Hamilton was called to him in a relapse of some days, which had been neglected, and he died that night after copious black vomit, and hæmorrhage from the nose. The boy was a pauper, a street vagabond, of bad constitution, and seeing him only in the dying stage, Dr. H., did not feel justified in forming a positive diagnosis, as to the nature of the disease. This boy slept about a quarter of a mile from the wharves, but was much about them, fishing and idling.

On the 28th of August, three unequivocal cases sickened, and died with black vomit on the 2nd, 4th, and 5th of September; viz: a boy of five years by the name of Bligh, on Cedar street, between Government and Church, and Creight and Shileto, on Dearborn, between Government and Church, two squares west of Bligh, Perret being about half a dozen squares south-east.

None of these cases, as far as can be ascertained, had any communication with yellow fever of foreign origin, and would seem to have sprung up from some local cause, or latent germ. Another case has occurred since in the house of Bligh, and two or three more in the house where Creight and Shileto died.

Our extreme contagionists in Mobile believe that the disease was brought from New Orleans, but certainly no facts have been brought to light, that would warrant such a conclusion. Dr. Ketchum, who attended Creight and Shileto, investigated the facts fully, and can find no ground for the idea of importation; a brother of Shileto came from New Orleans to Mobile *one month* before these cases occurred—spent a few hours at the house, with his unopened trunk sitting on the gallery, and left with his baggage, on an up-river boat without communicating disease to any one at the house in the country where he joined his family. It may be stated in connection, that hundreds of persons have been flying from New Orleans during the epidemic now prevailing in that city, and stopping for days and weeks in our hotels in Mobile, at Point Clear, at Bladon Springs and other places, where not a case of yellow fever has occurred.

There were two deaths the first week of September, five the second,

thirteen the third, and thirty-nine the fourth week. The number of deaths altogether from yellow fever up to the present date has been about seventy, nearly all of which have been on, or south of Government street, or about one-third of the town; it may be doubted whether a single case originated north of this line, or in other words, has been contracted out of the infected district alluded to.

The above are the facts as nearly as I can ascertain them, and I will take the liberty of adding a few remarks which you can print or burn as you think best.

I agree fully with Prof. Warren Stone, of New Orleans, that yellow fever does not reproduce itself in the human system, like small pox, syphilis, typhoid fever, and many other clearly contagious diseases. This fact I think clearly demonstrated by the facts stated by Dr. Stone, respecting your Charity Hospital, as well as similar proofs on a large scale in New York, Philadelphia, and other places. If in one of those cities you establish a hospital for yellow fever (as has been done repeatedly) and place several hundred yellow fever patients in it, who are attended by "unacclimated" physicians and nurses; and surrounded by "unacclimated" population, it will not be contracted by a single individual, if the hospital be situated beyond the "*infected district.*" This would seem impossible if the morbific cause were reproduced in the human body. This admission, however, does not disprove the validity of the position, that the *germ* of yellow fever may be transported in a vessel, in the closed baggage cars of rail roads, or otherwise. There is as ample reason to believe that the morbific cause of yellow fever is multiplied, extends itself from place to place and attacks its victims, as there is that the army worm propagates its kind—travels from field to field and destroys every thing before it. Not only is the germ clearly transported in vessels, etc., but, I believe, in some rare instances, it may be enveloped in the garments of persons and transported short distances. Instances of this kind, however, are rare.

Yellow fever in our Northern cities is not only an exotic, but the climate there is unsuited to it, and it dies out, when carried there, sooner or later—sometimes struggling on for several years, as it did from 1793 on to 1802 or 1803. I do not recollect now the last cases. When this disease occurs in Northern cities, it is always in the neighborhood of the shipping, which is not the case with us. There

being more filth and foul smells about the docks than elsewhere, most persons have attributed its occurrence to such local causes; but the fact is that the disease in Northern cities comes from *foreign vessels*, and not from filth—this may be the source of other maladies, but has no more to do with yellow fever than it has with small pox, or measles. In our Southern sea ports where we have the disease more or less almost every year, we never know where to expect it. It certainly has no particular connection with filth or even crowded population. I can speak confidently as regards the City of Mobile, where I have been watching this disease for twenty-three years. We have had it here, to greater or less extent, in 1837–'39–'42–'43–'44–'47–'51–'53–'55–'58, and except in the year 1842, when it commenced in Spanish Alley, near the docks, it has always occurred at a distance from the shipping, and often in those parts of the town, occupied by the better classes of population. Previously to the epidemic of 1853, no one here ever talked of the importation, or contagion of yellow fever.

The disease has never been traced with any satisfactory evidence here, to foreign origin, but the laws of contagion and epidemics are so hidden from us, that it may have been brought in ways unknown to us. Certain it is, that in '53 the disease traveled to every point in the interior, connected with Mobile by railroad or steamboat—an occurrence which had never been noticed before in our youthful State.

It seems clear to my mind that the extreme contagionists, and non-contagionists are equally in error, and that there is a middle ground on which they might meet with advantage. I will say a few words in illustration. Small pox, measles, scarlet fever, and cholera, are all Asiatic and imported diseases, and have all became domesticated in this country; and yellow fever, whether of African or tropical American origin, may, in like manner, have been domesticated in those Southern sea ports, where climate, soil, etc., are congenial.

There was not a pock-marked Indian on the continent when it was discovered. Measles was equally unknown to the people. Even as late as the time of Dr. Rush, scarlet fever was so rare, that he remarked no physician in America would be likely to see it more than once in his life time; and cholera made its first appearance on this side of the Atlantic in 1830. Now, does any one pretend that any one of these diseases requires to be imported to explain its appearances every time it springs up in various points of our wide domain?

Have not, on the contrary, these Asiatic diseases, scattered over the land, their several *germs*, which lie dormant for a time, and then like many forms of insect life, rouse up from their slumbers under the influence of exciting causes unknown to us? Why, in fact, may not yellow fever obey a similar law? This law does not conflict at all with the idea that a fresh importation of the morbific cause may be made from time to time. Why may it not be here domesticated like other foreign diseases, as well as animals, plants, insects, etc.?"

I received a short time since, from a leading physician in New Orleans, a letter, from which I give the following extract, as worthy of record and comment:

"We are in the midst of an epidemic of yellow fever. Until the last week it has been confined to the shipping, and the Third District. Now, cases of it are occurring all over the city. The origin is involved in considerable doubt, although I have but little hesitation in expressing my conviction of its importation, by a coral ship, in ballast from St. Thomas. Three-fourths of the cases up to this date have occurred in the direct neighborhood of the ship, before her removal lower down the river. At this latter position she again became a focus from which other cases happened. Yet it is due to truth and candor to say, that at the time of the sickness of the first patient supposed to have taken the infection from her, and on the very day of his death, an Irish girl sickened and died, who did not live within six or eight squares of the ship. Both cases sickened and died simultaneously," etc., etc.

For my own part, I can see no contradiction in the facts stated. Why may not the case of the Irish girl have been a sporadic case, arising from an old dormant germ, and why may not a fresh importation have been brought on the coral ship? I have, in Mobile, in different years, seen a single case, two or three cases—ten or twenty —fifty or one hundred, and so on to full epidemics. These scattering cases, and some of the milder epidemics were to all appearance of "local origin," or may be the product of dormant germs, scattered broad-cast by epidemics of former years. The *great* epidemics, are few and far between, and are usually seen from afar, advancing from the tropics, and ravaging like Tartar hordes, year after year, the Gulf and Atlantic coasts.

If the history of yellow fever be carefully studied, it will be seen

that one of these great moves is set in motion from time to time, and travels on, (though erratically like cholera) from South to North, for a series of years before it exhausts itself. The epidemic that reached Philadelphia in 1793, was one of those examples, of which there have been several since. The last one, was that of 1853, which, starting at Rio in 1849, and traveling steadily along the Carribean Sea, Gulf of Mexico, and Atlantic Coast, did not reach Norfolk before 1855; and I have my doubts whether New York is not yet to have its turn; it looked very menacingly at the city from Brooklyn and Fort Hamilton, and the germ may still be slumbering there, but not dead.

When one of these great epidemics comes along, it is extremely violent and fatal; it would seem to leave its germ along its track, which mingles with the local malarial fevers of our cities, assumes a *hybrid* form of less malignity than the original disease. Being an exotic, it dies out in Northern cities after a time, and *possibly* might, in Mobile and New Orleans, if fresh arrivals could be prevented.

That this disease is sometimes imported in vessels, I do not doubt, but that any quarantine could stay such an epidemic as the one that marched steadily for five years from Rio to Norfolk, I see no reason to believe. After watching its career from Rio to Mobile, I ventured to predict in print, even the years it would reach Savannah, Charleston and Norfolk, and the prediction was verified.

But I am wandering from my present object, which was simply to give you a reply to your queries about the origin of the yellow fever prevailing at this moment in Mobile.

Very respectfully, and truly yours, etc.,

J. C. NOTT.

ART. XI.—*Wonderful effects and new application of the "Ready Method"* of Marshall Hall: By HUMPHREY PEAKE, M. D., Arkadelphia, Ark.

ON Tuesday evening, August 23d, 1858, between eight and nine o'clock, I was called to see William A. Dickinson, of about the age of twenty-two years, a resident of this place. The messenger, a negress, came for me in great haste, saying that Billy was dead; that they had found him so in the garden. Suspecting that there might be some mistake, I ran with all my might to the house of his brother, a dis-

tance of three or four hundred yards, where the patient was. I was informed, as I entered the house, that he was not dead, and on going to the room in which he was, I found him tossing wildly about the bed, apparently, in the very greatest agony, uttering cries and groans indicative of the most intense suffering. The report of his condition having spread rapidly, a number of persons had collected at the house. He seemed conscious but returned no answers to my questions. His feet and hands were very cold, the coldness extending a considerable way up his arms and legs. The pulse was feeble and beating about one hundred in a minute. I gave him half a drachm of McMunn's elixir of opium, with two drachms of aromatic spirit of ammonia in a draught of brandy, which he swallowed, it seemed, with some difficulty. I ordered his legs and arms to be rubbed with flannel cloths wetted in brandy containing cayenne pepper; sent immediately for mustard, and had a hot bath prepared. I noticed that he had cramps in the muscles of his feet and legs. I now got the following history of the case: He had been in the river seining during the whole of the afternoon, exposed to a very hot sun. The day was a very warm one. He came home in the evening after dark, and ate his supper alone, the balance of the family having already eaten. Nothing unusual was observed of him. Shortly after eight o'clock, on the family's retiring to bed, his absence was noticed. Nothing, however, was thought of this; it was merely supposed that he had gone down town, which was nothing unusual with him. Shortly after this a negro woman happened to go in the garden where she found him lying upon the ground—speechless, senseless, and, she thought, dead. This negress was dispatched for me, and a general alarm given.

I regarded the case as one of pernicious fever. The rubbings seemed to afford little relief; when the bath being ready he was put in up to his hips, and his hands immersed; mustard was added to the bath. This soon afforded relief and he now answered my questions. He complained of great pain in the head, epigastrium, and hypochondriac regions, more particularly the left one. He was taken out of the bath and rubbed with dry cloths. His sufferings although relieved, were still intense. I began immediately to prepare sinapisms for the arms and legs. They were being put on him as I prepared them in an adjoining room. The last one made, I went in the room where he was. I had noticed a minute or so before that his moaning had

ceased. The room was full of persons, some of whom were applying sinapisms. I approached near enough to observe him as they were making the applications, for convenience in which they placed him cross-wise on the bed, his head being towards the wall, against which the bed was placed. I looked at him for some time. He seemed very calm and I watched to see him breathe, but could not. All this must have taken up the space, at least, of more than a minute. I then told those at the bed side to get away, and I took hold of his wrist to which the plaster had not yet been applied. The pulse was gone. I then felt the temporal artery, but could not feel it pulsate, he still not breathing the while. Then said I to those around "he is dead." I now straightened him out in bed, placing him lengthwise in it. He was as limber as a wet rag. I reëxamined the pulse, but could discover none. A thought of the "Ready Method" passed through my mind, and I at once, without any explanation, put it in execution. I continued my efforts as directed by Marshall Hall for at least four or five minutes—bystanders thought longer. Then thinking that those around would consider it foolishness to be thus handling a dead man, I was about desisting, and said, "poor fellow, he is dead; I don't believe I can do anything for him," still, however, continuing the turning, etc., when a man at my elbow, who seemed to understand my efforts, said "Doctor, keep on a while longer, don't qu. Somewhat encouraged, I still continued the turning. In something over a minute more he made a feeble inspiration, while in the supine position. I continued the turning and in a few seconds this was succeeded by another of considerable depth. They became more frequent as I continued the operation, and in the course of about ten minutes the breathing was fully reëstablished, and the senses had returned. He spoke a few words in answer to questions loudly propounded, and moaned piteously. I got him to swallow some brandy. The pulse, which was very feeble when I left off turning, became more full, and gained strength. I now had the sinapisms applied to to the forearms. All seemed to go on well for about half an hour, when the breathing became suddenly feeble and frequent, and in less than a minute ceased altogether. I did not take time to feel his pulse, but began immediately the posturing. At the least calculation five minutes must have elapsed before my efforts were rewarded by his gasping feebly while supine. This was soon followed by

others, and the breathing, the posturing being continued, was gradually reëstablished with partial return of the senses and evidently of great suffering.

