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Messrs. Editors—Having spoken of the periodic character of these fevers, (intermittent and remittent,) and insisted upon the necessity of attention to that which constitutes their most important characteristic feature—important, at least, so far as their proper treatment is concerned,—I will next examine them with reference to the nature of their special pathology; but before I do so, it is necessary that I should make some general remarks respecting the nature of that condition of the system called "fever."

Heretofore, in my remarks respecting the general pathological conditions of excitement and depression, I have designedly neglected to draw any distinctions between the febrile affections with which they were associated, whether they were regarded as idiopathic or symptomatic, for the reason, that I desired to keep the attention fixed upon the conditions, and not upon the diseases, with which they were associated; and for this purpose, selected pneumonia, or inflammation of the lung, as the disease, and the fever, as the consequential condition, varying in character and degree, according to the nature of the individual, and general predisposing causes, epidemic influences, &c., which I have already pointed out.
I come now to consider "fever" in the light of a disease, and not a condition, symptomatic of, or depending upon, the pre-existence of some other disease; and the first question which presents itself is—What is fever? If we define it to be a state of general nervous excitement, with increased vascular action to such a degree or extent as to derange, interrupt, or suspend, the healthy performance of the vital functions, with the characteristic signs of such a condition, as heat and dryness of skin, with increased strength and frequency of pulse, it may serve to convey a general idea of fever, but does not let us into the secret of its first cause, or the true pathology of the disease. A better definition of fever, in my opinion, is, that it is an effort of the system to relieve some laboring organ or impeded function, to restore some suspended secretion, or to rid itself of the presence of some noxious and offending cause. That fever, though it consists in a concatenation of morbid actions which involve the whole system, often producing the most dangerous and fatal results, is nevertheless a recuperative process, or a sanative effort of the system to overcome some pre-existing morbid impression under which it labors, as the result of the operation of one or more of the thousand causes, appreciable and non-appreciable, moral, physical and chemical, which are perpetually at war with the vital powers, and interfering with the healthy operations of the animal economy. That the concatenation of morbid actions constituting fever has its origin or starting point in some part, organ, system, or function, all being subject to the influence of the morbid agents, and capable, under circumstances, of originating the febrile movement. That the pre-existing morbid impression which calls the sanative effort of the system into action, is a change in the innervation of the part, which implies a departure from the line of healthy excitement, by such an accumulation or exaltation of nervous power above, or such a loss, destruction, or depression of nervous power below that line, as to interrupt, suspend or destroy the healthy action of the part, organ, system, or function involved, and consists of debility or nervous depression the first link, irritation the second, and inflammation the third, in the chain of all morbid actions; upon these depending all the changes which take place in the fluids and solid tissues consequent upon the febrile movement. This view of the subject assigns to all fevers a local origin, but it does not explain how the existence of a local morbid impression,
either of depression, irritation or inflammation, brings about the febrile movement. This may not be so difficult to explain, or understand, with reference to those fevers which are recognized as symptomatic, having inflammation or irritation as their ostensible cause, if we call to our aid the physiological relations which are known to exist between all parts of the system through the great nervous centres, and the facility and rapidity with which, by direct and reflex nervous action, impressions are transmitted and communicated from one part of the organ or system to another. But it is somewhat different with those fevers which have no such ostensible cause, and which have, in consequence, been recognized as idiopathic fevers. Such are the fevers under consideration (intermittents and remittents) in their essential typical character.

To explain the origin of these fevers, we must suppose, what has generally been conceded, that the causes, which are very numerous, act with a greater degree of force upon some organs than upon others—that some are directly depressing in their influence, and others indirectly so; but that they all tend to the same result, namely, debility and depression. Pathologists have endeavored to assign for the seat and origin of these fevers a special location; some have taken the brain, others the spinal or ganglionic centres, while others have taken particular organs, as the stomach, liver, spleen, &c. But the truth, I think is, that though some organs are more uniformly implicated in their origin, none can be said to be invariably or essentially concerned; but that those organs which have been most strongly impressed, or which yield most readily to depressing influences, will have to bear the brunt and burden of the disease. Hence the great variety of these diseases which have been described by the systematic writers, both in this country and Europe—varieties founded, not upon any difference in their essential pathology, but upon the organs involved. When, then, any important organ, from the operation of the depressing causes, falls into such a state of debility or depression as to render it incapable of performing its accustomed functions, the recuperative powers of the system are called into requisition in aid of the laboring organ; the sanative effort commences, and a rally of the vital forces takes place in the organs where it accumulates, at the expense of a proportional loss to other organs and parts of the system. This process will usually be attended with a sort of oscillatory motion in the nervous and circulatory systems,
constituting the forming stage of these fevers. At this time, the
general, remote or systemic capillaries, sharing in the general loss
of nervous power, become relaxed, allowing the free flow of blood
out of the arteries, while its flow is retarded in the lungs, from a
similar loss of power in the muscles of respiration. Thus, the bal-
ance between the arterial and venous portions of the circulation
becomes broken, and the blood accumulating upon the right side
of the heart, in the large venous trunks and cavities, constitutes
the congestive or cold stage or chill. Now, though an analysis
of the symptoms and phenomena which belong to, and distin-
guish this condition, shows not a single one which is character-
istic of fever, but indicates, on the contrary, a diametrically oppo-
site condition, we are bound to consider it an essential condition
in the pathology of these fevers, being generally found to appear
in the first and the last act, and frequently throughout the drama.
When this condition has reached the lowest point of depression,
and continued for a somewhat indefinite time, the recuperative
powers of the system are again called into operation, the sanative
effort and the reactionary movement commences. The accumula-
ted excitability, the nervous power, which had been rallied in
support of the laboring organ, being now no longer necessary for
its support, is redistributed to the organs whence it was taken.
The organs of respiration thus acquiring their accustomed power
of action, allow a free flow of blood through the lungs, while
the remote or systemic capillaries, having regained their lost tone
and power of action, and the blood retarded in its flow through
them, accumulates upon the left side of the heart, producing a
general arterial plethora, giving rise to the characteristic phenome-
na of, and constituting the true febrile condition. When this
reactionary movement commences, which is at first slow, impor-
tant changes take place with respect both to the nervous and cir-
culatory systems. The blood which had been pent up, and become
somewhat depraved, acquires, in its passage through the lungs,
new life, which it imparts to the heart and arteries, to the brain
and other nervous centres, increasing their activity, and thus
creating a state of general nervous excitement and increased vas-
cular action, giving rise to heat and dryness of skin, and strength
and frequency of the pulse, the characteristic symptoms of fever.
This state of excitement, after continuing for a somewhat indefi-
nite period, gradually subsides, the capillaries gradually relax, the
balance in the circulation becomes restored, the secretions become re-established, and an end is put to the paroxysm.

This hasty and imperfect sketch of an intermittent, serves to show us the existence of two distinct pathological conditions, one consisting in a state of general nervous depression, attended as a consequence with a broken balance in the circulation, and an undue accumulation of blood in the venous system; the other consisting in a state of general nervous excitement, with a consequent broken balance in the circulation, and an accumulation of blood in excess in the arterial system. That one, or the other, of these conditions constitutes the essential pathology in all fevers, there can be but little doubt; and that both are necessary and essential to the formation and progress of intermittent fever, I think there can be none; and I might say that there is little with respect to remittent fever. It is true, that a paroxysm of these fevers may be ushered in with such slight evidences of the existence of depression and congestion, as to escape the attention of the patient, and the observation of attentive and skilful observers; but such cases cannot be taken as exceptions to the general rule, as I have before shown that these conditions may exist in various degrees of development, from the lowest grades of depression below, to the highest grades of excitement above the line of a healthy excitement; and I think it would be much more fair and reasonable to suppose that the depression really existed in such cases, though not observable, than to suppose that it did not, because the signs of its existence could not be observed.

My impression, therefore, with respect to these fevers, is, that the first essential element in their pathology is nervous depression, with more or less of general venous congestion; and that the second essential element in their pathology is, nervous excitement, with increased vascular action, with more or less general arterial plethora. Whether this be the case with respect to other fevers, must remain for further discussion, while we go back a little, and examine into the laboring organ which has given rise to all this mischief and disturbance. Well, in the absence of positive proof to sustain the position, I would assume, as the most probable state of the case, that the accumulated excitability or exaltation of nervous power in the weak and laboring organ would serve to relieve it from its condition for the time being, but upon its re-distribution, and the subsidence of the general excitement,
it would be left in near about the same state of debility and depression as at first, and would be the subject for renewed or repeated efforts of the system, through a succession of paroxysms, until the condition was removed or overcome by the efforts of nature, or art; such being the character of our simple intermittents. Or, to take another step in the morbid concatenation, we may suppose that the weak and laboring organ being, in consequence, unable to resist the force of an increased column of arterial blood, which would accumulate in undue quantity, and thus give rise to irritation in the organ, would in turn have the effect of keeping up for a longer time, and in a higher degree, the febrile movement as observed in our simple remittent fevers; or, from the oft recurring state of irritation of the organ from repeated paroxysms, or a protracted state of general excitement, the local excitement or irritation might eventually spring up into a state of inflammation, the highest and last link in the chain of morbid actions, which we see occurring sometimes in intermittent and remittent, and in various other forms and grades of fevers. Such being the usual progress in the degrees of morbid action in the organs involved in our fevers, we are not required to recognise local inflammation as essential, either for their production or for their continuance. Local inflammation often exists without fever: it often occurs as the consequence of fever; and we often have fever in which there is no evidence of the existence of inflammation, either as cause or effect. Besides, inflammation is not a primary but an ultimate degree of morbid action, and even when considered as the cause of fever, it might not be found an easy matter to establish the fact, that the febrile movement had been incited by the ultimate, and not the primary morbid action. Now, I wish it to be understood that the examples of the degrees of morbid action, which I have stated, are not confined to the organ which created the disturbance; nor does it necessarily pass through these degrees during the progress of the fever, but that through the intimate anatomical and physiological relations which exists among the different organs, many of them become involved and assume one or other of these degrees of morbid action, having had no direct agency in the production of the fever. Hence they should be regarded as non-essential, and as the result of adventitious influences. As the character of all these fevers is determined by the general predisposing causes which I have elsewhere shown, so it
is with the different organs involved, being the result of individual and general predispositions, and often of prevailing epidemic influences.