To cut a long, and to me astonishing story short, he *died* and was brought to life *six times.* I say died, because I know no other word to express his condition. It sounds badly I know, but I use it for want of a better word. If I had left him when I said he was dead first, no one would have doubted that he "died." I know that he would have been buried the next day. So say twenty witnesses who saw what took place. But to return. Until he had "died" the fourth time, I had not the faintest hope of his ultimate recovery. It then occurred to me that if he could, by any means, be kept alive during the night, or until reäction should take place, that he might get well. I now determined to try electricity with, I confess, no very clear idea of how it was going to effect any good, and, in the absence of anything better, I had procured in a few minutes, a plate electrical machine. For this, and much kind assistance, I am indebted to Prof. Samuel Stevenson, of the Young Ladies' Institute. A Leyden jar was charged, and the contents passed through myself and the patient. The charge was a moderate one, yet lightning could have had no more instantaneous effect—the breathing stopped at once. The machine was immediately put aside, and "Marshall Hall's method" resumed. In about four minutes, it was supposed, an inspiration took place; this was followed by others, and the breathing was for the fifth time, restored. Still regarding the case as one of pernicious fever, I had been thinking all the while about giving quinine, but had thus far foregone doing so. I determined now to do so at once, and accordingly fourteen grains of the di-sulphate were given at a dose. My father, who is a physician, and was distant one mile, was now sent for. Before he arrived the breathing again ceased—the sixth and last time. I did as I had done before, and persisting, even after we had all given up hope, was rewarded with like success. It most certainly was eight minutes from the time he ceased to breathe until he made the first effort at inspiration. It was a very feeble one, and it was a long time before the breathing was satisfactorily reëstablished. I was completely tired out.

My father now arrived, and I told him what had taken place, and what I had done. On examining him now we found the pupils some-

what contracted, and insensible to light. The light of a candle, however, seemed to hurt his eyes, from which tears flowed freely, though the pupils did not obey the stimulus. My father regarded the case as one of the form of *coup de soleil* named by Dr. Bennet Dowler, of New Orleans, *solar exhaustion.* He proposed that he should take fifteen drops of tinc. opii. and have sinapism on the back of the neck. Both were immediately complied with. I had sent and got my apparatus intending to cup him, but had deferred doing so. It was now, however, thought proper, and, as preparatory, to shave the temples. He seemed to be getting better, and wanted to know what was going to be done. When told, and I ready to do so he objected stoutly and joeosely. He did not like the looks of the scarificator; he had seen persons cut with them, and thought he could get along without it. Said he wanted to go to sleep. After much persuasion he still objected, and as he seemed so much better, it was concluded not to cup him.

He was soon in a sound sleep. This was about 3 o'clock, A. M. I remained with him until 5, when I left him still asleep and doing well.

At six o'clock, he began taking the disulphate of quinine in four grain doses every hour. This he continued until 12, M., after which time, he took two grains every hour until night. He also took, during the day, some fluid extract of senna to move the bowels. He slept nearly all day, and generally had to be waked to take his medicine. At bed time he took eight grains of blue pill and six grains of quinine. He slept well during the night, and when he awoke, generally called for water. This he also did the previous day.

On Thursday he took two grains of the disulphate of quinine every hour during the day and until bed time at night. On the Sunday following, I met him on the street well. There was a complete blank in his memory from the time he ate his supper on Tuesday evening, until the following morning.

Remarks.—Whether the foregoing case was one of pernicious fever, as I regarded it, or one of *solar exhaustion,* or whether it was either one, I leave to the readers of this paper to determine. It may not have been either one. Possibly it was a complication of both. To describe the various forms in which this cousin-german of Proteus,

pernicious fever, may appear, would transcend the contemplated limits of this article.

These remarks are appended mainly for the purpose of offering a suggestion in the treatment of *ictus solis*, *coup de soleil*, or sun-stroke. All standard works, so far as I know, are meagre as to information on this subject. The only paper on this malady of any value, in my estimation, and that is a really valuable one, is from the pen of Dr. Bennet Dowler, of New Orleans. This may be found in volume xii of the New Orleans Medical and Surgical Journal, page 474, *et seq.*, to which I beg to refer the reader. Dr. Dowler in his pathological investigations on this disease, has shown no lack of that indefatigable industry which characterizes all his labors. The result of his inquiries prove conclusively that the theory which regards *ictus solis* as a cerebral apoplexy, is an erroneous one, and that the organic lesions are found in the lungs.

In regard to the cause of death, Dr. Dowler says, "Be it what it may, the cause of death begins, continues and ends in the breathing apparatus." But the most remarkable fact which his investigations have developed, is the post mortem circulation of the blood. He says, "after the death of the lungs, or the cessation of respiration, the heart and arteries will, in some instances, continue to act." Again he says, "Mr. C. died of solar asphyxia on the evening of the 24th of July, 1836. About an hour after he had been laid out, two messengers called upon me to visit the corpse which was supposed to be alive. I found the body as warm as at death, though it had since been washed. I found a slight pulsation at the wrist, and a feeble motion of the heart."

The suggestion which I set out to offer is, the application of the "ready method" of Marshall Hall to the treatment of *coup de soleil* in its worst form, *i. e.*, the *solar asphyxia* of Bennet Dowler. The remedy is so simple and easy of application, under all circumstances, that it seems unnecessary to urge its trial. An argument in its favor, however, may be found in the fact, that so far, no other remedy has effected any good.

There is one important fact, I admit, as noticed by Dr. Dowler that would rather lead to the conclusion that this means must fail. From his observations, it would seem that congestion or hæmorrhagic infiltration of the lungs, always present, precedes death. But from other

important facts noticed by the observer, may it not be possible that, in some cases at least, this condition is an effect of the post mortem circulation or exudation of the blood? Does it not seem probable that the "ready method" of Marshall Hall would have restored to life the man seen by Dr. D., in whom, an hour after death, there was a slight pulsation at the wrist and a feeble motion of the heart?

ARKADELPHIA, September 25, 1858.

PROGRESS OF MEDICINE.

ART. I.—*Pharmacy and Materia Medica.*

Ammonio-ferric Alum.—Prof. Wm. Proctor, Jr., in the March No. (1858) of the *Am. Jour. of Phar.* says:

In the Am. Journ. of Pharmacy for 1856, pages 305 and 478, I made some remarks, introducing to notice the "*Ammonio-ferric Alum*," or sulphate of ammonia and of sesquioxide of iron with a formula for its preparation. This combination having been since found by many physicians to answer the description given by Dr. W. Tyler Smith, of its advantages as an astringent tonic, the demand for it has occasioned the necessity for repeatedly preparing it; and, profiting by the suggestions of some of my friends, I have improved the process for its preparation, so as to make it much more readily, perfectly and economically, thus:

℞.	Ferri sulphatis cryst.	℥xxiv.
	Ammoniæ sulphatis,	℥xiss.
	Acidi sulphurici,	f.℥v. f.℥v.
	Acidi nitrici,	f.℥iij. vel q. s.
	Aquæ,	q. s.

Mix f.℥xxj. of the sulphuric acid, in a *large* mortar, with the sulphate of iron coarsely powdered; then gradually add, with trituration, the nitric acid, till it ceases to produce effervescence. Transfer the mixture to a porcelain capsule, and boil it with one quart of

water, added in two or three portions successively. Then add the remaining f.℥iij. of sulphuric acid, and the sulphate of ammonia; boil till the latter is entirely dissolved, and set aside in a cool place to crystallize. If the resulting crystals are not sufficiently pure and violet-colored, they must be re-dissolved by boiling in about a pint of water, acidulated with an ounce or two of sulphuric acid, filtered or decanted, and again set aside to crystallize. The crystals must then be drained, and dried in bibulous paper, before being bottled up. In this way we obtain very handsome, somewhat amethystine crystals.

Syrup of Protocarbonate of Iron.—The facility with which protocarbonate of iron dissolves in organic acids, and its perfect harmlessness in irritable subjects, render it one of the most valuable agents in therapeutics; accordingly all the new preparations into which sugar has been introduced, for the purpose of giving stability to this saline compound, have been adopted in practice.

M. Dannecy, a distinguished pharmacien in Bordeaux, having ascertained that the precipitate of protocarbonate of iron, obtained by mixing sweetened and boiled solutions of carbonate of soda and of protosulphate of iron, possesses the singular property of dissolving in simple syrup without becoming colored, conceived the idea of thus preparing a new ferruginous syrup.

This preparation, being permanent, will be employed in cases in which the form of syrup is preferable to that of pills; for example, in the treatment of children.

The following is the process for making M. Dannecy's new preparation: Take of purified protosulphate of iron, two ounces; distilled water, sixteen ounces; white sugar, two ounces; dissolve with ebullition, and filter. Secondly, take of crystallized carbonate of soda two and a half ounces; distilled water, sixteen ounces; white sugar two ounces; dissolve with ebullition, and filter. When the two solutions have cooled, mix them in a glass vessel, and shake for a moment; a precipitate is formed, which is at first white, but soon becomes of a greenish-grey color, preserving this shade. Allow this precipitate to collect during twenty-four hours; decant. Afterwards take a solution of sugar, in the following proportions: white sugar, two and a half ounces; distilled water, ten ounces; dissolve with ebullition, and filter. Add the precipitate to this saccharine fluid when cold; set it aside to rest; decant. Repeat this process once more, in order to remove the sulphate of soda resulting from the double decomposition. This washing ought to be accomplished as quickly as possible, to prevent the unnecessary solution of the ferruginous precipitate. Subsequently, agitate this precipitate, from time to time, in a fresh portion of saccharine solution (water, ten ounces; sugar, two and a half ounces). It will dissolve in the course of some days. Lastly, take of white sugar thirty-eight and a half ounces; distilled water nineteen ounces; add the saccharine ferruginous solution, and boil to specific gravity 1.262, at the temperature of ebullition; flavor with tincture of lemon and orange. The product will be sixty-four ounces of almost colorless and perfectly clear syrup of pro-

tocarbonate of iron, containing 9.90 per cent. of oxide of iron.—*Bulletin Gén. de Thérap.*

[It is evident that if the syrup above described really possesses the stability and composition assigned to it, it must prove a most valuable preparation.]

Glycerine a proposed substitute for Oils and Fats in Ointments,—Mr. G. F. Schacht proposes to substitute for oils and fats in ointments a "plasma," prepared by mixing one fluid ounce of glycerine with seventy grains of starch powder, and heating the mixture gradually to about 240° F., constantly stirring. There appear, however, to be numerous objections to the proposed substitution, among the principal of which are, the difficulty of procuring a sufficient supply of *pure* glycerine, the liability of the "plasma" to absorb moisture, and lose its original consistence, and the questionable effects of starch upon the skin in certain cases.—*Pharm. Jour.*

Iodide of Lime.—This preparation, made with one part of iodine and seven of lime, is recommended by Dr. Pidduck as superior to the iodide of potassium—in the comparative smallness of its dose; in its ready combination with the blood and tissues, manifested by its alterative effects; in not passing so quickly through the kidneys; in not producing gastro-enteric and vesical irritation; and in being nearly tasteless. Dr. Pidduck uses a solution of one drachm (containing eight and a half grains of iodine) in a pint of boiling water, which, when cold and filtered, is colorless and transparent. The iodine is found to exist in the solution in the form of iodide of calcium and iodate of lime.—*Ibid.*

New Preparation of Bark—Quinium—(the Alcoholic Extract with Lime of M. A. Labarraque, of Havre).—The objects of the investigations of MM. Delondre and A. Labarraque, which have led to the adoption of the above preparation, were—

1. To find a preparation admitting of the use of all the cinchonas which contain, at the same time, quina and cinchonia in considerable proportion, and these are the most numerous. M. Soubeiran, in his course of pharmacology, observes that the association of cinchonia with quina presents, in many cases, important advantages, and that the two febrifuge bases are complementary to one another in a therapeutic point of view.

2. To obtain uniformity in the product by an easy and strict proportionment of the febrifuge alkaloids, thus practically applying, in the most useful manner, the discovery of Pelletier and Caventou.

3. To preserve all the useful products of the cinchonas by removing only the inert matters which interfere with the easy absorption of the active principles, and oppress the digestive organs.

4. To establish a proportion of quina and cinchonia, similar to that found in the cinchona, which the experience of all ages has shown to be the most efficacious, the bright red cinchona, which is at present scarcely employed, on account of its very high price.

5. To simplify operations, so that nothing may be lost, and so as to afford the best febrifuge at the lowest possible price.

The question of price is, in fact, very important in dealing with a dear medicine, the use of which ought to be continued, and which is most frequently necessary for the poorest country laborers.

The following is M. Labarraque's formula, as it has been adopted by the Académie de Médecine, and entered in their Bulletin.

Formula for the alcoholic extract of cinchona by means of lime.—Take cinchona barks of known composition; mix them in such quantities that the quina may be present, relatively to the cinchonia, in the proportion of two parts of the former to one of the latter.

Pound the barks; mix the powder with half its weight of slaked lime; heat the mixed powder with boiling alcohol until the barks are exhausted; collect the greater part of the alcohol by distillation; complete the evaporation. The residue is the alcoholic extract of cinchona by means of lime.

Seventy grains of this extract ought to yield, by the ordinary processes, fifteen and a half grains of sulphate of quina, and nearly eight grains of sulphate of cinchonia.

Pills of Quinium.—Two and half grains of quinium in a pill represent one-third of their weight of febrifuge alkaloid. Thirty of these pills, which are sold for about fifteen pence, suffice in the majority of cases for the cure of an intermittent fever; from five to ten are given in the twenty-hours, as far as possible from the approaching paroxysm. Half a glass of wine is taken after each dose.

Wine of Quinium.—This wine, which may be of great use as a tonic, as a febrifuge, and in preventing the return of obstinate intermittent fevers, is prepared by M. A. Labarraque by dissolving seventy grains of quinium in twelve times its weight of alcohol, adding thirty-five ounces of good white wine, and filtering. The wine contains about twenty-three grains of the alkaloid in thirty-two ounces; the dose is from an ounce and a half to three ounces as a tonic, and from three to six ounces as a febrifuge.

The following are M. Bouchardat's observations in his "Traité de Thérapeutique et de Matière Médicale," on the comparative advantages of the sulphate of quina and quinium:

"Whenever it is necessary to cut short a paroxysm surely and quickly, sulphate of quina will always have the superiority over all the other preparations of cinchona; none of them, not quinium itself, can be compared to it for this marvelous power. For this reason no substitute is to be found for it when we have to deal with essential paroxysms. But when it is our object to cure a fever of long standing, surely and without shocks to the system, quinium resumes its supremacy.

"In treating intermittent fevers in an hospital or in a healthy locality, remote from the foci in which these fevers have originated, the expectant system alone (as M. Chomel had so well established for the hospitals in Paris, and as M. Lavern has verified in those of Blidah)

suffices in the great majority of cases; sulphate of quina is, under these circumstances, the most valuable adjuvant; it still shows in these special instances its incontestible superiority, in quickly and effectually relieving the patients from the intermittent fevers which tormented them.

"But it is when the patients remain in the localities, and under the conditions in which they have been attacked by the fever, that the remedy which subdues the disease without disturbing the system, in its turn resumes its superiority.

"It is in fever countries, in the midst of the causes which have given birth to the fevers, when these same causes persist, that all the advantages of quinium appear. Under such circumstances, M. Valu has administered it in Algiers, M. Hudellet in Dombes, and I myself in several fever localities in the department of l'Yonne."

To this first testimony borne by M. Bouchardat, we hope soon to add further information on the subject. Quinium is being largely tried in the cliniques of M. Trousseau, at the Hotel Dieu, and of M. Aran, at the Hôpital Saint Antoine, and these experiments will furnish the opportunity of returning to this important question.—*Bull. Gén. de Thérap.* and *Dub. Quart. Jour.*

Formula for the preparation of Anti-asthmatic Cigarettes, by M. Danneey, Pharmacien, of Bordeaux. Some of the properties of stramonium and belladonna—which plants, when smoked, justly enjoy the reputation of relieving asthma, and which are employed with the most undoubted success in the treatment of neuralgia—exist also in plants abounding in nitrates. Thus I have seen patients who had experienced great relief from the use of the leaves of borage and pellitory plants containing, as is well known, much nitrate of lime.

The fault which almost all patients find with narcotic plants, smoked in pipes or in the form of cigarettes, is a copious production of smoke, which fatigues them, and sometimes excites cough—a symptom they are, on the contrary, employed to allay.

In order to obviate this inconvenience, I have added nitre to the leaves of belladonna and of stramonium, by watering these plants, dried and conveniently spread out, with a solution of nitrate of potash, in the proportion of three ounces of the salt to rather more than two pounds avoirdupois of the plants. It will be easily understood, that as this solution penetrates the entire vegetable tissue, the latter will, when dry, burn completely, without the formation of the pyrogeneous products above alluded to.

I have for many years prepared cigarettes according to this formula, and the benefit derived from their use by a great number of patients induces me to publish it, and to call the attention of practitioners to this mode of treatment, consisting in the smoking of narcotic plants combined with nitre.

New Caustic Pencil, modifying the tissues in different degrees—Double Salt of Nitrate of Silver and of Soda.—M. Brun, in the *Gazette Médicale de Lyon,* proposes the use of the above double salt, in varying proportions, in order to obtain the modifying, in contradistinction to the

caustic or destructive, effects of nitrate of silver upon the tissues, in cases in which it is necessary to confine this action to a circumscribed point—as, for example, in chronic inflammation of the urethra, where the disease is localized—as in irritations of the prostatic region, in spermatorrhœa, neuralgia of the neck of the bladder, etc.—in which injections would irritate the surrounding sound parts, and consequently dispose to an extension of the evil. For this purpose he has had pencils prepared of nitrate of soda and nitrate of silver, in varying proportions.

The following is the process adopted by M. Livernay, pharmacien, in the manufacture of the pencils: Dissolve, on the one hand a portion of nitrate of silver in a sufficient quantity of distilled water; on the other, a portion of nitrate of soda in a like quantity of water; mix these two solutions, evaporate them to dryness. Place the residue in a capsule, and heat until it be fused; then pour the product into the mould, previously warmed and greased. When cold, a cylinder is obtained, representing a double salt of nitrate of silver and of soda.

This double salt is of a pearly-grey color, if each of the salts employed was pure; it acquires a brown and blackish tint, if the fusion was too long continued, whether in consequence of the reduction of a small quantity of silver, or of the action of the fatty matter with which the mould was smeared.

The nitrate of silver and soda presents a crystalline and radiated fracture. It is very soluble in water and in boiling water. It is not more brittle than nitrate of silver, and it is more easily cast. Generally speaking, it presents the same characters.

These two salts may be combined in the most varied proportions.

M. Brun has used this double salt, with much advantage for touching apthæ of the mouth, for smearing the glans, in cases of balanitis, and for applying to a localised inflammation of the lower eyelid. He sums up its advantages in the following propositions:

1. It better fulfills the object of the practitioner, where only a modification of the tissues is desired.
2. Its action may be increased or diminished, by varying the relative proportion of the two component salts.
3. It is more easily manipulated.
4. In no case is its use attended with danger.—*Bul. Gén. de Thérap., Dub. Hosp. Gaz.* March 1st, 1858.

On the Value of the Iodide of Iron.—(Journal des Cliniques des Hôpitaux de Paris, May 10, 1858.)—The iodide of iron is comparatively a new preparation, as it was only in 1834 that Dupasquier, of Lyons, distinguished no less as a chemist than as a physician, made some very interesting trials of this preparation in the treatment of pulmonary phthisis, and proposed a new method of preparing it. He then showed that he had performed several cures upon patients affected with crude pulmonary tubercles, and had considerably relieved others whose tubercles had begun to soften. In the hands of others, however, the use of iodide of iron has not been attended with uniformly good results, probably in consequence of the uncertain nature of the

preparation; sometimes too much of the iodine was received into the stomach, and besides this, there was occasionally an excess of iodine present, which was converted in the system into hydriodic acid. In recent times, M. Gille has proposed to administer the iodine in the form of sugar-plums, and in the formula recommended it is said that the proportions of the iron and the iodine are preserved unaltered. M. Boinet records two cases of abscess of a very aggravated character cured by the internal use of iodide of iron, together with iodu-retted injections into the sac of the abscess. The first case was that of a child, nine years old, in whom there was caries of the fourth, fifth, and sixth dorsal vertebræ; and there were two abscesses, one in the back, on a level with the diseased vertebræ; and the other, also on the right side, in the iliac region. The disease had continued for several months without any amelioration, when, on the 8th of January, 1857, the abscess in the iliac fossa was punctured and injected with tincture of iodine; the patient was put upon good diet, and cod-liver oil was given internally, together with sugar-plums *(dragées)* of the iodide of iron. On the 17th of January both the abscesses were punctured and injected at the same time. This operation was subsequently repeated on several occasions, and under this treatment the fistulous openings caused by the punctures gradually dried up, and at last became completely cicatrized. The child improved in all respects, and under the use of the iodide of iron in sugar-plums, and the influence of strengthening food, became strong and well. The second case was that of a man aged sixty-three, under the care of M. Malgaigne, at the Hôpital St. Louis, who had a large abscess below the crural ligament. M. Boinet punctured this abscess, which discharged more than three pints of a grumous pus. The opening was then injected with tincture of iodine ; the abscess remained fistulous for some time, and allowed a small quantity of pus to flow out every day, but it soon closed. The iodide of iron was administered internally, and the patient was put upon a strengthening diet; and six weeks after the operation and the internal use of iodide, the patient left the hospital quite well.

In a lecture on chlorosis by M. Gendrin, the lecturer recommends iron and manganese as the best remedies in this disease, and passes in review the different preparations of iron which are employed in medicine. One of the most useful preparations in cases of chlorosis complicated with scrofula, or only with a lymphatic temperament, is the iodide of iron. But, unfortunately, this salt is very easily decomposed, and in order to employ the syrup with advantage, it should be prepared at the very time when it is to be taken. The invention of M. Gille fulfills the object of practitioners in recommending this medicine, for he envelopes the iodide with a layer of sugar, which altogether prevents the access of air. These sugar-plums have been preserved more than two years without any alteration of the iodide. M. Gendrin speaks strongly in favor of this preparation, the value of which consists not only in the indefinite preservation of the ferruginous salt, but also because it renders its administration easy and agreeable. The iodide of iron has the advantage over other prepara-

tions, of being well borne by the patient, an advantage which it undoubtedly owes to its great solubility.

M. Rostan, in a lecture on chlorosis, says that the iodide of iron possesses, in the promptitude of its curative action, a very manifest advantage over the other ferruginous compounds; the syrup of iodide has not been used in medicine so much as it would have been if it had more stability; but the preparation of M. Gille renders the administration of this salt as easy as that of the oxide or the carbonate.

In phthisis, the iodide of iron has been found to act very beneficially, and its importance in this disease will be increased when it is preserved chemically pure, and given in suitable doses. Louis, Andral, and Bricheteau, have all used with success the syrup of the iodide of iron in pulmonary tubercle; and more lately Dr. Belouino has published some cures of this disease effected by the administration of the sugar-plums of M. Gille. "Recent observations," says Dr. Belouino, "have assigned to iodide of iron an important place among therapeutic agents, and it may be boldly placed among the best medicines which we possess. Formerly it was unworthy of confidence, because it was badly preserved and was easily decomposed, and consequently did not give always identical results. Physicians, in consequence, declined to make use of it. At present the iodide of iron—thanks to the laborious researches of M. Gille—is preserved in a state of perfect purity. I have had occasion to employ very often the preparations of this gentleman, and I have attained experimentally the conviction that the iodide of iron is an excellent medicine in cases of anæmia, scrofula, rachitis, chlorosis, and often in certain cases of pulmonary phthisis in which the organism requires to be strongly fortified." Dr. Belouino records two cases of phthisis which were cured by the administration of iodide of iron; one was the case of a lady, aged twenty-four; the other that of a child, aged five; in both the existence of tubercular disease was ascertained, but it disappeared under the use of the iodide.—*B. and F. Med. Chir. Rev.*, July, 1858.

Art. II.—*Cure of Aneurism by Digital Compression.*

[At present, surgery not less than physic, tends to conservatism. In both cases, this tendency is due to the progress of discovery in or an accurate knowledge of physiology and pathology unknown to the past. This conservatism tends to bring within the domain of the physician, certain morbid affections and injuries hitherto consigned to operative surgery. The surgeon is not less the surgeon, because he seeks to merge his art, as far as possible, in that of the physician. The *éclat* he might acquire from the public for a brilliant operation is sacrificed for the good of the patient and to sense of duty. Of the propriety of

an operation, the public can seldom know much, but its skillful performance, whether called for by the exigencies of the case or not, almost always enhances the reputation of the operator much more than a tedious course of medication how skillful soever that may be conducted.

To cut down upon and tie an artery in order to cure an aneurism, or to cure the latter by merely compressing the vessel with the fingers, must impress the public mind very differently. The digital compression, and the ligation of an artery, in their *modus operandi* agree; both arrest the impulse and supply of blood into the sack; both favor the formation of a coagulum of blood or a plastic concretion on the proximal side of the force or ligature. Thus the calibre of the artery is obliterated or occluded by an organized fibrinous plasma effused within or without the vessel, after which, neither compression nor the ligature can be otherwise than hurtful. If digital compression prove successful, it has the great advantage of being devoid of the serious dangers attendant on the ligation and the wound necessarily made in the latter process.