To enable us to trace the phenomena of these fevers as near as possible, through the order in which they arise, and to preserve as accurately as possible their relations, one to another, we will commence with those which result directly from the broken balance of the circulation, and the undue accumulation of the blood in the large venous trunks and cavities, which we recognise as General Congestion from General Nervous Depression. Having designated the lungs, heart and liver, as the points for the formation and the chief seats of congestion, we are bound to suppose that under the operation of general causes it would commence simultaneously at all those points, and consequently all the organs anatomically related which become involved, do so, at the same time. Passing by the general outward signs of the condition of nervous depression and congestion, the first organ which claims attention is the brain. Though this organ shares alike with other organs in the general loss of nervous power, and is often the seat of irritation and inflammation, it is never, as I have before shewn, the seat of congestion, except as the consequence of congestion in the lungs or the heart; consequently, whatever importance may be attached to that condition of the brain, (and I am not disposed to attach a great deal, from the fact that it is rather a mechanical than a pathological effect,) for its removal, we must look to the organs which gave rise to it. Congestion of the brain is not in itself a very dangerous affair, for occupying a closed cavity, it is subject neither to a very great increase or diminution of its proper quantity of blood, either of which would be apt to produce sudden fatal results, in cases of great general depression. This circumstance, which I have endeavored to explain elsewhere, enables the brain to continue in the exercise of its functions, often in a wonderful degree, amidst the general wreck and prostration of all other vital functions. Cases, however, often occur in these fevers, in which the action of the heart is sufficiently strong to throw an undue quantity of blood upon the brain, giving rise to more or less pain or uneasiness in the head, dullness of intellect, drowsiness, and not unfrequently to stupor and insensibility, which usually occurs suddenly as by an impulse. Now, these evidences of cerebral disorder, though not devoid of danger, are generally
more alarming than dangerous, for the reason that they imply no seated morbid action in the brain, and generally subside and pass away with the paroxysm. They cannot, however, be regarded with indifference, or without apprehensions of subsequent danger, as upon the establishment of the febrile stage of the disease, the brain would be more apt to become a seat of morbid action, and a state of irritation kindled up in the previously distended vessels, which might continue, in that character, through a succession of paroxysms, producing in each, headache, delirium, convulsions, and other signs of cerebral irritation, and, as has been explained, ultimately to spring up into a state of inflammation with all its direful consequences. I feel inclined to pause here, for the purpose of making some practical comments upon these different morbid conditions or states of action in the brain, knowing as I do, that they are too often mingled up and confounded, and that cases have been heroically treated (to the great detriment of other organs, and the system, generally,) as inflammation of the brain, which were but slight congestion or irritation, which would have yielded, and which did yield at once upon the arrest of the paroxysm. As this subject, however, must claim attention hereafter, I will proceed to enquire into the effects produced upon other organs from congestion of the liver.

Whether the liver is the prime seat of these fevers, (which I am inclined to believe, but which is not for us now to determine,) one thing I think is very certain, that it is more uniformly involved or complicated with them, than any other organ. Though it has been maintained upon high authority, that these fevers are of gastric origin, to which doctrine I cannot assent, much less that inflammation is essential to their production—I cannot deny that gastric disorder is a very common (though not an invariable) attendant upon them. Nor is it strange that it should be so, when we look at the character of the organ, its size, the extent of its mucous surfaces, the number of its bloodvessels, its extensive nervous sympathies, and the readiness with which it sympathises with other organs; yet it is strange what it can endure without evident disturbance, when we see it receiving large quantities of crude, heterogeneous, and apparently indigestible substances; besides, other strong and irritating substances, such as salt, vinegar, pepper, mustard, wine, brandy, hot tea and coffee, or ice-water, and sometimes physic—all with apparently the same degree of
impunity. Now, these things I will not attempt to explain, but there is one thing which I think their existence serves to show, namely, that the stomach is not so sensitive to first impressions, or ready to take on morbid action from the influence of general causes, as it is through sympathy, or direct communication with other organs.

The liver, which may become congested from congestion in the lungs and heart, being itself a point for the formation of congestion, especially in the portal circulation, when that organ and system is in that condition, has the effect of throwing the blood back, or daming it back to the organs whence it came, thus obstructing the flow of blood through the capillaries of those organs producing such a state of plethora, or mechanical congestion in them as occurs in congestion of the brain. The effect of congestion of the liver is, therefore, felt in all the abdominal viscera, (itself included,) and most of these being secreting organs, besides the effects or consequences which are likely to result from the obstruction, and the accumulation and retention of an excess of blood in their capillaries, in the establishment of irritation and inflammation in the manner already described, they have their functions thus mechanically interrupted, without having necessarily labored previously under any morbid action. The consequence of this stagnation and accumulation of blood in the vessels of the stomach, from both forward and backward pressure, is to create a sense of fulness and distress in that organ, sometimes pain, and often nausea and vomiting; indeed, so common are these things to our fevers, as to have lent support to the doctrine that they are of gastric origin. But observation has taught me that they seldom occur in fevers where there is no hepatic obstruction, and seldom or never fail to disappear when the obstruction is removed. As a consequence of the accumulation and stagnation of blood in the mesenteric vessels, from congestion in the liver, we frequently have, as a concomitant of these fevers, a serous diarrhoea from the percolation of the thinner constituents of the blood into the intestines, and sometimes when these vessels are in an unusual degree or state of atony, blood itself is poured out in such quantities as to be both alarming and dangerous. Besides the spleen, which we may suppose to suffer in the ratio with other organs, the liver only remains to be considered among those which furnish the chief pathological and characteristic phenomena of these fevers, and as
I shall have occasion more than once to refer to it, I will next examine into the effects of general congestion upon the constitution of the blood, and the consequences resulting from the changes thereby induced. Whatever changes may take place in the constitution of the blood from excessive or defective secretions, and the agency which such changes have in the production of these fevers, (which we suppose to be no little,) belongs to another branch of the subject; but, there can be no doubt that as cause or effect, the stagnation of the blood in these organs (whereby its elaboration and its depuration are both arrested or suspended,) acts injuriously upon the constitution of the blood, rendering it less fit for performing its offices, and sustaining the great vital functions depending upon it, and thus favoring the continuance or increase of general nervous depression. This change in the constitution of the blood consists chiefly in its defective and imperfect oxygenization, consequent upon the feeble respiratory movement, and pulmonary congestion, and in an increase of the carbonaceous products from the same causes, together with the suspended functions of the liver, which products are known to possess powerfully depressing influences upon the nervous centres. Having thus examined into the pathological condition of the principal organs and systems, as they may be supposed to exist in the first stage, and at the commencement of intermittent and remittent fevers, the next step will be to examine them in connexion with the second or febrile stage, which must necessarily be deferred until my next letter.

Before I close, however, it may not be amiss to say, that any cause which is capable of producing great and sudden depression in the nervous centres, can bring about a condition similar to that which I have described, such for instance as heavy blows upon the head, violent injuries, strong or violent passions and emotions of the mind, the action of deadly poisons, &c. But the depression and congestion thus produced will generally be sudden, evanescent and transitory, leaving no traces behind; while the depression and congestion of these fevers is generally the work of their predisposing causes, which have operated for a length of time, and though they may not be so perfect and complete, are always more permanent than when suddenly induced. The subject to be continued in my next.

Yours, sincerely, &c.,

SAML. D. HOLT.

This is a question that has engaged my attention and observation for several years. In examining the standard works, I find considerable discrepancy of opinion upon this, as I regard it, important subject. Dr. Dickson, in his manual of Pathology and Practice, says, "a large proportion of writers of the past and present age consider this disease communicable by contagion; and within my own observation, so many circumstances have occurred, which seem to confirm the doctrine, that with Cullen, I dare not assert, that consumption is not contagious."