Instrumental compression, which for more than a decennium has been growing in favor, particularly in the city of Dublin, has happily effected a number of cures. This method, tedious and painful in itself, is generally unsuccessful and consequently unsatisfactory.

While the practical surgeon may view with more or less distrust the efficiency of digital compression for the cure of most cases of aneurism whether idiopathic or traumatic, yet if one case in a hundred can be cured by this method, which is free from danger, it ought to be tried before resorting to the knife. The latter method virtually discovered and directed by the genius of Hunter, so accordant with anatomy, physiology, and pathology, is a discovery of modern times tested by two generations, and deserves to be ranked with Harvey's discovery of the circulation—Jenner's of vaccination—a triune glory for Britain before which crowns and coronets "pale their ineffectual fires."

Before proceeding to reproduce cases of cure by digital compression, I will give a translation of a very remarkable example of cure by instrumental compression, recently published.

The *Journal de Médecine* de Bordeaux, for July, 1858, gives the following history of a case which occurred in the service of Dr. Denucé; reported by M. Rabaine.

Aneurism of the Brachial Artery, cured by compression in two hours and a quarter: A man, aged about 40, entered the hospital July 23d, 1857, to be treated for a tumor developed upon the anterior and inner aspect of the arm, at the junction of its superior third with the two inferior thirds.

The patient was a vine-dresser, accustomed to hard work, but had not hitherto received any blow or wound in this member. This tumor, nearly three years ago, began to show itself; it was then as large as a small filbert. Little by little it grew to the size of a small egg of the hen. It had for a considerable time caused a stricture or cramp in the arm, with neuralgic pains propagated to the hand, which continued to increase. The pulsations of the tumor, visible to the eye and very strongly perceptible to the touch, had also a very strong *bruit de souffle;* the beatings ceased during pressure of the artery above the tumor in the axilla. There could be no doubt as to the nature of the affection.

Compression was immediately adopted by means of M. Broca's apparatus, *for two hours and a quarter*, when it was discontinued, the pain having become insupportable. During the evening of the same day, the arterial beatings did not return; but on the next day, they reäppeared though very slightly. The patient would not submit to the reäpplication of the first mentioned apparatus.

The compressor of J. L.-Petit was now resorted to, but unsuccessfully, as the patient was refractory, and had probably relaxed the pressure of the instrument. The aneurismal beatings, though feeble, still persisted, when the patient left the hospital on the 8th of August, 1857. On the 25th of this month he returned, *cured*. For eight days before his return, there had been no beatings in the tumor, which was firm and reduced in size; the pulse at the wrist was scarcely discernible.

This man was repeatedly examined subsequently, and when seen early in June, 1858, was still well. The tumor continued to decrease progressively, being now the size of a large pea and very hard. In the meantime, the man had resumed his laborious occupation.

His aneurism was actually cured by compression for *two hours and a quarter*, though this definitive result was not fully obtained until after the lapse of several days. This, however, is by no means the only case of the kind, as many others may be found in M. Broca's *tableaux*.—(*Traité de l'Anéurysme.*)—B. Dowler.]

An Historical Critique on the Cure of Popliteal Aneurisms by Digital Compression; with reference to several communications made to the Société de Chirurgie. Drawn up by AR. VERNEUIL, Fellow of the Faculty; Surgeon to the Hospitals.—Since the publication of the valuable work of our excellent friend, M. Broca,* the question of the treatment of aneurisms has been reöpened, and compression has resumed a preëminence it ought never to have lost. Perhaps this is, to a certain extent, to be attributed to the legitimate crusade waged with so much ardor against the surgery of the knife. However this may be, the Société de Chirurgie has latterly had the good fortune to receive three reports of aneurisms cured by compression performed with the fingers.

We had lately announced the communication of M. Vanzetti, of Padua; at the same time, a work by M. Michaux, of Louvain, containing a similar case, reached the Society; so that we shall, without further delay, proceed to bring the subject before our readers.

With reference, first, to the new facts. It occurred to M. Vanzetti, who had seen compression employed in Dublin, in 1843, to substitute, for the mechanical means there used, the hand of several successive assistants. In 1846 he made a trial of this mode, at the Hospital of Kharkoff, in Russia, conjointly with M. Serebraikoff, surgeon to the hospital. He himself instructed the persons intrusted with the maintenance of the compression, as to the place and manner in which it should be employed. The compression was kept up for two days, but without effect. The patient was operated on by ligature.

Eight years later, in 1854, M. Vanzetti received under his care, in hospital, at Padua, a mason, aged about thirty-six years, of tolerably good constitution, laboring under a well-marked, though medium-sized, popliteal aneurism.

The pulsation ceased as soon as the femoral artery was compressed. M. Vanzetti resolved to employ compression; but, discouraged by the failure just alluded to, he tried a great number of compressors, with much patience on his part, and much inconvenience to the sufferer. Before resorting to tying the femoral artery, he wished to try digital compression once more, in the hope that, under his immediate superintendence, it would prove more successful. Several assistants—seated or standing; sometimes with one hand, sometimes with both hands, placed one over the other—compressed the artery with a *very moderate force*, sufficient to bring the walls of the artery together, but without inconveniencing the patient. The situation chosen was the middle third of the thigh, in order that the profunda artery should not be obliterated.

At the end of twelve hours there was considerable diminution of the expansive movements of the tumor. At the end of forty-eight hours there was no pulsation, nor could any bruit be heard, and the compression was withdrawn. There was no relapse. The tumor in the ham gradually disappeared, and, at the same time, the leg became straight again. This man has resumed his occupation; he walks without limping, and experiences no inconvenience in the movements of the knee.

* "*Des Aneurismes et de leur Traitement.*" Paris: 1856.

The second case is still more remarkable. In 1855, a very intelligent officer, aged twenty-seven, consulted M. Vanzetti for a popliteal aneurism of the right side. The employment of digital compression was resolved on, but was deferred for twenty days During this period the patient himself made compression, the action of which had been explained to him. The tumor, which was of small volume, seemed to diminish even under the influence of these imperfect attempts.

The real treatment was commenced at noon, and was confided to six intelligent and reliable assistants. At six o'clock, M. Vanzetti returned to see what state things were in. The patient was fast asleep; not one of the assistants was with him. Great was the surgeon's surprise; greater still was his astonishment when he learned that after FOUR HOURS of careful compression, *the aneurism had become still*, and that its pulsations had completely ceased! Accordingly, at five o'clock—that is, an hour after—the assistants had withdrawn. The patient remained at the Clinique for a month. He was seen again several months later; there was no lameness, and the movements of the limb were free. The aneurismal tumor was converted into a solid nucleus, of the size of a filbert.

Such are the facts furnished by the Paduan surgeon, and which he has put forward with the most remarkable clearness. M. Marjolin, in his turn, has communicated two cases reported by M. Michaux, of Louvain, one of the most distinguished of foreign surgeons. The first is a long chirurgical drama, commencing with double and intermittent compression, and ending with amputation of the thigh, rendered necessary by gangrene of the limb, after ligature of the femoral artery. This case is one of great interest, but as it does not bear directly on the point under consideration, we shall not further allude to it. The details may be found in the Bulletins of the Society.

The following is a succinct analysis of the second case:

A man, aged 50, of good constitution, who had always enjoyed excellent health, complained, for the first time, in 1851, of rheumatic pains in the left thigh. In August, 1856, the patient discovered, at the anterior and inner part of the thigh, a pulsating tumor, situated nearly two and a half inches from the crural arch. Its progress was rapid, for in the month of November, it measured five and a half inches in its transverse, and four and a third inches in its vertical diameter. It presented very distinctly all the signs of aneurism. The complete examination of the patient denoted a general alteration of the arterial system; abnormal sounds towards the centre of the circulation, and a rather rough souffle in all the large arteries. The other functions were tolerably well performed.

What treatment was to be adopted? If ligature was to be employed, it should be applied to the external iliac, always a serious operation, especially when the vessel is altered. On the other hand, alternating compression with two pads was not compatible with the very high situation of the tumor. M. Michaux decided on making single compression on the ilio-pectineal eminence, with the small pad of M. Broca's apparatus. The treatment was commenced on the morning of

the 28th November. The compression was to be intermittent—that is to say, every four or five hours the apparatus was to be loosened, to allow the patient to rest. The next day the severity of the pain rendered it necessary to suspend the compression for four hours. On the 30th a vesicle had formed on the surface of the compressed skin; the pressure was again suspended for four hours. The pad was placed a little lower. Compression was alternately suspended and reëstablished until the 2nd of December, as the presence of vesicles indicated the formation of an eschar.

During these four days the pain had been extremely severe, especially in the loins and in the heel. There was complete want of sleep, notwithstanding the employment of opium. The patient was very much fatigued; and nevertheless the tumor had diminished in size, and had become hardened. On several occasions, when the permanent compression had been prolonged for four or five hours, the pulsations had disappeared. M. Michaux then thought of using digital compression. By good fortune, a portion of sound skin remained within the eschar, where the application of the finger was sufficient to arrest the pulsations. The pupils of the hospital applied themselves to the task with the greatest zeal, on the 5th of December, at half-past 9 in the morning; the compression was to be total; the pulsations of the artery being energetic, tolerably strong pressure was necessary to obstruct the vessel.

Compression was very painful on account of the vicinity of the eschar. In the afternoon, œdema, tingling, numbness, and rigidity of the limb were present; the feet became cold; there was complete want of sleep, as well as acute thirst, intense fever, and violent colic, at 2 o'clock in the morning; but the tumor became perceptibly solidified; the pulsations diminished; soft clots filled the sac. The local improvement continued without interruption; at 9 o'clock in the morning, twenty-four hours after the establishment of the digital compression, the pulsations and the bruit de souffle entirely ceased. The arteries no longer pulsated below the aneurism, but above it, on the contrary, the commencement of the femoral pulsated strongly.

As a matter of prudence, the compression was kept up until 2 o'clock in the morning; it was then interrupted for half an hour, to afford some respite to the unhappy patient, after which it was resumed until 9 o'clock in the morning, to be finally given up.

I stop here to impress more forcibly the mind of the reader, and I pass in silence over the details of the general treatment, which was very ably conducted during all this period, as well as during convalescence. On the 14th of December the eschar separated; it involved the entire thickness of skin. On the 16th of January the wound had cicatrized. On the 24th the patient left the hospital perfectly cured. The aneurismal tumor was very hard; it is still of a tolerably large size, (two inches by two and three-fourths), but the cure is no longer doubtful.

Did they stand alone, the remarkable cases just reported would suffice to commend digital compression strongly to the attention of surgeons. What objection can there be to a proceeding which cures

popliteal aneurisms in five hours—even in forty-eight hours; a considerable aneurism of the femoral in twenty-four hours? What, without digital compression, would have been the fate of M. Michaux's second patient? Let us observe that, in two cases, no resource remained but the ligature; the insufficency of compression by means of apparatus was palpable. If M. Broca had had these facts before him when he was preparing his book, he would certainly have modified the chapter he has devoted to the operative proceeding we are speaking of. Without tracing, step by step, the history of digital compression, we may be permitted here to discuss with our learned friend some of the propositions he has announced in his book.

While he considers digital compression to be "a very valuable resource," M. Broca regards it as a means which we must reserve for the following cases: 1. When the deviation of the limb does not permit us to act efficiently on the artery by means of mechanical apparatus. 2. When excessive irritability of the skin is opposed to every other species of compression.

Now, the latter condition alone existed, and in but one of the three cases above quoted. The two other cures show that it is advisable to enlarge the scope of the process. On further consulting the facts collected in M. Broca's book, I find another very remarkable case, that of Mr. Greatrex. The alternating employment of a compressor *and of the patient's fingers* put a stop to the pulsations at the end of twenty-four hours.

In a case of Mr. Knight's, compression with apparatus could not be borne for more than an hour. Forty hours' continuance of digital pressure on the pubis was sufficient to insure a cure.

To crown the work, we have the curious case of the patient under Mr. Colles's care, who cured himself by keeping up for seven days a digital compression, which was necessarily very imperfect.

It is far from being my intention to conceal the tolerably numerous failures of this process. But, on the whole, what have we lost when we have failed? Nothing; even when the means employed prove unsuccessful, as happens also occasionally with compression by apparatus. It appears to me that digital pressure has been less painful for the patient than the latter, which may then be advantageously substituted for it.

For my part, notwithstanding the constantly progressive improvements of apparatus, which, according to M. Broca, render cases very rare in which digital compression is indicated, I consider, with him, that it constitutes the type of the method, and that consequently it is only in extreme cases that we ought to draw back from the difficulties of its application. In common with M. Michaux, I can scarcely doubt that, in the immense majority of cases, we should be able to find the devoted coöperation of a sufficiently large number of assistants to take part in an enterprise as noble in a humane, as it is interesting in a scientific point of view. Hitherto digital compression has most frequently been employed only after mechanical compression. I think that, hereafter, we should proceed in exactly the inverse direction, and, when compression is indicated, commence with the pressure of the fingers before having recourse to a less gentle process.