Dr. Dunglison says, he has had no adequate evidence, that it can be extended in this manner; yet, says he, "singular instances of the kind have been related by different writers, and if they prove nothing more, they exhibit strange coincidences." In Italy the contagious nature of this disease appears to be admitted by almost all medical men. At Naples, when an individual dies of Phthisis, his house, effects and furniture are destroyed; or if his house is not destroyed, the walls are scraped and whitewashed, and the ceilings, floors and partitions removed. Similar views are entertained at Rome, where the disease is much more frequent than at Naples. "If," says Dunglison, "a person be constantly breathing the deteriorated atmosphere of the rooms, which the consumptive occupy, by sleeping perhaps in the same bed, the health may ultimately suffer, tuberculous cachexy be induced, and finally confirmed Phthisis." Dr. Bell does not admit that phthisis can be communicated by contagion, but admits nearly the same thing in substance, though not intentionally; he says, "unhealthy air, whether from closeness, humidity, or impurities, combined with other causes, is a common cause of the constitutional origin of tuberculous matter." In Spain and Portugal, the contagious nature of this disease is so universally believed, that the clothes of those who have died of it are burned by the civil authorities. Morgagni, so frightened at its contagiousness, never opened the body of one who died of it, but that he evinced great nervousness. Morton mentions this as a contagious disease. Elliotson says, "I do not believe that phthisis is in the slightest degree contagious;" but just at the top of the same page he says,
"we see a family, brought up with every care in guarding against cold,—having good food, good clothing, and good lodging, and attention paid to the slightest indisposition; and yet, one after another, especially if they be females, often become the victims of this disease." This would go very far to prove the very idea, that he so flatly contradicts at the bottom of the page. He says, one is taken after another, and die, especially if they be females. This, it seems to me, may, with much plausibility, be accounted for in this way; one having a constitutional tendency to tuberculous cachexy, is exposed to bad air, as Dunglison says, or other external causes, and takes the disease; the others are in the room where he lingers out weeks, perhaps months, or even years of painful suffering; the air of the room, perhaps by close confinement, is kept in a contaminated state, and very soon after, if not before, the first dies, a second one perhaps is seen in the incipient stage of consumption. Now, Dr. Elliotson says, especially if the family be females; and why are females more predisposed to the disease than males? They are not as much exposed to the vicissitudes of atmosphere as the other sex, which is laid down at a common exciting cause of the disease, not only by him, but by most writers; but females are confined to the room to nurse, and soothe a brother's pathway to the grave, or to palliate the sufferings of a sister, as she slowly declines by mental gloom and decaying lungs. Thus it is reasonable to account for the greater frequency of the disease in females, than in males (ceteris paribus). It seems quite plausible to me, that, if bad air has any agency in the production of a tuberculous cachexy, or that it tends even to excite, or call into action, hereditary or constitutional predispositions to the disease, that it, in the same way, would produce the disease when coming fresh from the lungs of a patient with phthisis, while it is so fully contaminated, not with the common impurities produced by atmospheric changes, that are admitted to be causative of phthisis, but with a more concentrated virus, the exhalations of tuberculous matter itself, imbibed from the diseased air cells as the air passes through the lungs. What the nature of the virus may be, I do not pretend to know, or what its modus operandi. I can only say, that if the theory of Broussais be correct, which is, that phthisis is disorganization, which is the product of inflammation of the pulmonary parenchyma—though this is denied by some writers, others contend that irritation or hyperæmia is con-
nected with tubercular formation and development. I believe irritation to be the first step in the development of phthisis, which seems very presumptive, from the fact, that Bronchitis, or any other inflammatory disease of the respiratory organs, will hasten the development, or the fatal termination of this disease.

I have frequently seen Pneumonia, Pleuritis, Typhoid fever, and various other frebrile and inflammatory diseases grafted upon phthisis, which, as well as I recollect, invariably aggravated the disease. Some writers acknowledge that a chronic inflammation of the pulmonary tissue may be developed in the absence of any tuberculous tendency, eventuating in phthisis. It is generally believed, that certain employments may excite the disease, such for instance as stone grinding or dressing, and flint making. This fact is noticed in Berri in France, (a village,) where almost all the inhabitants follow the profession of making gunflints, and all of them die of phthisis, sooner or later. It is noticed too, that feather dressers, cotton manufacturers, needle grinders, labourers in coal mines and other dusty employments, seldom escape consumption, which cannot be attributed to any thing but to the irritation of the dust, consequent upon these employments. Climate has been always acknowledged to have influence in producing, or mitigating the disease, just in proportion as it was harsh and irritating, or mild and soothing. There is no climate entirely exempt from the disease; but there is a vast difference in countries in regard to the aggregate amount of cases of this disease, which is conclusive proof that a large proportion of the cases is produced independently of hereditary predisposition. I would ask, then, what can be the cause of so many cases among those who are employed in dusty situations? If it be not simply irritation of the dust, what can it be? Those thus engaged, generally are clothed and fed as other laborers of different occupations. I therefore conclude that irritation from dust in the air, as well as the harshness of cold, damp air, may be causative of this disease. I infer, then, if irritation be the first step in the development of the disease, it is quite reasonable to conclude, that the air, loaded with the virus exhaled from a tuberculous lung, may produce the irritation necessary for the production of the tubercle. So much for the plausibility, or possibility, of tuberculous contagion.

And now, I will give my reasons for my own suspicions of the contagiousness of this dreaded scourge of humanity. In 1843,
I was reading Physic in Harris county, Ga.; there lived a man in the neighborhood, whose lady was laboring under phthisis pulmonalis; she lingered for some considerable time, and finally died. Her husband, a stout and apparently healthy man, was necessarily confined to her bed-chamber closely in her last illness, soon took the disease, and in despite of all remedies, soon died also. This case, coming under my immediate observation, led me to notice many similar cases, that have since fallen under my particular notice. This man had no hereditary taint that I could find out, nor did he exhibit any serofulous or tuberculous diathesis, that I could perceive.

In a village near where I now reside, a gentleman was laboring for several years under consumption, and finally died. His wife, who was confined to his chamber in his whole illness, soon took the same disease, and also died. She, too, was unknown to have any hereditary predisposition, as none of her progenitors had died with the disease, as I could learn.

Many cases, of a similar nature have been related to me, but I only give such as have fallen under my own observation. I have seen quite a number of families, that, one by one, would fall victims to this disease; these would not be allowed to be adduced in evidence, as an inherited predisposition might be supposed to exist; but they followed in such quick succession to the grave, that I have been led to doubt that hereditary transmission had more, if as much agency, in the production of the disease than contagion. There may have existed a tuberculous diathesis in some, or even all the members of the families thus observed, but why they should all appear healthy, and clear of any serofulous tendency, until some one of the family took phthisis, is hard for me to account for, only by supposing that, if they had any predisposition to tuberculous nature, it was, by the contagious air of the sick room, brought into active development.

I am acquainted with a family, some members of which are now under treatment for phthisis; some have already died of the disease; none of them, I believe, was known to have any appearance of the disease, until two or three years ago, one of the family took it, and died; another, and another, until several have gone to an early grave—they followed in quick succession, three, I think, in one year. I know it may be, as it has always been said, that this was only an instance of predisposition; but why did these all en-
joy health until one of the family should die of consumption; then, in such quick succession, so many die in one family? There appears much plausibility in the conclusion, that if there really existed a hereditary tendency to the disease, that that tendency was, by some exciting cause, then brought into active development, to have produced the disease in several persons in so short a time.

I pen these observations to elicit the testimony of others of better opportunity. I hope that it will not be withheld on this highly important subject, and that correct information may be had.

ARTICLE XXII.

Sulphate of Bebeerine. By Robert Neilson, M.D., of Tuscaloosa County, Ala.

Sometime since, a friend sent me a few ounces of the sulphate of bebeerine, requesting me to test its power, and publish the result, if I thought it proper to do so,—which is my apology for placing this communication at your disposal.

The adulteration of quinine and its increasing scarcity, strongly demand the development of some other article to supply, if possible, its place. Occasionally an agent possessing some merit is brought forward, and with little trial, and still less accuracy observed, is discarded or placed under ban.

Such has been in part the history of the sulphate of bebeerine; mankind being ever prone to search for precedent, rather than endure the patient toil to discover things new and untried, prefer to take the views of the old world, than to call forth the energies of our native intellect.

I have experimented with several indigenous plants, but have found them inferior to the sulphates of quinine and bebeerine. The latter is inferior to the former in general application, but superior in some special conditions of the system. In cases of intermittent, prone to relapses, Fowler's solution is not more effective than bebeerine; or if diarrhoea attends them, its astringency and anti-periodicity are happily combined to arrest it. We do not intend, while now in its infancy, to place it side by side with quinine, but trust that whatever of merit it may possess, will be submitted to the closest investigation.
It is my opinion, that one reason why it has not been more approvingly noticed, is because its therapeutical application is not well understood. To succeed with any medicine requires a nice adaptation of its peculiar power to the diseased condition.

No skilful physician would think of giving opium in active inflammation of the brain, yet after depletion it may be highly useful. Thus, while the bebeerine possesses strong anti-periodic power, it is also astringent and slightly stimulant, I am satisfied from reason and actual experiment, that it cannot be exhibited successfully where inflammation or a tendency to it obtains. Nor can it be given during the paroxysm of fever without increasing vascular action; also, if exhibited in this stage it produces emesis, or nausea, to which it is more prone than quinine. It is more effective when used in the distinct remissions or intermissions of fevers, and hardly admissible during any other stage. It sometimes has an emetic effect, best obviated by combining with it some one of the tinctures of opium. Being soluble in water, I am accustomed to exhibit it in a solution of twenty grains to the ounce of water.

During the summer and autumn of the years 1854–5, I used the bebeerine occasionally in intermittents when relapsing, after quinine seemed to have lost its action, and generally with success. Pleased as far as I tried it, I determined to test its relative value as compared with other anti-periodics. Accordingly, twenty cases were treated with it, and the usual preparatory adjuvants,—seventeen of whom were cured, one (a case of tertian,) ran into the quotidian type, cured by quinine; the two remaining cases being threatened with gastritis, the remedy was abandoned, and they were cured in the usual way; but I am satisfied the bad symptoms were occasioned by over-eating. The case of quotidian type was a perfect failure under apparently favorable circumstances for its use.
Remarks on the blending of Periodical and Continued Fevers. By

AUSTIN FLINT, M. D.

I shall preface some remarks on an interesting and important subject, viz: the blending of periodical and continued fevers, by the report of a case of disputed type by W. J. Chenoweth, M. D., of Decatur, Illinois. This report was received some months since by my esteemed friend and colleague in the University of Louisville, Professor Rogers, and was accompanied by a request that it be also submitted to my examination. It was designed for publication if deemed advisable. The writer is a young practitioner of much promise. I may add that its publication, with remarks, is at the suggestion of Professor R. The reader will perceive that the case was the occasion of a difference of opinion among a number of physicians who visited the patient during the progress of the disease. The difference was purely one of opinion, and Dr. Chenoweth states in a postscript, that having read the report to Dr. Trowbridge, who saw the patient in consultation oftener than the other medical gentlemen, he expressed satisfaction with it as giving a fair history of the case, although differing from Dr. C. as regards the nature of the febrile affection.