Perhaps it will not be uninteresting to take a rapid survey of the cases of aneurism in which digital compression has been employed, whether alone or combined with mechanical pressure.

These trials, in number seventeen, include seven unsuccessful and ten successful cases. I shall add some explanatory remarks on the two categories.*

FAILURES.—*Four times* compression was employed primarily—that is to say, before any other means. A. Popliteal aneurism; two days' compression; ligature (Vanzetti). B. Popliteal aneurism; four hours only, then mechanical compression for six days (Jameson). C. Diffused aneurism of the femoral; sixty-two hours; apparent cure, which proved not permanent; seven days' compression with a weight on the groin; decisive cure (Parker). D. Popliteal aneurism; digital compression maintained by convalescent patients for three days; mechanical compression, previously intolerable, was rendered possible; a cure was obtained (Monro, J.)

In two instances, digital compression was not employed until after the insufficiency of apparatus had been demonstrated, and their use abandoned: E. Diffused aneurism of the ham; sixteen days of mechanical compression; ninety-four hours of digital compression, at the end of which the pulsations and the souffle had disappeared; the success was not permanent; amputation; death (Nélaton). F Popliteal aneurism; employment of bad apparatus for five days; twenty-four hours of digital compression; ligature; amputation; recovery (Norgate). G. In this instance, both modes of compression failed, but the case was one of inguinal aneurism; compression with the fingers was kept up during four days and four nights; the tumor was greatly ameliorated; the tourniquet having been applied, subsequently produced an eschar; the external iliac artery was tied (Fox).

In these seven cases of failure, we find three ligatures subsequently applied, A, F, G, and two amputations for gangrene, E, F. Two ligatures were successful, A, G. One of the two patients subjected to amputation died. Three times, on the contrary, B, C, D, mechanical compression was followed by cure. Even when digital compression had to be abandoned, it modified the tumor advantageously in two cases, C, E. In a third, D, it singularly promoted tolerance of the apparatus.

Let us proceed to the—

SUCCESSFUL CASES.—*Two* belong to digital compression, employed primarily and alone: H. Popliteal aneurism; five hours were sufficient (Vanzetti). I. Colles's patient, who, without assistance, cured himself in seven days of popliteal aneurism, by intermittent, and, of course, very imperfect compression.

Three times digital compression succeeded when mechanical compressors were inapplicable, or abandoned: J Femoral aneurism; mechanical compression for four hours; after twenty-four hours digital

* In M Broca's work details relating to these facts will be found. I have given only a very brief analysis of them, distinguishing the cases by letters and the names of the observers.

compression produced a cure (Michaux). K. Popliteal aneurism; various apparatus were tried for a long time, and were given up; forty hours of digital compression (Vanzetti). L. Apparatus intolerable; forty hours afterwards, the employment of the fingers; cure (Knight).

Four times the pressure of the fingers having been combined with the use of the tourniquet, it is difficult to award to each method its due share of merit. This method, which is equivalent to double and alternating compression, has further produced results so rapid and decisive that, perhaps, we ought to dwell on this combination, which would singularly relieve at once both the patient and the surgeon.

M. Popliteal aneurism; tourniquet below the groin; the patient's finger pressing on the pubis; at the end of twenty-four hours, cure (Greatrex) N. Popliteal aneurism; digital compression on the pubis; below, Dupuytren's compressor; alternating action, forty-eight hours; recovery. O. Same lesion; same mode of proceeding; sixteen hours were sufficient for the cure (Wood). P. The following case by Mr. Tufnell, is more complicated; in it, however, we find a combination of several modes of compression. Very voluminous popliteal aneurism; compression, according to Bellingham's process, for two days; suspended on account of swelling of the inguinal glands; digital compression on the pubis by the patient and a neighbor; tourniquet acting inferiorly, and alternating with the action of the fingers; the whole kept up for twenty-four hours; the pubic weight† was then employed; cure at the end of seven days.

Lastly, in the following case, digital compression having been employed conjointly and alternately with apparatus and the direct compression of the tumor, it is difficult to assign to it a precise part in the case Q. Arterio-venous aneurism of the bend of the elbow; direct compression on the tumor for several days, then indirect compression with the tourniquet, which could not be borne; digital compression kept up by the patient, and suspension of every other means; resumption of direct compression on the tumor; digital compression was made by assistants for twenty hours, and then abandoned; it was intrusted to the care of the patient alone; finally, some days after, the aneurism was cured (Nélaton).

We should have more than one remark to make on all these interesting cases, and, in particular, on the pl ce where it is advisable to apply the fingers in cases of popliteal aneurism. Perhaps we should adopt the situation chosen by M. Vanzetti; but this discussion would carry us too far.

In the present state of the question, we may safely announce the following propositions:

I. Indirect digital compression, continuous, and even intermittent, performed by the able hands of assistants, or by the patient, has succeeded alone, and without the previous or subsequent assistance of any other means in curing aneurisms.

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† A conical weight of lead, varying in amount from four to nine pounds, as may be necessary efficiently to compress the artery, where it is superficially situated in the groin. See "Practical Remarks on the Treatment of Aneurism by Compression," by Jolliffe Tufnell, M.R.I.A., etc. Dublin: Fannin & Co. 1851.—Translator.

II. Associated with the tourniquet, and alternating with it, it has produced cures both rapid and extremely simple. In general, when success is to crown the attempt, it does not tarry.

III. Employed alone, it has cured aneurism when mechanical compression was impracticable, or had to be abandoned; much better borne, in fact, than the latter; digital compression may be applied on points where the skin is already inflamed.

IV. "This compression is the most efficacious and least dangerous of all; it enables us to act only on the artery, avoiding the nerves and neighboring veins, and sparing the skin."—*Broca*, p. 807.

V. Digital compression may fail; but in this case it most frequently modifies advantageously the state of the aneurism.

VI. We are justified in believing that, alone, it would have succeeded more frequently if it had been practised with more perseverance and regularity than were employed in the above quoted cases.

VII. Never, so far, has any accident been attributable to this proceeding.

VIII. Having been applied successfully, for the first time, by Saviard, subsequently to an operation for aneurism after the old plan, indirect digital compression is essentially of French origin. Hitherto it has not received all the extension and generalization of which, in my opinion, it is susceptible.—*Gaz. Heb. Dub. Quar. Med. Jour. Dub. Hosp. Gaz. May* 15, 1858.

Clinical Reports of Surgical Cases. By SAMUEL A. CUSACK, F.R.C.S., M.R.I.A., Resident Surgeon, and Lecturer on Anatomy and Physiology.—*Case of Popliteal Aneurism cured by pressure in twenty-six and a half hours.*—William Connolly, aged 38, was admitted in Madam Stevens' Ward, under the care of Mr. Wilmot, on the 27th October, 1857. He is a tall, thin-looking man, of pale complexion, but of good general health. He has lived temperately, has not had syphilis, and the heart and arteries, with the exception of the right popliteal, are free from disease.

Nine weeks before admission, while shoeing a horse, he received a severe strain in the right leg, from the animal making a sudden movement while the hoof was against his knee. He was thrown to the ground by the blow, but was able to go on with his work presently, and did not think much of the injury until the next day, when, after walking a mile, he found that the right knee had become stiff, and that there was a swelling behind the joint.

He continued to walk about for two days more, when a pulsating pain set in in the part, which obliged him to keep to his bed for a month, by the end of which time, this pain and the swelling had so far subsided, that he was able to walk about a little, but could not take any active exercise, or return to his trade.

Up to the period of his admission there has been but little change. His chief complaint now is, that any attempt at walking, or moving the limb, is followed by increased pain and swelling. He sleeps well at night, but feels a little pain in the day; otherwise he appears in excellent health. His pulse is 82, and very tranquil. The tumor lies

quite at the lower end of the popliteal space, being covered up by the soft parts; so that at first sight it looks like a tumor in the muscles of the calf of the leg. On careful examination, however, the whole of the upper portion of the calf may be observed to pulsate, and a very distinct bruit is evident when the ear is applied. The tumor itself, as felt through the soft parts, may be estimated at the size of an orange; it is of a fusiform shape, and probably engages the bifurcation of the popliteal artery. It can be nearly emptied by direct pressure. He is a spare man, and the artery can be easily compressed in the groin, so as to stop all pulsation in the tumor. The foot is slightly œdematous.

Treatment.—On admission he was ordered small doses of the tincture of digitalis (gtt x. every six hours) in camphor mixture; he was also put on the low diet of the hospital, and advised to use but little fluid.

October 30, 8, P. M.—A laden weight, of a conical shape, and weighing five pounds and a-half, having been placed on the femoral artery in the groin, it was found that there was scarcely any pulsation in the tumor, and that he could bear this amount of pressure without much inconvenience. He was ordered a draught containing twenty drops of the liquor morphiæ, and it was hoped that he would be able to sleep with the pressure on. He was unable to sleep, but kept the pressure steadily on, except for about two hours, until half-past seven the next morning, when it was removed, to allow him to sleep, having been on nine hours and a half. At half-past nine, A. M., (Oct. 31), the weight was re-applied until 9, P. M., making in all twenty-one hours' pressure out of the twenty-five. The tumor had by this time become much more solid, and the anastomosing vessels on its surface remarkably distinct; the leg a little more œdematous than before; the foot kept warm by a water-bottle.

November 1, 10½, A. M.—He has had a good night's rest. There is a small gland, just where the pressure had been applied, which is tender; this, however, could be drawn to one side with the integuments, and the pressure was re-applied, having been increased to eight pounds, so as to stop all appreciable pulsation in the tumor. At 4, P. M., on removing the weight, which had been on this occasion applied five and a-half hours (making in all twenty-six and a-half hours) it was found that the pulsation and bruit did not return. The limb was warm, and he had no pain anywhere; but the tumor felt hot and tender. He was directed to keep the pressure on for six hours longer, to make sure of a perfect cure. At 10, P. M., all pressure discontinued; "repeat morphia draught."

October 4.—No change since last report. The limb is maintained in a semi-flexed position on a pillow, and he is kept slightly under the influence of opium. The surface of the tumor is so covered with anastomosing arteries, that it almost seems to pulsate.

October 9.—The tumor is begining to be smaller, and the œdema of the leg is subsiding.

October 15, the tumor had so far subsided, that he could not be persuaded to remain in hospital any longer. Nothing further has been heard of him since.

When the almost uniform success which has attended the treatment of aneurism by compression, in Ireland, is compared with the unsatisfactory results which have often followed a similar mode of procedure in England and Scotland, the question naturally arises, whether this circumstance is to be attributed to the general principle of treating the disease by pressure, or to some imperfection in the particular mode of its application. On referring to some cases which have recently been treated in this hospital, of which the above may be taken as a fair sample, I am induced to think that the nature of the pressure, and the time during which it is kept up, whether continuous or intermitting, has much to do with result.

When the compression treatment was first adopted in Ireland, it was, I think, laid down as a general rule, that the pressure should be continuous, for which purpose instruments with two pads were contrived; or one pad was placed in the groin, and another lower down on the limb; so that before pressure was taken off in one place, it was applied in another. Lately this plan of treatment has been very much modified, and, as in the present case, the pressure has been applied periodically.

The successful result may, perhaps, be attributed to the more healthy condition of the patient's blood which must be obtained by a proper amount of sleep, and to the more healthy condition of the integument at the seat of pressure, which, instead of ulcerating and sloughing, as it frequently does under continued pressure, may be reasonably supposed to regain its natural state during the interval of repose.

Another conclusion to which we are led is, that when once the fibrinous deposit has been caused to commence on the interior of the aneurismal sac, it is not washed away or reäbsorbed by the restoration of the circulation; so that when pressure is reäpplied, the process of solidification is, as it were, taken up, where it had been left off at the cessation of pressure. Indeed, the idea has sometimes occurred to me, that once the coagulum has commenced to form, it may, for a short time, continue to appropriate to itself the fibrinous element of the blood, even though rapid circulation is going on.

With reference to the intensity of the pressure, it was formally laid down that a small stream of blood should be allowed to flow through the sac, as it were to afford material from which the fibrin may be withdrawn. From the result of my own observation, I am inclined to think that this opinion requires modification. The treatment of aneurism by compression may be conveniently divided into two stages: first, that during which the collateral circulation is being developed; second, that period subsequent to its establishment, when the coagulating process goes on with greater rapidity.

In the first of these stages, I think the former opinion holds good, and that some blood should be allowed to pass through the main artery; but once the second period has arrived, as much pressure may be applied as the patient will bear, and certainly it should be enough to keep the wall of the main artery in contact; the establishment of collateral circulation being a guarantee that the vitality of the limb will be kept up, and that blood for the formation of a healthy coagulum is present in the sac.

Lastly, the pressure in this case was effected throughout by means of a conical leaden weight, the lesser end of which, about the size of an ordinary tourniquet pad, was applied over two folds of soft leather to the artery. To keep the weight in position, and to direct the pressure, a stiff iron wire which has been inserted into its greater end while the metal was melted, was passed through a ring fixed in a cradle, large enough to stand on either side of his pelvis.

From the satisfactory results which have followed the use of the weight applied in this way, I am induced to hope that its simplicity and facility of construction may render it useful in the country, where a more complicated apparatus cannot readily be procured.—*Dub. Gaz., March 15th*, 1858.

On the treatment of Aneurism by Manipulation. By Dr. George C. Blackman, Professor of Surgery in the Medical College of Ohio.—This is the third case in which this mode of treatment has been employed. The account of Mr. Fergusson's case, and of Mr. Little's, will be found in our last volume, p. 276.

Case.—John A—, æt. 28, a native of England, entered the Commercial Hospital on the 7th of April. Four months previously, he felt a sharp pain along the course of the femoral artery at the junction of the lower and middle third of the thigh, and for the first time he observed a pulsation in this region. He had worked for many years as a file-cutter, and had been accustomed to use a small anvil, which was held between his thighs. A swelling was soon detected, and this continued to increase until the time of his admission. There was a space of about three inches between the upper margin of the tumor and Poupart's ligament, and measured along the axis of the limb, the swelling was five inches at its base. The aneurismal bruit was very distinct, and the pulsations perceptible across the amphitheatre. Compression at the groin caused the tumor to diminish considerably in size, and it would immediately regain its former dimensions when the pressure was removed. The patient complained of numbness and other painful sensations in the knee, leg, and foot. As the tumor was daily increasing, and as there was no other indication of disease of the arterial system, I determined to bring the patient under the influence of veratrum viride, in order to subdue the force of the circulation. From the time of his admission he was kept on a low diet, and cathartics were administered. On the 11th, I ordered six drops of the tincture every three hours. On the morning of the 12th, I found that the pulse had been reduced in frequency from 94 to 65. At ten o'clock, A. M., of this day, he was brought before the class, when with my thumb I pressed forcibly into the aneurismal sac, for the purpose of dislodging a portion of its fibrinous layers, hoping thus partially to obstruct the artery, and to favor the further deposition of fibrin in the sac. Skey's tourniquet was now applied with moderate force between the tumor and Poupart's ligament. The progress of the case may be learned from the following record, kept by Dr. N. J. Sawyer, the house-surgeon: At 12, A. M., his pulse being 110, full, strong, and bounding, he was bled ʒix. Pulse came down

to 50, soft, and regular, and continued low for several days. (The following are extracts from the Case-Book): April 13th, A. M.—Suffers no pain or uneasiness at all; slept well last night. Entire limb diminishing rapidly in size. Kept the apparatus tight. General health good; whenever any untoward symptom arose, it was promptly met, and the patient kept in a good condition. At intervals, the shooting pain was felt in the tumor, but it gradually subsided altogether.

17th.—Prof. Blackman ordered the tourniquet to be taken off, the band age reäpplied from the toes, up over the tumor, upon which it was to be tightly wrapped, and the patient to be bled, after which the following was administered:

℞. Antimon. and Potass. Tart., gr. $\frac{1}{4}$;
Pulv. Opii, gr. $\frac{1}{8}$;
Sig.—Take every three hours.

Patient's pulse came down to 65, soft and regular.

19th.—*Souffle* ceased entirely, but the pulsation continues, though it is very weak.

22d.—Pulsation in tumor has ceased altogether.

25th.—Is in fine spirits; has no pain, and wants to walk about. General health very good.

30th.—Has walked some steps, and complains of nothing but weakness.

May 21st.—The pulsation in the tumor has never returned. The femoral is firmly plugged as far as the origin of the profunda, and in the popliteal space the pulsation of the artery is hardly perceptible. The tumor is daily decreasing in size, and the patient is anxious to leave the hospital and resume his business.—*Am. Jour. Med. Sci., Ranking's Abs.*

Treatment of Aneurism by Digital Compression.—We notice, in a foreign journal, a report of two cases of the successful treatment of popliteal aneurism by means of compression with the fingers, by M. Michaux, of Louvain, France. The first patient was a man of 57 years. The aneurismal tumor, which was situated in the lower part of the popliteal space, was of the size of the fist, and quite reducible. The patient also had signs of serious disease of the heart. Compression of the femoral artery with a tourniquet was first tried in the groin, but this causing swelling of the inguinal gland, and sloughing, it had to be discontinued. M. Michaux then decided to try digital compression on the lower portion of the femoral artery, which was performed by hospital pupils, in rotation, commencing the 4th November, at 5 o'clock in the morning. and continuing uninterruptedly for fifty-three hours, when it was omitted for thirty-four hours, and resumed again for twelve hours. After another interval of thirteen hours it was again resumed for fifteen hours. On the 10th Nov., the pulsation of the tumor was barely perceptible, and the swelling was very hard. On the evening of that day no pulsation could be felt; compression was, however, continued until the morning of the 12th, since which time the cure has remained perfect. The affection of the heart continued in the same state.

The second patient was cured much more quickly. The aneurism, which occupied the same situation, had existed for more than three years, and measured three and three-tenths inches in length by three and six-tenths inches in breadth. Digital compression was begun December 15th, at half past three o'clock in the afternoon, and constantly applied to the groin. In twelve hours, the sac already contained some coagulated blood. At 10 o'clock, on the 16th, there was no dilatation, and the pulsations were very feeble. At 4 o'clock the tumor was solid, and the pulsations entirely ceased after twenty-four and a half hours of digital compression, which was, however, continued till the next day, by way of precaution. In a few days the collateral circulation was established around the knee. The patient was discharged on the 29th.

Of course the employment of digital compression requires the coöperation of a large number of reliable assistants, who must be willing to undertake a most irksome duty. It can hardly be accomplished in private practice, and can only be made available in a hospital under exceptional circumstances. Still, the successful issue of the above cases should be borne in mind, as the method may sometimes be usefully practised, where the pressure of a pad cannot be endured, or where it produces sloughing, in order to avoid the necessity of tying the femoral artery.—*Boston Med. and Surg. Jour., Aug.* 1858.

Art. III.—*The Elimination, Catalysis, and Counter-action of Syphilitic and Lead Poisons:* By J. A. Easton, M. D., Professor of Mat. Med. in the University of Glasgow, etc.

[The following extracts, taken from an extended essay read before the Med. Chir. Soc., June 8, 1858, and published as the leading article in the *Glasgow Med. Jour.* for July, elicited in the society, a luminous discussion highly complimentary to the essayist, yet imbued with a strong, but discriminating dissent to several of the fundamental postulates of the learned professor. The remarks of Drs. Watson, Ritchie, Morton, and Mr. Reid, members of the society, possess a clearness and directness worthy of the essay which they criticise, and at the same time, for the most part, commend. This article is suggestive outside of the topics specially discussed]

I. On Syphilis.—The primary sores have been divided into two classes; the soft or non-infecting; the hard or infecting.

The form of primary sore, then, of which I am to speak first, is, as its name implies, of soft consistence, is comparatively superficial, has

a tendency to spread, and yields a copious secretion of purulent matter. Systemic infection, it is maintained, does not follow such a sore, mercury is not required for its cure, the syphilitic poison being removed from the surface by a process exclusively local, through the destruction, either by slough or by ulceration, of the part to which the virus had been applied. Poison and part are removed together, no mercury is administered, systemic contamination from either poison is averted, and thus an illustration is afforded of the statement made nearly at the outset of these remarks, that the effects of a poison are sometimes counteracted through destruction of a part of the organism. There is another circumstance connected with this form of sore which must now be mentioned, and which, if established, cannot fail to exercise a most important influence upon the diagnosis and treatment of syphilis. After sores of this kind, bubo is certainly not a necessary occurrence, but it is said that when the groin is affected, it will be found that generally one gland only is implicated, and that suppuration ensues as an almost invariable consequence. It is maintained, further, by our modern authorities, that while inoculation with the pus of the open bubo will produce a sore similar to that which existed at first, no other evidence of the *specific* character of the pus can be adduced, and that no constitutional symptoms are manifested. The statement of Ricord on this point is absolute and unqualified: "Tout bubon qui suppure spécifiquement, c'est à dire qui fournit du pus inoculable, n'est jamais suivi d'accident d'infection constitutionelle."* Assuming this to be true, the discharging bubo may be regarded as the completion merely of the eliminating process which had commenced at the primary score. By the prolongation, so to speak, of the eliminating surface, the open bubo may be supposed to effect what the primary sore had been unable to accomplish; an additional guarantee is thereby afforded against systemic contamination, and hence the exhibition of mercury is not only unnecessary but injurious. The mere softness, however, or induration of the primary sore, cannot of itself be accepted as a certain test for determining whether secondary results shall or shall not follow, and whether mercury should or should not be administered.

The other kind of primary sore, differs from that which we have just been considering. Unlike the former this sore is hard, isolated, and though the precursor of damage to the system at large, is comparatively torpid in its influence on the surrounding parts. The secretion from it is scanty and thin, and furnishes no pus globules when treated with acetic acid. But according to Mr. Henry Thompson, the most marked peculiarity of this sore is, that "it is INVARIABLY *(sic in orig.)* followed by slight swelling, and marked induration of the inguinal glands on one or both sides; usually several glands are affected; they are hard, incompressible, and roll under the finger—are painless, and do not inflame or suppurate, except in rare instances from over-exertion, in scrofulous subjects, etc.; but then the pus is *not specific* and *not inoculable*." The induration, then, even of the sore,

* Lettres sur la Syphilis, p. 199.

and still more so, that of the inguinal glands, may be regarded as the first act of elimination—no doubt an abortive one—to be succeeded at a longer or shorter period, on the average of about six weeks from the appearance of the primary sore, by more general excreting efforts, of which cutaneous eruption, ulcerated fauces, and periosteal thickening may be regarded as the most common. But even these are not adequate to remove the virus, and hence, under such circumstances, the counteracting assistance of mercury or of iodine is required. From several cases illustrative of this form of the disease I select the following: William Harley, admitted on 4th May, 1858. Above the frænum there is a small, circular, indurated, hollow sore, secreting a thin, glairy, apparently serous fluid. This sore has existed since the middle of last January. Several glands in each groin are enlarged, hard, roll under the finger, but are not painful. About six weeks before admission, patient's throat became sore, and on examination, a circumscribed, tolerably deep ulcer is found on each side of the uvula. The following was prescribed: ℞. Bichlorid. hydrarg. gr. ij., iod. potass ʒij., aq. ℥vi. Misce, cuj. mist. sumat ʒi. ex decoct. sarsapar. comp. ℥iv. ter in die. Under this treatment the patient rapidly recovered. It may be noticed that in this case there were not only the painless indurated glands in the groin to indicate the diagnosis, but that this was still further established by the unusual occurrence in our medical wards of a primary sore, which corresponded exactly in consistency, shape, and secretion with the description already given of the second or infecting variety. But the case of this man derives additional interest from the fact, that four years ago he had a "different-looking, bigger" sore, which was followed by one suppurating bubo, but on these there supervened no secondary symptoms, thus giving an example in his own person of the two different morbid conditions and results to which I am now directing attention. Notwithstanding the statements which have been made by high authorities to the contrary, I believe that such a manifestation of the syphilitic poison as was presented in this man's second attack can only be got rid of by the counteraction of mercury, and that to withhold that medicine, and to rely upon the reparative powers of the system for the ultimate elimination of the virus, is nothing short of wanton and most reprehensible tampering with the comfort and even with the life of the patient. At the same time it is equally culpable to give the mercury to profuseness, and in the great majority of cases the mere induction of tenderness of the gums—kept up for a longer or shorter time according to circumstances—will be quite sufficient to eradicate the disease. Further, I think that the mineral is most successful when administered in the form that was had recourse to in the case under consideration. By adding the bichloride of mercury to an excess of the iodide of potassium, the former acts the part of an acid, the latter that of a base, and the result is the formation of a double salt, the hydrargyro-iodide of potassium, which has the advantage of being very soluble, is thus very readily absorbed, and proves itself to be not only an efficient, but a rapidly acting remedy even in very small doses. By employing such a preparation, we give but a small quan-

tity of mercury, quite sufficient, however, for all therapeutic purposes.

But while it may be assumed to be an all but established fact, that the secondary symptoms of syphilis only supervene upon the kind of primary sore which we have just been considering, and which we have seen is of a very uniform character, it cannot be denied that the manifestations which are afterwards witnessed are of the most dissimilar and incongruous description. Throat affections and skin diseases, varying in intensity and in appearance—iritis, rheumatic pains, nodes, carious bones, nervous disorders of every possible kind and degree—not confined to the original recipient of the poison, but transmitted to his ill-fated progeny—these but faintly shadow forth the evils which follow in the wake of that solitary, indurated, ash-colored sore. This great diversity, however, in the secondary manifestations of syphilis does not, in my opinion, invalidate the notion that, notwithstanding their dissimilarity, they all proceed from one kind of poison, manifested at its earliest developement by one kind of primary sore alone.