A Case of Fever of Disputed Type.

"The following is one of a class of cases which have occurred in this town and neighborhood, and which have been called remittent fever by some of our physicians, and, by others, typhoid. Those of us who believe the disease to be remitting fever treat it with quinine, while those calling it typhoid fever prescribe opiates and stimulants. And inasmuch as the name of the disease carries with it different views of its pathology, and will almost necessarily govern the treatment, however guarded we may be, it may be useful to report a single case, so that physicians living in miasmatic districts may be led to give an opinion as to the disease and its treatment. We believe that the importance of being able to diagnose like cases is a sufficient excuse for laying it before the profession.

Sept. 23d, 1855, Rev. Mr. C. was attacked with a chill, followed by a fever. The chill and fever returned on the following day, and for three or four nights following he had "night sweats."

Sept. 30th he again had a chill. The next day he took ten grains of quinine, by the advice of a physician, and in a few days a dose of purgative pills. He had no return of the chill but did not feel well.

Oct. 7th. I was called to see him. I found him (in the forenoon) with a pulse 80 per minute; cool skin; eyes and surface of the body without capillary congestion; mind clear; bowels moved once the day before without medicine.

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I ordered 12 grains of quinine to be divided into three doses, one to be given every three hours.

5 o'clock, P. M. Pulse 90; surface of body hot; cheeks flushed; restless and wakeful.

Directed to wash him in cool water.

Oct. 8th, A. M. Learned that he was awake until after twelve o'clock, when he fell asleep and rested until about seven. Pulse 78; much the same appearance as on preceding morning.

Ordered quinine as on yesterday.

P. M. No apparent change since the same hour on the previous day.

Oct. 9th, 9 o'clock, A. M. Has been up for half an hour and has just got to bed. His breathing is a little more labored, but otherwise symptoms as on yesterday. His tongue in the forenoon has been moist, in the afternoon dry. It has been covered with a white fur, and has had a red spot at the tip. He has had liquid stools once or twice daily.

I did not see him in the afternoon of the 9th, but learned that although the paroxysm returned as usual at one o'clock, he was not quite as restless.

Oct. 10th. Very little difference in the symptoms from those present on previous days in the forenoon.

No treatment.

6 o'clock, P. M. Pulse 90; quite restless, (throwing his arms about and changing his position in bed;) skin hot; had been dozing a little and imagined he had a body of soldiers at his command, and gave them orders.

Oct. 11th. I called in my partner, Dr. Trowbridge. Pulse 84; breathing regular, but labored; tongue coated and dry; eyes clear; a slight defined redness on both cheeks; skin cool; has had as many as three or four brown colored evacuations during twenty-four hours.

Dr. T. thought the case was probably typhoid fever, but agreed to let him take 15 grains of quinine, divided into three doses. The medicine was left, but as the patient refused to take it, he had no treatment.

P. M. Pulse 96; skin hot and dry; stupid and restless; face sallow; tongue red in the middle.

At Dr. T.'s request we gave him brandy in small quantity, with orders to discontinue it if he was more restless, and substitute Dover's powder, three grains every two hours.

Oct. 12th, A. M. We learned that he had taken the three Dover's powders before he obtained sleep; but as this was at midnight, and he generally fell asleep and rested until morning, we did not know what credit to give to the medicine.

Dr. King was called in consultation, Oct. 13th, and advised to give quinine, but it was not given on account of difference of opinion, until the next day. The symptoms were about the same
as on the 12th, except that about daylight he is reported to have sweat.

Oct. 14th. Drs. King and Kellar saw him with Dr. T. and myself. Little, if any, difference in symptoms.

Dr. Kellar proposed to give him valerianate of quinine, which was agreed to by Dr. King and myself, thinking that the difference between that and the sulphate would not warrant us in declining to concur in the proposal. We ordered five grains three times a day.

Oct. 15th, 5 o'clock, P. M. Pulse 80; skin hotter than natural; somewhat deaf; drowsy; talking in his sleep; tongue red in spots, where the coating has fallen off; vomited once, water and bilious matter.

Oct. 16th, 9½ o'clock. Pulse 72; has been sweating for half an hour; two rhubarb colored motions since yesterday.

6 o'clock, P. M. Pulse 84; skin dry and hot; paroxysm of fever returned at four instead of one o'clock as previously.

Oct. 17th, 8½ o'clock. Pulse 78; eyes look well, no disposition to stupor; bowels moved once; a slight appearance of sordes on the teeth.

No medicine.

10½ o'clock, A. M. We were sent for under the impression that he had a chill. Mrs. C. had found his hands and feet cold, finger nails blue, and said that he was shivering; when we arrived his skin was rather hot, but he drew the bed clothes around him, and was evidently cold. This chill (or as Dr. T. preferred to think, nervous tremor;) lasted three-quarters of an hour. He then threw off part of the bed clothes, and appeared to be quite warm.

6 o'clock, P. M. Pulse 72; mind clear.

In consultation I declined to give brandy, and persisted in giving him 20 grains of the sulphate of quinine (divided into three doses) between twelve o'clock at night and daylight. He had taken on the 14th, 15th and 16th, fifteen grains of the valerianate each day.

Oct 18th, 9 o'clock, A. M. In a profuse perspiration; pulse 72; mind clear; bowels moved once since yesterday; tongue dry; (he has kept his mouth open, asleep and awake, during his illness.)

3 o'clock, P. M. Not sweating; otherwise as in the forenoon.

Oct. 19th, A. M. Pulse 72; mind clear; bowels not moved since yesterday; tongue moist, covered with a light fur; says he is a good deal better.

Oct. 20th, P. M. Had a slight paroxysm of fever at one o'clock.

Nov. 1st. Has been convalescent since I last saw him, and is now able to be up half of the day; wishes food, and complains only of muscular debility.

He has had a small red pimple on the neck, elevated above the surface, and disappearing under pressure; another under the right nipple; a third a little to the right of the median line over the
stomach; and between these last, two or three not so well defined. His wife says that mosquitoes have been abundant, and also that there have been fleas about his bed. I could not say that they were not the characteristic rose spots of typhoid fever.

We have been looking at the case from different stand-points, and have called in other counsel, (as the minutes will show,) One of them, Dr. King, saw him several times. He has practiced in this town and neighborhood for sixteen years. He agrees with me, Dr. Kellar, who has practiced here for three years, and Dr. McBride, just here from Ohio, (neither of them saw the case but once,) unite with Dr. Trowbridge in calling it a case of typhoid fever. As the case was under my care, and as Dr. King concurred with me in opinion, I persisted, as the notes show, in giving quinine in liberal doses. Dr. Keller preferred the valerianate, and we therefore gave it. Drs. Trowbridge and McBride declined giving the treatment their approbation.

The grounds for calling the disease typhoid fever were—

1st. Its continuance after the use of quinine, on the 7th and 8th—assuredly not an uncommon thing in remitting or even intermitting fever.

2d. The tongue was coated and dry in the middle. Dr. Flint does not think that the tongue furnishes any positive criterion of the disease.

3d. He had diarrhoea. This is not an uncommon event in ague, and is common in remittent fever, after the use of purgative medicine.

4th. On the 17th there was a slight appearance of sordes, a symptom acknowledged to exist in the typhoid condition occurring in remitting fever.

5th. There was capillary congestion. This was more intense in the afternoon, and presented a dull but defined appearance. His eyes were always clear, until convalescence was established, and then never congested except after sleep.

6th. He was stupid. This was not noticed until on the forenoon of the 12th; he had then been in bed for six or seven days, and the disease had evidently increased, and we may easily account for the stupor by congestion of the brain.

7th. He was deaf. He had taken a good deal of quinine.

8th. He had red spots on his neck and abdomen. My reason for not believing they were the characteristic spots of typhoid fever are hinted at in the report of the case.

The reasons for believing the case to be remittent fever are the following:

1st. The attack was preceded by intermittent paroxysms, recurring regularly on the seventh day after it was arrested, as is common in ague.

2d. There was a regular exacerbation, commencing at noon and
lasting until midnight. Oct. 7th, 8th and 9th, there was almost, if not quite, an intermission in the forenoon.

3d. The fever ended with a chill.
4th. There was no epistaxis.
5th. There was no tympanites."

Decatur, Ill., Nov. 5th, 1855.

W. J. CHENOWETH.

The following note, written subsequently to the following report, has an obvious bearing on the question as to the diagnosis:

"DEAR DR: The person whose case I recently reported to you, moved on Saturday last, Nov. 10th, to a new house, freshly plastered, (the room in which he slept was dry, but the next room was quite wet.) On Sunday he ate a small quantity of milk, (his appetite was good,) and in about half an hour he was seized with considerable pain, which he referred to the head of the colon. Injections were given, and two motions produced. He was then given opiates, but the pain continued. On Tuesday evening his bowels were tympanitic, and on Wednesday he died.

I suppose his death was not connected with his first attack as a sequence, but I think it necessary to mention the fact. He was able to walk about, had an excellent appetite, and bid fair to be entirely healthy, but was cut off at a time when hope had well nigh resulted in certainty.

Respectfully,

Decatur, Nov. 15, 1856.

W. J. CHENOWETH."

REMARKS.—Having introduced the foregoing report, an opinion respecting the diagnosis will be expected by the reader as well as the reporter.