What changes are produced upon poisons after they are conveyed into the system, and have been subjected to the vital chemistry of the blood—how different structures are differently impressed thereby—or how these changes may be determined by the constitutional peculiarities of the persons affected, or by the nature of the tissue through which the excretion of the poison is attempted, it is, of course, impossible to tell; and thus, the reason why one person exhibitis, from the same kind of poison, an eruption on the skin; another, ulceration of the throat; and a third, periosteal thickening, will ever remain among nature's obscurest arcana, into which neither the light of chemistry nor the genius of physiology, shall ever be able to penetrate. Leaving this topic, however, and looking at the secondary manifestations from a practical point of view, it will at once be admitted that these are numerous and diversified, so the treatment adapted to them must of necessity be different and varied. In these circumstances, which are often very perplexing, we have little to guide us except clinical experience, and hence a few practical hints derived from that quarter may not be considered inappropriate; and on so extensive a subject I shall, of course, confine myself to those forms of the disease which presented themselves in the wards under my charge.

Beginning, then, with skin diseases, I think that in all cases in which the eruption has been at first either of the papular or the pustular kind, and has afterwards degenerated into scaly, copper-colored patches, it will be impossible effectually to remove the poison without having recourse to mercury, either in the cautious and modified form already referred to, or in that recommended by Dzondi. In illustration of this form of skin disease, I select, out of many, a case in which there was the very common adjunct of rheumatic pains: Letitia Hamilton, aged 50, admitted on 6th April, 1858. Peculiarities of primary sore not ascertained. During the last three months patient has had a pustular eruption on the chest, back, and limbs, which has appeared in successive crops, its recession being marked by the permanence of copper-colored spots, from which scales are thrown off from time to time. The skin over the upper lip is glossy and puck-

ered, and both angles of the mouth are ulcerated. The posterior surface of the ears is ulcerated also. During the last four years has been frequently troubled with rheumatism. The combination of bichloride of mercury with iodide of potassium mentioned before, and in the same quantities, was administered in this case also. On the 14th April the gums became slightly tender, the eruption began to disappear, the rheumatic pains to subside, and on the 4th May she was dismissed cured.

Ulceration of the fauces, as is well known, is one of the most frequent forms of secondary syphilis. The ulceration, however, is met with under two aspects—circumscribed and phagedenic, or having a phagedenic tendency. Each of these manifestations of syphilis requires, I think, a different counteracting or catalytic remedy, mercury being the efficient agent against the circumscribed form of ulceration, iodide of potassium against that which I shall take the liberty of calling the phagedenoid. The case of Harley, already given, may verify and illustrate the former of these statements, and in elucidation and corroboration of the latter, I submit the following: Archibald Young, aged 25, had been nearly a month in the infirmary before he came under my care, and had been subjected during that time to considerable variety of treatment, which it is unnecessary, however, to detail On taking charge of the patient on the 1st November, 1857, I found from the hospital journal that the following report had been drawn up at the date of admission, the 5th October before: "Had primary syphilis three or four years ago, and five months since had gonorrhœa. Three months ago, after being exposed to cold, his throat became swollen and painful, and his deglutition difficult, but his voice was at that time unaffected. After being under treatment for some time, the symptoms almost entirely disappeared, with the exception of pain on pressure over the thyroid cartilage. Six weeks ago, however, the symptoms again appeared, and they have gradually increased in severity. On admission, the following symptoms present themselves, viz: a constant hawking of frothy mucus, voice exceedingly rough, the nares stopped, breathing embarrassed and painful; deglutition also so difficult and painful that food cannot be swallowed except in the liquid form; larynx generally, and thyroid cartilage in particular, swollen and painful on pressure; the posterior fauces and edges of the epiglottis in a state of ulceration; left tonsil inflamed and swollen."

On the 1st November, when this man became my patient, I found him in a very exhausted state, confined constantly to bed, with no appetite for food—irritable, desponding, feeble, and emaciated. Ragged, ill-defined, though not very deep ulceration was spread over nearly the whole extent of the velum, the voice was hoarse, deglutition painful and difficult. He was ordered fifteen grains of iodide of potassium three times daily; was put upon full diet, with the addition of a pint of porter; and had a full opiate every night. Under this treatment he improved daily; so much so, that on the 24th November he was reported to "have gained very much in health and strength. Ulcers in throat have completely cicatrised; but there is

considerable loss of substance in anterior velum. Considerable contraction evidently exists also in throat and upper part of gullet, and deglutition is impeded from that cause." On the 25th November he was dismissed cured.

Farther, the iodide of potassium proves itself a valuable remedy in the treatment of syphilitic nodes. In this form of syphilis, as well as in phagedenoid ulceration of the throat, the iodide, according to my experience, is as superior to mercury as mercury is to the iodide in the treatment of circumscribed ulcers on the fauces and of papulo-squamous or pustula-squamous eruptions on the skin. But, in order to be beneficial, the salt must be given in doses not less than those which were exhibited in the case of Young. To this point, however, I shall return in the sequel. Meanwhile, I select the following case as illustrative of the value of large doses of the iodide of potassium in the treatment of nodes: "William Wylie, aged 35, admitted 26th October, 1857. Had syphilis five years ago, and two years after had scurvy; since then he has had syphilitic periostitis of tibiæ and general articular rheumatism. Numerous nodes exist over tibiæ. Within the last six weeks a swelling made its appearance over the upper part of the sternum, which has gradually increased, and is now the size of a pigeon's egg. This swelling is hard, and slightly painful on pressure. At the period just mentioned patient began to complain of hoarseness, and of some degree of pain in the larynx, increased on pressure and in deglutition. No ulceration of the fauces has as yet been observed."

This man did not come under my charge till at the beginning of November, when, as in the case just narrated, fifteen grains of the iodide of potassium were administered three times daily. The beneficial results of the treatment soon became apparent. On the 24th November, it is reported in the hospital journal that the "nodes over tibiæ and sternum have all but disappeared," and on the 28th of that month he was dismissed cured. These cases may suffice for the illustration of the only forms of secondary syphilis which have occurred in my wards since I took charge in November last. The examples given not only illustrate some of the more common modes by which nature attempts to eliminate the poison of syphilis, but they also exhibit by contrast some of those secondary manifestations which are more amenable to the catalytic power of mercury, and some of those likewise which are more under the control of the iodide of potassium. Lastly, on this subject, it may be matter of wonder, that while considering how the poison of syphilis is excreted from the system, I have not alluded to the "syphilisation" of our own day, as practised by Turenne, Sperino, Boeck, and others. The reason is soon given. I have never practised syphilisation, nor have I seen it practised, and therefore did not wish to discuss a plan of treatment the effects of which I could not illustrate by clinical observation. Far be it from me to say that this plan will not realize the object for which it was instituted; but I suspect that in this country, at least, patients will rather demur to having each side of the chest converted into a "plot" for raising succulent, young chancres, the more es-

pecially that, for the successful cultivation of these salubrious exotics, it is necessary that new ground be broken every third day, and that the chancrocultural operations be continued until the humus is completely exhausted, and no more crops can be raised. I rather think that not even the lure of being "immuned" will make syphilisation popular among our cautious countrymen, albiet Professor Simpson seems to think that it is not "so objectionable as three doses of blue pill every day."* *De gustibus, etc.*

II. Lead Poisoning.—I shall next illustrate the law of elimative effort, as well as some allied topics, by referring to lead poisoning. The toxic effects of lead, though occasionally resulting from some one of the preparations having been swallowed, are more frequently caused by the mineral having been introduced into the blood through the medium of the skin or the lungs, or through both channels at the same time. Hence the well-known occurrence of saturnine poisoning among painters, plumbers, and others who manipulate with lead, especially with its oxides and carbonate. Lead, then, like syphilis, is introduced from without, but there the resemblance stops. No doubt after the introduction of lead into the system, as after that of the lues venerea, nature soon commences the work of elimination; but then, the eliminative manifestations arising from the plumbeous poison, so far from assuming the Protean character of those which are indicative of the syphilitic, are few, uniform, and typical.

The poison is deposited at one or two points of the solid organism; and the tissues which are selected for the reception of the pernicious ingredient are the muscular fibres of the abdominal walls and intestinal tube, those of the extensor muscles of the forearm, the edge of the mucous membrane of the gums, and, more rarely, the brain and spinal marrow. Hence colic, obstinate constipation, inability to extend the hands, a blue line in contact with the teeth, with the occasional, though rare existence of epileptiform convulsions, constitute the few but significant attestations to the fact that the saturnine poison has been in the blood, but that nature, like a watchful guardian, determined at all hazards to protect the vital fluid, has precipitated the intruder into less-favored portions of the system.

The symptoms which have just been named illustrate the statement made in a former part of this communication, that sometimes, in consequence of the natural eliminative process, serious structural changes and functional disturbances take place in some parts of the organization. But these eliminative efforts, though they have succeeded in extruding the foreign ingredient from the blood, do nothing towards repairing the injury which, through their instrumentality, has been inflicted on those parts which have been doomed to receive the abnormal—I had almost said the unwelcome—constituent. Hence the necessity for artificial means to repeal the unnatural union that now subsists between the poison and those structures in which it has been lodged. The means which have been employed for this purpose are various, consisting of purgatives, diaphoretics, strychnia, galvanism, etc.

* Edinburgh Medical Journal for April, 1858, p. 953.

These agents, however, will never of themselves restore the paralysed fibres of the intestinal tube, and of the extensor muscles of the arm, to their normal condition. And the cause of the failure is obvious. The poison has become part and parcel of the tissues into which it has been deposited. An insoluble, and consequently, an ineliminable compound has been formed between organized structure on the one hand, and an inorganic mineral on the other. Thus imprisoned in the organism, the poison can never be liberated by the means mentioned above; its paralysing influence can never be obviated, its ultimate removal from the system never accomplished, until the compound alluded to—and which, for subsequent reference, I may call an albumino-plumbate, has been broken up, and transformed into that soluble condition in which alone it can reënter the blood to be finally and altogether ejected by the kidney—the great conduit of the animal economy. It being impossible then to eliminate what is insoluble, so long as it continues insoluble, the basis of all subsequent operations, the first step in a rational plan of treatment, is to disintegrate and dissolve the albumino-plumbate which has been formed; for until that has been effected there can be no hope of success. Two methods of accomplishing this object have been adopted of late years—the use of sulphur baths, recommended by M. Legrouse, and the administration of iodide of potassium, suggested and successfully practised by M. Melsens.

[The remaining space of this journal, is insufficient to admit the insertion of Dr. Easton's cases, illustrations, and reasonings, which are of much interest.]

ART. IV.—*Physiology.*—DR. BROWN-SÉQUARD'S JOURNAL.

[DR. BROWN-SÉQUARD'S *Journal de la Physiologie* (quarterly) for April, and for July, owing to some mistake in mailing, did not reach the office of the N. O. Medical and Surgical Journal until the November number was nearly through the press, in consequence of which, opportunity was wanting to make translated extracts from the highly interesting *Journal de la Physiologie de l'Homme et des Animaux.* To supply this omission, a few paragraphs will be subjoined, from an estimable cotemporary, by an able pen, just received—an analytical aperçu, which, the remaining limits of the New Orleans Medical and Surgical Journal will not permit the insertion of *in extenso,* but which will sufficiently indicate the high claims of the Journal of Physiology to the patronage of the American profession.

The original contributions in the order following, in the July number, by MM. BROCA, BROWN-SÉQUARD, ROUGET, THOLOZAN, VERNEUIL, E. SMITH, COLIN, BONNEFIN, POGGIALE, occupy 126, and the analyses of books,

translations, selections, and the appreciation o … physiology, nearly 100 pages, including numerous …strations.]

"This, the second [April] number of the *Journa… …iologie*, fully sustains the very favorable opinions which greeted … …ppearance of the first. Dr. Sèquard, its able and industrious editor, is evidently determined to make his Journal the vehicle of much that is both new and valuable in physiological science, and thus render it an indispensable ad lition to the library of every scientific practitioner of medicine.

"Until within a very few years, it has been the custom to regard pure physiology as a science more speculative and theoretical than substantial and practical; fuller of crude and fanciful notions than of important and well-established facts; and, consequently, as of little or no practical use to the healing art. Those, however, who have most narrowly inspected the toilsome course of modern physiology, from the time of Haller and Harvey, its justly reputed fathers and founders, down to the present day, know that it has been steadily progressive, constantly accumulating numerous and highly valuable facts, the true nature and relations of which have, in many instances, been thoroughly determined, and a positive step thus made towards the establishment of the fundamental principles or laws of the science. As a necessary result of this progression, physiology is, at the present moment, specializing itself in another direction. It is assuming, in fact, the character of an applied science, and practical physicians of all countries, with a daily increasing confidence in its capabilities, are hopefully looking to it to illuminate their empirical and uncertain course, and dissipate the doubts which hover, fog-like, about their path. The importance, indeed, of applying physiological facts to the elucidation of the complex problems of pathology is slowly but strongly attracting the earnest and respectful attention of the profession. This we regard as a healthy sign.

"The present number of the journal contains thirteen original memoirs, contributed by MM. Brown-Séquard, Bernard, Robin, Blondlot, and others; the usual analysis of new books; translations from the English and German; extracts from the journals, and a brief *apprécia-tion* of the progress of physiology.