I need not say that, in discriminating between diseases which may have many points in common, the data contained in a written description, however complete this may be, are much less satisfactory than the evidence afforded by personal observation. The account of the case, as given by Dr. Chenoweth, was undoubtedly prepared with a disposition to state the facts fairly, and in a truth-seeking spirit.

The circumstances, however, under which the phenomena were observed, and the history related, are such as to render it extremely difficult to divest the mind altogether of bias, and therefore the confidence of the author in the correctness of the position which he was led to take early in the progress of the disease, must not be taken into account in forming an impartial judgment concerning it. There are certain points on which it were to be desired that the report had been more full and explicit, viz: the condition of the abdomen as respects meteorism, tenderness, and gurgling; the state of the mind; the presence or absence of cough, and the bronchial rales; the period when the spots were observed, and a more minute description of their physical characters.

Judging from the symptoms detailed, and taking into view the occurrence of fatal peritonitis, probably from intestinal perfora-
tion, during convalescence, I must think that the evidence decided-
ly preponderates in favor of the conclusion that the disease was
typhoid fever. It is much to be regretted that the autopsical ap-
pearances could not have been ascertained. These, in all proba-
bility, would have sufficed, in connection with the antemortem
history, to determine positively the diagnosis.

Occurring, however, in a malarious district, the case presented
certain symptomatic events which belong to periodical fever, and
which rarely enter into the symptomatology of typhoid fever as
observed in parts of the country where intermitting fevers do not
prevail. I refer more especially to the recurring febrile paroxysms
preceded by chills, which characterised the early part of the history
of the case, and the occurrence of a single paroxysm at the termi-
nation of the disease. We have then, in this case, apparently an
intermingling of the events pertaining to both typhoid and remit-
tent fever; and Dr. Chenoweth states that the case was one of a
class of cases observed in that town and neighborhood, which
were regarded by some of the physicians as cases of remitting, and
by others typhoid fever. Now, is it not probable that there, and
in other situations where periodical fevers prevail to a greater or
less extent, the two forms of fever, viz., periodical and continued,
may be blended, giving rise to a hybride affection, the phenomena
of either species being manifested in different cases in varying pro-
portions? It is with reference mainly to this question that I have
introduced this case of disputed type of fever, and to this question
the remainder of my remarks will be devoted.

With the present amount of positive knowledge bearing upon it,
this is a speculative question. We have not data on which to
base a definite answer. The question can only be answered defi-
nitely by historical facts, which it will require not a little time and
labor to collect. We are not to prejudice the result of analytical
investigation by conclusions which may be hypothetically ration-
al. Nevertheless, here, as in other instances, a discussion profess-
edly on rational or speculative grounds is legitimate, and may be
useful by exciting and guiding the researches which will either
confirm or disprove the suppositions.

Let us clearly understand what is involved in the hypothesis of
the blending of periodical and continued fevers. It is a matter of
common observation, that in remitting fevers the occurrence of
remissions frequently ceases after a time, and the febrile move-
ment becomes continuous. It is not, however, on this account
regarded as nosologically transformed into continued fever.
Practitioners, it is true, often speak of remitting terminating
in typhoid fever. But this is a loose mode of expression,
which has given rise to not a little confusion. The remittent
fever puts on more or less of the external characters which
belong to typhoid fever; or, in other words, the patient lapses
into a typhoid condition common to a variety of affections.
This is all that can with propriety be said, and yet the pertinacity with which many who are not over nice in pathological distinctions, insist on the conversion of the one form of fever into the other, is a significant fact to which I shall have occasion presently to refer.* The two forms of fever are, in fact, reckoned distinct genera of the pyrexia. It may be logically, although not demonstratively proved that each proceeds from the introduction into the system of a special cause; that this morbific agent, or poison, in either instance, is capable of producing a certain definite series of morbid results, which, in the one case, give rise to the characteristic symptomatic phenomena of periodical, and, in the other case, to those of continued fever. The two genera are, therefore, considered as essentially distinct from each other. The poison improperly called marsh miasmata is capable of producing the species of fever called intermitting and remitting, and these only; while the special cause of typhoid fever, be it the matter of contagion, or not, will give rise to the species of fever last named, and none other. Now the question is, may these two different morbific agencies act in conjunction within the organism, the processes peculiar to each going on simultaneously, and consequently giving rise to an union of the symptomatic phenomena peculiar to each?

The affirmative answer to this inquiry by no means involves a pathological absurdity. The old doctrine that two diseases cannot co-exist in the same place and time within the body, is now obsolete. Observation has abundantly established that two essentially distinct fevers may run their respective courses simultaneously. Scarlet fever has repeatedly been known to be thus associated with measles and with smallpox.† The supposition, then, that periodical and continued fevers are capable of being blended, is sustained by analogy.

The special cause of periodical and continued fevers are alike impalpable, inappreciable. In the present state of medical science we can only study their pathological effects. We argue for the distinct specific character of these causes from the uniformity and peculiarity of the effects. This being so, the evidence in behalf of the blending of the two diseases must consist in the union, under

* The conversion, as distinguished from the blending of fevers, is a point which I shall not discuss. The reader is referred to an able article entitled the "blending and conversion of types in fever," from the pen of Prof. Dickson, of Charleston, S. C., contained in the Transactions of the American Medical Association, Vol. V., 1852. Prof. D. shows very clearly the improbability of a conversion of one species of fever into another, in the literal sense of that term. Two diseases, so special in their character, and so distinct as regards their phenomena and their causation as different species of essential fevers, in a certain sense become merged into each other by being blended, but it is not likely that either in reality parts with its individuality, or, in other words, that an actual metamorphosis occurs of the one into the other.

† For a collection of facts, from different sources, in support of this statement, the reader is referred to a note in Gregory on the Eruptive Fevers, by the American editor, Dr. Buckley, page 344.
certain circumstances, of the symptomatic characters of the two kinds of fever. I have already alluded to the fact so well known to physicians who reside in districts termed malarious, that cases of remitting fever frequently not only fail to preserve during the career of the disease remissions, but present many of the distinctive traits of typhoid fever; and, hence, it appears to the medical observer to be a matter of common sense that the former undergoes a conversion into the latter. Is it not reasonable to suppose that in these cases there is actually a combination or blending of the two diseases, albeit the diagnostic criteria by which they are discriminated from each other are sufficiently constant and reliable to enable the practitioner to make the distinction practically in the vast majority of cases?

I am disposed to advance a step beyond an affirmative answer to this question, and to ask whether it be not a rational view of remitting fever to regard it as always involving a blending of the intermittent and the continued. Why do we have in malarious districts, in certain instances, remitting instead of intermitting fever? It is not owing to the greater abundance of the so-called malarious poison, or its greater virulence, as was imagined by Dr. Eberle, for intermitting fever may be as pernicious, in other words, as severe and fatal as remitting fever, and it is fair, other things being equal, to measure the quantity and potency of the special cause by the degree of the specific effects. The specific effects of the malarious poison are intermitting febrile paroxysms, more or less complete and intense recurring at regular intervals. In remitting fever an additional pathological element appears to be involved—two separate affections, viz, a continued and a paroxysmal fever, are united. The disease is a hybrid.

This theory is favored by an inference from a well known fact, which I do not recollect to have ever seen cited for that purpose. The fact to which I allude is the indigenous development of typhoid fever, more or less abundantly, in different portions of our country, directly intermitting fever disappears, and the infrequency or absence of well marked cases of the former so long as the latter form of fever prevails. This fact has been observed in the locality in which I now write by those who have resided here for the last twenty or more years, and it is a fact which has arrested the attention of observing practitioners in various sections.* Now is it probable that the special cause producing typhoid fever is not developed, or is deficient, in miasmatic districts precisely so long as periodical fever prevails, and that, as a rule, it starts into existence or becomes more abundant just as the latter form of fever disappears? Essentially distinct as are the two poisons, why should the one, in this way, supplant the other? Is it not almost absurd to suppose that the typhoidal poison, as it were, obsequiously stands

* For a collection of testimony relative to this point, the reader is referred to Bartlett's Treatise on Fever. Edition of 1847, page 108 et seq.
aside for marsh miasmata, and patiently waits the departure of the latter before presuming to take its place, and assert its rank among the grand morbidic agencies of nature! On the other hand, is it not more reasonable to suppose that in districts at one time abounding in periodical fevers, in which subsequently typhoid fever becomes the prevalent form of febrile disease, the special causes of both were at the same time indigenous, and that the association of the two poisons in different relative proportions, gives rise to an union of intermittent and typhoid fever, the phenomena of either predominating according to the relative quantitative proportion of the efficient causes?

The speculative character of these remarks was conceded at the outset. The blending of periodical and continued fevers, either occasionally in cases of the former, which exhibit in a marked degree more or less of the characters belonging to the latter, or uniformly in cases of remitting as distinguished from intermittent fever, is to be established, or otherwise, by evidence more solid than that pertaining to the considerations just adduced, and others of a kindred character. These considerations claim attention only as suggestive of an interesting and important subject for scientific investigation. What are the requisites for an investigation directed to the points which have been raised? A few words with reference to this inquiry.

Bearing in mind our ignorance of the special causes involved in the production of both forms of fever,—of their nature and origin as well as the primary and essential morbid processes to which they give rise when they are introduced within the organism, it is plain that the supposition of their conjunctive operation is to be proved or disproved by the analytical study and comparison of their appreciable phenomena, that is, the symptomatic characters of the two kinds of fever. The data for this method of investigation are yet to be acquired. The natural history of continued fever, so far as concerns the species now known as typhus and typhoid, is pretty well ascertained. These fevers have been carefully studied by means of the analysis of recorded cases in different countries. As much cannot be said of periodical fever, especially the species or variety called remitting. The natural history of this fever cannot be considered as satisfactorily settled on the basis of that of the two species of continued fever just mentioned. Great as are the opportunities in various parts of this country, and in other countries, to collect recorded cases of remitting fever for analysis, it remains to accomplish for this disease what the labors of Louis, Gerhard and others, have done for typhus and typhoid fever. Something has been contributed toward an end so desirable by Stewardson* and Alfred Stille,† but the materials gathered by these distinguished

* Analysis of twenty cases of remitting fever in the Amer. Jour. of Med. Sciences, April, 1841, and April, 1842.
† Analysis of ten cases of remitting fever recorded by Dr. ———, of Baltimore, by Alfred Stille, M.D., of Philadelphia, April, 1846.
physicians, although valuable, are not sufficient. What a tempting field is here open to the clinical observer and historian! But of how many diseases may this be said? "Truly the harvest is great, but * * * * ."

The results of the analysis of different collections of recorded cases of remitting fever in different situations, including districts in which typhoid fever is seldom or never seen, and those in which it has become more or less prevalent, will establish the points pertaining to its natural history which distinguish the former from the latter; and by bringing these results together and comparing them with each other, it will be seen whether remitting fever maintains uniformly certain characters essentially distinct, or whether in proportion as cases of periodical fever disappear, and cases of continued fever succeed, the symptomatic phenomena of remitting fever manifest a transition to typhoid, showing a gradual predominance of the characters of the latter until those of the former are finally lost.

According to the general impressions of observing practitioners who have seen more or less of the disease, the descriptions by various authors, based on unrecorded experience, together with the results of analytical investigation (as yet far too limited) of recorded cases, certain differential characters of remitting and typhoid fever are generally sufficiently marked to serve as diagnostic criteria. It is, however, admitted that in a certain proportion of instances a positive discrimination is not easily made, especially in cases not observed from the commencement of the disease; and it is perhaps not incorrect to say that in certain situations it is oftener difficult to determine whether the disease be typhoid or remitting fever than to distinguish typhoid from typhus fever, in places where the two latter diseases prevail. As already stated, it is a matter of common observation that the symptoms generally characterised as typhoid, such as low, muttering delirium, a dry furred tongue, soreness, etc., are not infrequently developed in the course of remitting fever. The points to be settled are, to what extent are the characters considered as distinctive of each form really entitled to be regarded in this light, and of the characters which are truly distinctive, what significance or importance do they possess as representatives of the essential nature of the disease. Take for example the abdominal symptoms which belong to the natural history of typhoid fever, viz., diarrhœa, meteorism, tenderness, gurgling. These are undoubtedly wanting, as a rule, in cases of remitting fever; but are they not present in a certain proportion of cases, and if so, in what proportion? Again, complementarily to the inquiry just made, are the intestinal lesions supposed to be characteristic of typhoid fever, ever found in cases which, irrespective of their existence and of the presence of the symptomatic phenomena associated with them, would be entitled to be called cases of remitting fever? A statement to that effect is positively made by a distinguished
teacher and writer,* can it be substantiated by evidence adduced by the analyses of recorded cases? The same interrogatories may be applied to the eruption, and to other diagnostic events which are of more or less significance and importance in their relations to the essential or special nature of the disease. When these points are settled, as they may be, and as they alone can be by the accumulated results of numerous analyses of recorded cases, then we shall be prepared to bring logical proof for or against the doctrine that remitting fever, either occasionally or habitually, involves a blending of the special causes and essential pathological processes pertaining to the periodical and continued fevers.

In the mean time indulging, as we may do, theoretical views, (provided they are not permitted to engender pre-conceptions which will blind the perception of truth,) this doctrine is sustained by cogent considerations, and explains satisfactorily well known facts. Variation in the relative proportion of the periodical and the continued causative and pathological elements, serves to account for certain palpable differences in cases of remitting fever. We can understand that in proportion as the former elements predominate, the character of the disease will approximate to that of intermittents; the febrile exacerbations will approach to paroxysms, and the career of the disease will be arrested by anti-periodic remedies. On the other hand, the predominance of the latter elements will proportionately give rise to the phenomena which are characteristic of typhoid fever, in addition to continuity in the febrile movement. These are the cases which resist the sulphate of quinia in large doses, and which sometimes perplex even the accomplished diagnostician.

In the same way we may account for a fact which might be cited as tending strongly to sustain the doctrine. I refer to the prevalence of remitting fever in malarous districts during seasons when cases of intermittent fever are comparatively unfrequent, and vice versa. It is well known that the two forms of periodical fever (intermitting and remitting) do not observe any law of relative proportion in their concurrence. In one year intermittent fever may be rife, and remitting fever be rarely observed, while in another year remitting fever is abundant and intermittents not usually prevalent. Now, is this fact consistent with the notion that remitting fever involves the same special cause, and that only, which produces intermittent fever? Is it not, on the other hand, more consistent with the supposition that two special causes are conjoined, viz., that which is called marsh miasma and the typhoid poison? Adopting this supposition, when remitting fever preponderates, the typhoid poison has the ascendency. Under these circumstances, were the district not malarious, there would be an endemic of pure typhoid fever. But when intermittent fever is the prevailing form,

* Prof. Dickson, in his paper on the "Blending and Conversion of Types in Fever," already referred to.
the special cause of a purely periodical fever abounds, unattended by the typhoid poison.

In conclusion, the doctrine in behalf of which these few discursive remarks are offered, simply assumes that the special cause of typhoid fever* is not restricted, geographically, to territory free from periodical fevers, but existing in miasmatic districts, is associated in its morbid manifestations to a greater or less extent, with the phenomena due to the special cause of periodical fever, and, thus associated, gives rise to remitting fever, which is therefore a hybride affection. This, in reality, is only an extension to continued fever of a fact to which the experience of all practitioners residing in malarious districts will testify, viz: that, in general, the various affections originating in these districts are liable to receive important modifications from the conjunction of the malarious poison.—[Buffalo Med. Journal.

Form of Metamorphosis of Nerve and Muscle into Areolar Tissue.

A paper with this title appears, by Dr. Billroth.† After alluding to the observations of Blunnette and Schroder van der Kolk, on the changes which muscle and nerve undergo in the neighborhood of carcinoma, he goes on to speak of this change occurring, not as a specific carcinomatous degeneration, but essentially as a transformation of the muscles and nerves into areolar tissue. In hard carcinoma of the breast it unites intimately with the fascia of the pectoral muscles, and this again with the muscular substance itself, so that the muscle is drawn into the mass, and from the first point of growing together is arranged in a radical direction. One still distinguishes the fascia a long time after this growing together has taken place; but at a later period the tissues form such a firm cicatrix that one can, neither by the naked eye nor microscope, distinguish any of the original elements. The muscle passes right into the tumour, loses its dark red colour, and at last assumes a white glittering colour, but often the bundle-like arrangement is preserved. The like occurs in cancer of the lip. In investigating microscopically these spots of transition, a very careful tearing is required. First of all, a number of small cells and nuclei come into sight, and the muscular fibre is found to be very brittle, easily tearing transversely where the fibres immediately pass into the carcinomatous cicatrix, but one seldom can follow a free fibre very far. The muscular fibre first becomes less cross-striped in places assuming a more homogeneous and stringy appearance, and at the same time a new formation of tolerably dark oval nuclei arises in or under the sarcolemma of the fibres, which takes on a completely homogeneous glittering look. Whilst

* I limit this statement to typhoid fever, inasmuch as this is the species of continued fever chiefly indigenous in this country.
this change is progressing, new cells are formed between the fibres, and the tissue becomes so coherent that single fibres can only seldom be recognised, and the substance thus formed is no longer cleavable like muscle, but friable. The newly-formed nuclei compress the muscular substance, and afterwards appear to dwindle as the substance arising from the metamorphosis becomes much less nucleated than it was during development. The fore-mentioned process is the one most frequently met with, but yet there are many variations; for instance, the fibres may maintain their breadth, losing their cross-stripes, they may assume a fine punctuate bright appearance, with only a scanty formation of nuclei. In other cases the covering is filled with such a mass of nuclei that it appears as if the muscular substance passed into the new formation, and perhaps itself served as material for new formation. But these forms are seldom proportional, and may possibly be a deception, as this material does not correspond to single fibres, but only depends upon the coherence of the nucleated and cellular material deposited between the muscular fibres, which on mechanical grounds also assumes a cylindrical form. Along with these nuclei one sees a good number of fine spindle-cells, unaffected by acetic acid, which must be regarded as proceeding out of the cells deposited between the muscular bundles. This metamorphosis of muscular fibre is not peculiar to the neighborhood of cancer. The author relates a case of a boy, part of whose lip was excised in the Berlin Hospital, for the removal of a tumour, and the labial muscles adhering, the rete was found to be metamorphosed into strong nucleus-holding areolar tissue and elastic fibres. In other cases, such as the diffuse cavernous tumours, the transformation of muscle into areolar tissue may be seen.

Just as it is with muscular fibre, so do nerve fibres pass into a kind of matrix, whilst elongated nuclei form in their sheaths. A firm cancer was removed from the mamma. It had grown into the pectoral muscle, which was removed with it. At the place of transition of the sound muscle into the tumor, in a portion kept in acetic acid for twenty-four hours, an abundance of nerves more clear and numerous than usual was seen. In a thick nervous trunk, raylike extensions of the primitive fibres in a lateral direction were seen which, partly single, and partly united with small secondary branches, proceeded into the muscle. Here and there the bright-dark contours of the primitive fibres were seen, but for the most part the nerve substance had passed into a kind of matrix, and only a row of nuclei placed alongside each other indicated the original course of the fibre. One clearly sees in single places that the nuclei were imbedded in the sheaths of the nerve fibres, which were also in great part destroyed by reagents. This degeneration was advanced also in the neurilemma of the larger nerve branches. Our author considers that the pain often felt in cancer of the mammæ arises from the above-described new for-
mation of nuclei in the sheaths of the primitive nerve fibres by which the nerves are manifestly exposed to great pressure; and this the more likely, as the cancerous growths are almost free from nerves themselves. Probably something of this kind occurs in the fibroids of the skin and periosteum. The substance resulting from the above degeneration of the muscles and nerves become brittle, and swells up on maceration in weak acetic acid, as also in weak alkalies, being therefore not completely analogous to ordinary areolar tissue.—[British and Foreign Med. Chir. Review.