"The titles of the memoirs are as follows: 1. Researches upon the Causes of Death occurring after the Extirpation of that part of the Medulla Oblongata which has been called the Vital point. By Dr. E. Brown-Séquard. 2. Upon the Variations in Color of the Venous Blood of the Glandular Organs, according to their condition of functional activity or repose. By Prof. Cl. Bernard. 3. Note upon the Influence exercised, in certain Cases, by the lateral half of the Spinal Marrow, upon the corresponding half of the Encephalon and Face. By Dr. E. Brown-Séquard. 4. The Origin of Sugar in the Animal Economy. By A. Sanson. 5. Researches upon the Action of the different parts of the Solar Spectrum upon Vegetables. By Dr. C. Guillemin. 6. Limits to the Possibility of the Spontaneous Return of Cadaveric Rigidity, after it has been destroyed by Elongation of the Muscles. By. Dr. E. Brown-Séquard. 7. Note upon some Points in the Anatomy and Physiology of the Red Globules of the Blood. By Dr. Ch.

Robin. [illegible] facts which appear to show that several kilogrammes [illegible] e formed and transformed, every day, in the Body of M[illegible] upon the Seat of this Production and Transformation. By Dr. E. Brown-Séquard. 9. New Researches upon Digestion. The Acid Principle of the Gastric Juice. By M. Blondlot. 10. Researches upon the Erectile Organs of the Female, and the Tubo-ovarian Muscular Apparatus, with their Relations to Ovulation and Menstruation. By Dr. Ch. Rouget. (With three plates.) 11. Upon Contractile Sensibility, and a Method of Measuring it, in Anæsthesia and Hyperæsthesia. By Dr. E. Brown-Séquard. (With a wood-cut.) 12. Note relative to the Existence of Sexual Generation among the Infusoria. By Dr. E. G. Balbiani. (With a plate.) 13. Experimental Researches upon the Physiological Properties and Uses of Red and Black Blood, and their Principal Gaseous Elements—Oxygen and Carbonic Acid. By Dr. E. Brown-Séquard.

"All the above memoirs are well worthy of careful perusal. Our limits, however, permit but a very brief reference to their contents.

"It is well known to the physiologists, that some years ago, M. Flourens announced, as one of his most important discoveries, that the removal or destruction of the pen-like point of the very small triangular or V-shaped piece of gray matter bounded by the posterior pyramids and restiform bodies, and known to anatomists as the calamus scriptorius, is followed immediately and invariably by the cessation of respiration and vital activity. This spot, scarcely a line in extent, he calls the vital point or knot (*le point* ou *la nœud vital*) of the nervous system; and he regards it as the nervous centre of respiration—the seat, in fact, of the vital principle. In the highly interesting paper which heads the above list of memoirs, Dr. Séquard attempts to show that death is not always the immediate consequence of the destruction or removal of the *point vital.*

"From his experiments and observations upon this interesting subject, Dr. Séquard arrived at the following conclusions: 1. That death is not always the immediate result of the extirpation of the *nœud vital.* 2. When death takes place suddenly, after such extirpation, it is partly due to the sudden arrestation of the cardiac movements—produced by the irritation of the medulla oblongata. 3. The irritation of the parts contiguous to the vital point like the removal of the latter, arrests or enfeebles the action of the heart. 4. After section of the par vagum, extirpation of the vital point never occasions the sudden cessation of the heart's motion. 5. The cessation of the respiratory movements which sometimes follows the extirpation of the vital point, results not from the absence of this point, but from an irritation of the medulla oblongata. 6. Irritating the nervous tissue surrounding the vital point sometimes retards respiration, although this point is not itself injured. 7. Respiration and circulation may continue strongly and regularly for very many days after the ablation of the vital point. This point is, therefore, neither the original seat of a so-called vital force, nor the primary motorial centre of the mechanics of respiration. 8. Voluntary movements and the sensorial functions often persist after the extirpation of the *nœud vital.* 9. The vital point appears not to be essential to life.

"In the bibliographical section of the journal, we observe with pleasure, a brief, but important notice of Dr. Dowler's Researches on Post-mortem Contractility, in which Dr. Séquard takes occasion to acknowledge the priority claimed by Dr. Dowler for the annoucement of certain facts connected with post-mortem rigidity of the muscles. Although the physiological investigations of Dr. Dowler have long been familiar to the physicians of the United States, they are, as yet, almost wholly unknown to those of Europe. With equal pleasure, we find, under the head of "Translations," the paper of Dr. Isaacs, of New York, on the "Function of the Corpuscles of Malpighi," while in the *appréciation* of the progress of physiology, the scientific labors of no less than five American physicians are noticed. It will thus be seen that the *Journal de la Physiologie* is already, at this early stage of its existence, becoming an important channel for the introduction of American physiology into Europe. The long sojourn of its able editor in the United States, his intimate acquaintance with many members of the profession in this country, and his familiarity with the English language, are qualifications well calculated to render his journal the best European exponent of the labors of American physiologists. J. A. M."—*Amer. Jour. Med. Sci., October*, 1858.

[Addendum.—The Oracles responded evasively. The feminine for *no* is *yes;* some late physiological "adjudications" mean both yes and no, an affirmative-negative, all of which, Dr. Brown-Séquard avoids. He examines Dr. Dowler's essay on muscular contractility, (N. York, 1846) quotes special cases and doctrines, and then, of the whole, (not merely of "post-mortem *rigidity*") says: "It is our duty to avow that we have established facts similar to those above mentioned, and which, for a long time, we believed we had been the first to describe. We were deceived in this respect. We seize with eagerness this occasion to render homage to the truth, by declaring that to Dr. Bennet Dowler belongs the *discovery* of these facts: *Nous saisissons avec empressement l'occasion de rendre hommage à la vérité, en déclarant que c'est au docteur Bennet Dowler qu'appartient la découverte de ces faits,*" p. 374. Of other productions of Dr. D., as on animal heat, the capillary circulation, etc., which he characterizes as "extremely *original,*" etc., he purposes to give details in the future numbers of the *Journal de la Physiologie.*

The essay on contractility, and others by the same writer, never appeared in the New Orleans Medical and Surgical Journal; hence, allusions to them from the banks of the Seine, may be acceptable on the banks of the Mississippi, particularly to kind friends in the South, who have offered to defend them, but whose letters on this subject remain unanswered.]

TO OUR SUBSCRIBERS, AGAIN.

To our last appeal, we have received so many and so satisfactory responses, that we are induced again to entreat the large majority still in arrears, to "pay up." As *unrewarded* laborers in the cause of Southern medical literature, we have the right, perhaps, to expect something from the *generosity* of Southern physicians. To this, however, our appeal is not addressed, but solely to their sense of justice. We do not place our trust in that magnanimity of which when one eye may be demanded twain are given, but simply solicit the delivery of the single eye appertaining to us.

To all who were in arrears when this Journal passed into the hands of the present proprietors, we again repeat, that the bills sent are as we purchased them, and as they stand on the books; but that we leave the decision of the amount *justly* due, entirely to the discretion of the subscriber himself; and pledge ourselves to return a receipt for the money which may be sent, up to whatever time he positively asserts it justly pays.

In September last, a circular letter was addressed to some three hundred and fifty subscribers, the sum total of whose indebtedness amounted to nearly $10,000; a debt which had for several years been increasing with *diminished* receipts, so that it became evident that the loss of these patrons, as well as of their debt, was preferable to continuing as heretofore. We were, therefore, compelled to demand of them some evidence of their intention to pay what they might acknowledge to be due, or in default thereof, to erase their names. So much was required for self-protection. Many of these gentlemen have already replied most satisfactorily; to those, and those only, from whom we have received no reply to these letters we would say, that in April last a circular letter was addressed to each of them, that twice since September, 1857, simple bills without comment have been enclosed in the Journal; and that after each of these three little messengers, we have like Brutus of old paused for a reply, and like him too received none. Upon which our unceremonious communication of September, 1858, was sent them, which we must *act* upon. By so doing, we no doubt may erase many good subscribers and honorable gentlemen from our list; but among these are so many others from whom we will never receive anything, that we will gain by their loss. Let them remember that we have no possible means of distinguishing the good from the bad ex-

cept by the date of their credit. This we know, from past experience, is probably in some instances wrong, we ask you to correct it, giving you discretionary power to determine the date to which you ought to be credited. If, after a proposal so fair, and a silence continued after four attempts to obtain some response, we still fail to hear from you, we shall try to believe, for the honor of our common profession, that you are dead or removed, the mails failed, or that some disaster has occurred which will make it to the interest of both parties to cease the present connection, and our duty to collect what we can by the most effectual means in our power.

STANFORD CHAILLÉ,
Business Manager, etc.

Mortality Statistics of New Orleans, from August 1st to Oct. 17th, 1858, compiled from the Weekly Reports politely furnished by Dr. Baldwin. Secretary of the Board of Health. Population of New Orleans estimated at from 150,000 to 200,000.

Time.	*Total Deaths.*	*Children under 2 yrs.*	*Under 20.*	*U. States.*
August (4 weeks)	1826	216	546	690
September (4 weeks)	2469	263	733	829
October (3 weeks)	1567	187	504	600

Principal Diseases.	*August (4 weeks.)*	*Sept. (4 weeks.)*	*October (3 weeks.)*
Still born	34	36	20
Tris. Nascent	20	17	19
Cholera Infantum	8	7	3
Infant Convulsions	65	75	22
Infant. Marasmus	11	14	8
Teething	27	16	11
Croup	14	3	8
Scarlatina	6	2	6
Rubeola	0	0	0
Variola	3	1	0
Diarrhœa and Dysentery	52	34	38
Gas. Enteritis	10	15	6
Inflammation of Liver	5	11	6
Inflammation of Lungs	12	11	8
Phthisis	54	55	45
Apoplexy	13	5	6
Congestion of Brain	27	24	22
Fever Typhoid	26	27	14
" Miasmatic	65	121	56
" Yellow	1140	1825	1078

The weekly mortality of yellow fever for the seventeen weeks, June 20th, to October 17th, has been as follows: 2, 8, 9, 20, 25, 70, 140, 286, 312, 402, 448, 472, 460, 444, 380, 390, 308.

There will be observed an unusual proportion of deaths from In-

fantile Convulsions, Congestion of Brain, Typhoid and Miasmatic Fever, it is highly probable that very many of these were yellow fever. The certificates of *Pernicious* Fever have have reported by the Board of Health under the head of Conjestive Fever, which composes nearly all of those classed by us as "Miasmatic Fever." A majority perhaps of our physicians consider this Pernicious Fever, occurring chiefly among children, as veritable yellow fever.

STANFORD CHAILLÉ.

METEOROLOGICAL TABLE.

To the politeness of Dr. Porter, U. S. A., we owe the subjoined Table, which will enable those interested in tracing a connection between our epidemics and meteorological conditions to institute a comparison between the healthy summers of 1856 and 1857, and the very sickly season of this year.

1856.	THERMOMETER.				HYGROMETER.				RAIN.
	7 A. M.	2 P. M.	9 P. M.	Daily Mean.	7 A. M.	2 P. M.	9 P. M.	Daily Mean.	Quantity Inches.
April	70.37	76·30	70.30	72.32	66.27	68.17	67.93	67.00	2.85
May	77.35	81.03	78.87	79.08	70.87	72.06	71.97	71.66	2.53
June	81.13	85.06	82.70	82.96	77.61	76.41	77.16	77.06	5.79
July	82.55	86.13	83.71	84.12	78.40	78.06	78.71	78.40	8.63
August*	81.93	85.55	84.19	83.88	78.38	78.93	79.48	78.93	16.12
September	77.40	80.46	79.13	79.	73.93	74.73	75.03	74.58	.320
1857.									
April	69.90	67.70	66.30	64.96	58.36	62.70	63.03	61.36	1.73
May	70.51	76.90	75.09	74.16	67.57	70.32	70.74	69.57	7.33
June	77.20	82.13	80.96	80.09	73.93	74.90	76.03	74.95	2.90
July	79.64	83.48	82.29	81.80	76.25	77.12	77.61	76.99	5.86
August	80.16	83.06	82.19	81.80	77.35	78.12	78.45	77.97	4.64
September	77.66	82.20	80.60	80.15	73.70	75.80	76.23	74.24	2.55
1858.									
April	67.26	74.33	72.93	71.50	64.13	69.16	68.30	67.19	2.26
May	74.32	78.84	78.	77.05	71.80	73.13	74.35	73.09	3.45
June	79.43	82.26	81.53	81.07	76.53	76.66	77.46	76.88	4.89
July	81.96	84.67	84.09	83.57	79.32	79.16	80.29	79.59	7.72
August	81.64	85.64	84.51	83.93	78.45	79.12	80.38	79.32	7.35
September	77.46	81.70	80.73	79.96	74.13	76.10	76.96	75.73	3.67

[Correct]

J. B. PORTER, M. D. SURGEON, U. S. A.

*Last Island Storm.