**Progressive Atrophy of Muscular Fibre.**

Virchow* relates at length a case of a man, aged forty-four, who was affected by progressive muscular atrophy. He had been affected, when aged twenty-one, with almost complete lameness of the extremities, supposed to be of rheumatic origin. His father had been similarly affected when aged forty. In this case the lameness began in the leg and spread upwards. The intestines and urinary bladder remained natural until his death. The muscles of the extremities were very emaciated, and of a pale reddish-yellow colour, some being entirely degenerated. Under the microscope they exhibited areolar tissue and fat-cells containing granular material, partly corresponding to the old muscular bundles in an uninterrupted way, partly not so. In some muscles the microscope also showed the presence of slender vesicles of 0,000—0,01 millimètres broad, containing very small fat corpuscles. Here and there were elongated nuclei, and in some places small round nuclei, showing a double contour on addition of acetic acid. These were partly single and partly heaped together from 2 to 7 in number, partly in files. These vesicles at times appeared quite isolated, with round extremities, and many had a more caudate character. In other places, where the muscle was redder in colour, the vesicles were broader, containing more numerous granules, mostly oval, and of 0,075 millimètres in length. The signification of these structures was difficult to decipher. Often there were evident fat-cells, surrounded by a membrane entirely uplifted from the fat drops, and with an oval nucleus. Areolar tissue bundles existed, with spindle-shaped very delicate corpuscles, which were mostly connected at their extremities; also spindle cells, broader, and filled with fine fat granules, which gradually became larger, and finally pass into large oval cells, containing large fat drops as well as the fine granules.

Finally, there were decided fat cells, only differing from ordinary ones in that along with a large fat drop they contained many smaller ones. Hence it appeared to Virchow that a new formation of fat-cells had taken place out of areolar tissue corpuscles.

Where the muscle was still more unaltered, the primitive bundles were delicately pale, with finely granular contents and incomplete striae. The arteries of the diseased muscles had fine granular fat in their walls. The nerves contained less fibres than usual, and on longitudinal as well as transverse section very broad intervening spaces were seen occupied by a very richly nucleated tissue; the nuclei were long, delicate, something pointed, almost like nuclei of organic muscle fibre, and in every direction much finely-granulated fat existed. The various nerves did not appear atrophied to the naked eye.

The spinal marrow, as well as the roots of the nerves, were healthy in look, but on section, even to the naked eye, a remarkable variation was seen, beginning at the upper cervical region and proceeding downwards, becoming gradually more marked, and most remarkable about the lumbar swelling. In all these places one saw in the posterior fibres of the chord, and more decidedly, near the posterior longitudinal fissure, a clearish grey, somewhat translucent mass, instead of the white nerve substance, which so extended into the under part of the medulla as to reach the posterior horn of grey substance. Here it so united with the grey matter that an obvious limit could not be seen. In general the degeneration began at the posterior longitudinal fissure, and proceeded thence into the substance of the posterior fibres. As seen by the microscope, only the posterior fibres, and not the horns, were affected. The change was of the same nature as that in the peripheric nerves, only that some broader nerve-fibres existed grouped together, which on transverse section were separated from each other by a distance of 0.005 to 0.012 millimètres. Between them existed a very soft friable granular material, containing thickly-strewed corpora amylacea, and also many granulated nuclei, chiefly oval, and here and there enclosed in round elongated cell membrane. No fat was visible, and the bloodvessels had a natural look. On the addition of chromic acid, instead of finely granular substance, much shreddy firm and fine fibrillated material was seen.—[Ibid.

On Jugular Venesection in Asphyxia, Anatomically and Experimentally Considered.

A paper on this subject was read before the Medico-Chirurgical Society of Edinburgh (March 19th, 1856,) by Dr. Struthers. The object of the paper, which was illustrated by preparations and drawings of the valves in the cervical veins of the human subject, was to ascertain whether distension of the right side of the heart could be relieved by opening the external jugular vein in the human subject. The experiments of Drs. John Reid, Cormack, and Lonsdale, had satisfactorily shown that, in the lower animals (dogs, cats, and rabbits,) the right side of the heart could be thus
disgorged so as to restore its action, which had been arrested by a simple mechanical cause, over distension. He considered that the indication of restoring the heart's action by jugular regurgitation, had not received that attention which Dr. Reid's suggestive paper demanded for it. Dr. Struthers described the anatomy of valves which he had found in the cervical veins, as well as those usually alluded to as present in the external jugular. A pair of valves at or within the mouth of the internal jugular vein; a pair in the subclavian vein immediately external to the point of union with the external jugular; a pair at or within the mouth of the external jugular; a second pair in the course of the external jugular, at the upper end of its sinus, or large portion, about 1 ½ inch above the clavicle, and various lesser valves at the mouths or within the tributaries of the external jugular. The varieties, and relative position of the two portions of each pair of valves was described, as he had found them in numerous careful examinations. With the view of ascertaining whether regurgitation could take place notwithstanding these valves, Dr. S. performed a series of experiments on the dead subject. A pipe was fixed in the femoral vein, and tepid water thrown freely upwards. The general result was, that the external and other jugular veins very soon became distended, and that when the lancet opening was made, at about an inch above the clavicle, the fluid regurgitated freely. At first a jet came, emptying the distended sinus, and then it continued to flow, never in a jet, but in an active stream across the neck, escaping by the wound with a wriggling motion, evidently due to the obstruction offered by the valve which it had overcome. Care was taken to ascertain that the fluid came by regurgitation, not from above; but, if allowed, it also came freely from above, having ascended by the internal jugular. The introduction of a probe so as to hold aside the guardian valve of the external jugular did not much accelerate the regurgitating flow. When the catheter was introduced, however, the fluid came very freely by it—as freely as from a distended bladder. It is easy to introduce a common male catheter to the vena cava or right auricle, by directing it backwards and inwards, as well as downwards, from the point of venesection. But as soon as the catheter has entered the subclavian vein, the fluid comes as freely as when it is pushed farther. As soon as the point of the catheter is withdrawn into the external jugular, the fluid ceases to come by it. In one subject the fluid could not be made to regurgitate. This was at the time attributed to the circumstance that the cranium had been opened for the removal of the brain, the fluid pouring out by the cranial sinuses; but, on dissection, two pairs of valves were found in the external jugular below the lancet opening, besides the pair above it, as usual. Regurgitation seems to be prevented by two pairs of valves, though one may be overcome. In these experiments the veins of the arm did not become distended, and no re-
gurgitation took place from a lancet-opening in the axillary vein, although afterwards it was seen that only two pairs of valves had stood in the way, between the heart and the opening. By "pair," Dr. S. meant the two separate portions which act together as one valve. He (Dr. S.) drew the following conclusions: 1. No venesection can be of any use in asphyxia, except in the neck, on the principle of regurgitation; which, however, may also relieve congestion of the head. 2. That, besides warmth and friction, and (the most simple and effectual of all means,) continued artificial respiration by alternate compression and relaxation of the sides of the chest, jugular venesection should be tried. 3. With reference to Dr. M. Hall's recent recommendation of the prone position, to prevent the tongue falling back and closing the glottis, the question occurred—Does the tongue fall back, under passive circumstances, in the supine position? Is not the closing of the superior glottis, under all circumstances, a muscular act—both the carrying down and back of the tongue and epiglottis, and the lifting upwards and forwards of the larynx? The mouth, however, should be cleared of frothy mucus. 4. That to obviate the evident risk of entrance of air into the veins, the wound should be closed as soon as regurgitation is about to cease, and artificial respiration be then commenced; the jugular venesection having been performed as early as possible.—[Edinburg Med. Journal.

Observations on the Cause of the Disease known as Sun Stroke. By Sanford B. Hunt, M. D.

As the season is now approaching in which we may not reasonably expect to witness occasional cases of this sudden and terrible malady, it may be useful to recur to it now as a subject, in itself interesting, and especially so to those who have made its causation and pathology a subject of study.

The name "sun-stroke," or coup-de-soliel, implies that it is produced by the direct rays of the sun, and its pathology has been almost universally conceded to be a sudden and intense congestion of the brain sufficiently severe to cause immediate death. Based on this view of the pathology, the treatment has consisted almost entirely of heroic blood-letting, and the application of cold to the head.

Unfortunately no one of these propositional or theories is proven, and perhaps it is not too much to state that no one of them is correct. It is the object of this article to consider briefly the questions involved in them, and taking them one by one, to bring to bear upon them such light as is afforded by the copious statistical tables which emanate from the health-offices of our principal cities. In conclusion, I shall endeavor to reconcile the facts of the disease so far as known, and suggest another theory of causa-
tion—one which I have already incidentally advanced on two or three occasions, but in which I have no especial claim to originality.

The first question for consideration is,

Are the direct rays of the sun necessary to the production of the disease known as sun-stroke?

Undoubtedly the larger number of cases occur in the open air, and in unshaded locations. But a very large number, so great as not to be considered exceptional, occur within doors or on cloudy days.

During the great epidemic of sun-stroke in New York, in August, 1854, of 235 deaths in that city from this cause, 49 were females. The argument here would be that as females do not live much out of doors, that some, at least, of these occurred under shelter. This supposition is confirmed by the fact that Dr. H. D. Swift, in his valuable paper on "Exhaustion from the Effects of Heat"—a monograph remarkable for the intelligence of its pathological views—mentions that "Eleven patients were attacked one morning in the laundry of one of our principal hotels; several were brought to us from a sugar refinery, where, after working several hours in a close and over-heated apartment, they fell down suddenly in a state of insensibility; and we had an opportunity of comparing their symptoms and lesions with those who became exhausted after laboring in the sun, but was unable to satisfy ourselves of any distinction."

Again, Dr. Reyburn, of St. Louis, in his "Report on the Diseases of Missouri and Iowa," made to the American Medical Association at its meeting for 1855, furnishes the following statement: "The cases of the disease that occurred in the last summer were not all traceable to direct insolation; the furnace-tenders in engine rooms, and bakers unexposed to the sun, were sometimes attacked. One case is reported to us of a female who had not been out of the house during the entire of a very hot day, being attacked."

It is evident, then, that insolation is not essential to the creation of this disease, but that it may occur in shaded localities, and even on cloudy days, for Dr. Reyburn's statistics show that four deaths by "sun-stroke" occurred in St. Louis on the 6th of July; and on reference to Dr. Engleman's meteorological tables, I find that this day there was a very cloudy sky, with rain and thunder. Moreover this was not a very warm day, the mean temperature being only 79°. I shall have occasion to refer to this day again, with reference to another meteorological condition.

SECONDLY.—Is congestion of the brain the special pathological condition present in this disease?

It is only recently that the fact has been fully recognized, that in the great majority of instances of sun-stroke, the symptoms have been those of syncope or exhaustion. Dr. Swift, in the paper before alluded to, proves from both symptoms and post-mortem
appearances that the latter is generally the true condition. Indeed, he says distinctly, that he has seen "a few, a very few cases, of insolation verified by a post-mortem examination,—certainly not one during the past year, although examinations were made in all the cases in which we suspected any cerebral lesion." And he gives one case where a hot head, suffused and injected eyes, contracted pupils, swollen countenance, coma and stertorous respiration, with "fair strength" of pulse, all seemed to indicate intense cerebral congestion; but after death none was found.

So far as we know, the question of congestion of the brain rests rather upon the symptoms than the pathological evidence. In a case which we witnessed a few years since, all the symptoms of intense congestion were present. We bled the patient freely from both arms. Soon after the flow of blood commenced he passed into the most violent convulsions, which were only discontinued on the production of profound syncope. Yet the symptoms next day did not indicate that congestion had been present. His recovery was rapid, and we have always felt some doubt as to whether congestion were really present in that apparently well-marked case.

At any rate enough is known to prove that contracted pupils and convulsions are not to be accepted as sufficient proof of congestion, and we must wait for actual post-mortem evidence of it to verify its existence. It is already plain that only a very limited number of cases are congestive, and it is quite probable that that limited number will be much decreased on careful study.

Dr. Swift has more correctly given the name of "Nervous Exhausation from the Effects of Heat" to the disease. The nervous exhaustion is proven, and it is also proven that the brain is rather anaemic than congested. But the cause assigned by Dr. Swift is not satisfactory to me, and I expect to prove that heat is not alone sufficient to act as a cause.

What are the facts in relation to heat as a cause of coup-de-soliel?

During the period of greatest mortality from sun-stroke in 1853, in New York, the temperature was, according to the register kept at the New York Hospital, not very high. During August, as we have already mentioned, 235 deaths occurred from this cause. It will be interesting to examine the weather record of this period. We find from it that the temperature at 3, P. M., the hottest hour in the day, was above 90° on three days only; on which it stood respectively at 92°, 91° and 90°. There were two days only on which it stood between 85° and 90°. There were five days on which it stood between 80° and 85°, leaving twenty-one days in this month of "excessive heat," on which the temperature ranged lower than 80°. With reference to insolation we may also note that twelve days were cloudy, and that it rained on eleven of them, the total amount of rain falling being large, viz.: 6.04 inches.

Turning to St. Louis we find that in the summer of 1854,
during a period of nine days, (the last four of June and first five of July) fifty-three deaths occurred from sun-stroke. The mean daily temperature of this period was 86°. Subsequently in July, another period of nine days occurred, the mean temperature of which was 82°, during which only seventeen deaths occurred. If temperature is the cause, why this disparity? Again four deaths occurred on the 5th of July, with a very cloudy sky, with rain and thunder, and a mean temperature of only 79°.

Here we leave the question of temperature. The facts adduced are conclusive that, though a certain temperature is necessary, neither the frequency or the fatality of the disease increase with a further rise of the thermometer. Heat then is not the essential cause; neither are the direct rays of the sun necessary.

What, then, is the essential condition for the production of coup-de-soliel, or rather exhaustion from the effects of heat?

I have been led to believe, from a careful survey of the premises, that a high humidity is the essential condition. This opinion is based upon weather records and upon analogy.

In the first place the condition of nervous exhaustion is never produced by a high, yet dry heat. The various experiments going to prove that a person may live and breathe without evil effects in a temperature high enough to cook a steak, or an egg, are too well known to need comment. Experimenters have borne for half an hour a temperature of 135°, without great discomfort.

No person, however, could live that time in a vapor-bath of that heat. The effect of surplus moisture in prostrating the nervous system, impairing the vigor of the heart's action and producing syncope, is as well known as the vapor-bath, and needs no comment. The symptoms of coup-de-soliel, in its ordinary form, are precisely similar to prolonged syncope. The exhausting influence of a sultry day, or of a hot sun directly after a shower, are familiar instances of the effect of high humidity upon the system. But the proposition remains to be proven by hygrometrical observation, and not by the uncertain perceptions of the senses. I purposely omitted to mention the hygrometrical condition of New York and St. Louis in the epidemics cited, because it was desired that the relation of heat in their causation should be tested by itself.

We may now, however return to it.

Mr. Blodget (then of the Smithsonian Institution) said of it (New York Journal of Medicine, Oct., 1853,) that "the temperature of evaporation at New York, at the time of greatest mortality in August, was from 80° to 84°, being higher than the maximum temperature of evaporation at New Orleans at any time in 1852, by 2°. Commenting on this point on another occasion, we made the following remarks:

"This implies, necessarily, a high fraction of saturation, and placing all the evidence together—the fact that the temperature
at 2 P. M., was only 90° to 92° (not an unusual heat for the season,) that the cases were mostly among foreigners, that Dr. Swift describes the symptoms as indicative of 'nervous debility,' and not of 'cerebral congestion,' that the dew-point reached a tropical maximum, and the conclusion is irresistible that, not dry heat, but a long-continued bath of aqueous vapor was the true cause of this unparalleled mortality. But, as if to make the evidence irresistible, we are told that 'eleven patients were attacked one morning in the laundry of one of our principal hotels; and 'several were brought to us from a sugar refinery, where, after working several hours in a close and over-heated apartment, they fell down suddenly in a state of insensibility.' I regard these last mentioned facts as of the last importance. 'Here, in a laundry, or in a sugar-refinery, unaffected by solar rays, filled with vapor artificially produced, having an excessive humidity, unventilated (for on these fatal days there was no wind,) men fell by dozens in sudden death. The experience of all time contradicts the idea that dry heat can produce these effects, and I regard them as conclusive upon the question, whether or no the combination of high heat and humidity is of itself a cause of disease."

Turning again to St. Louis, we ascertain from Dr. Engleman's tables, that during the period of nine days before alluded to, the mean temperature of evaporation was 78°, against a dry bulb temperature of 86°. The fraction of saturation is high enough to prove the presence of an immense amount of aqueous vapor, nearly as much, in fact, as could be forced into the air without having it in the form of steam.

Without multiplying statistics, I may state that the examinations of such weather records, corresponding with periods of mortality from sun-stroke, as have fallen under my notice, has uniformly resulted in fixing a high temperature of evaporation as the efficient condition of the cause of the disease, and that without definite relations to the dry bulb temperature.

A question of much interest connected with this theory of causation, is as to what power a humid atmosphere exerts on the absorption of the solar rays. I made last summer, some experiments, with a view to ascertain this point. So far as they went they seemed to prove that the difference between a thermometer in the sun and one in the shade, is greater on days of least humidity. The observations were, however, made without sufficient precaution against reflected heat, and will be repeated during the present summer with care. The method of making them is to use four thermometers: 1st, one with a dry bulb; 2d, one with a wet bulb—these two to be in the open air, shaded. A third thermometer should be exposed with a naked bulb to the sun's rays, while a fourth should be similarly exposed and wrapped around with black wool. Only clear days should be noted. In this way we hope to establish some relation between humidity and
radiation, corresponding with that which is evident to the senses in the chill produced on going into the shade on a humid day.

Buffalo Medical Journal.

Carbolic Acid as a means of artificially producing Premature Labor.

By M. Scanzoni, Professor to the Faculty of Würzburg.

Two years since M. Scanzoni proposed to provoke premature labor artificially by exciting the breasts by means of cupping glasses, and by reflex action to produce contractions of the muscular fibres of the uterus. This process, made use of on several occasions, often promptly induced labor, at other times it succeeded better as an adjuvant, while in many cases its effect was incomplete, or failed almost entirely. The application of the glasses frequently produced excoriation of the mamellæ, inflammation, abscesses, and more or less severe pain. Generally, the result of the mammary excitation was particularly evident, when the irritation of the nerves of the mamellæ was accompanied with local excitation of the womb, and when besides the cups, the uterine douche, the colpeurynta of Brown, the tampon, &c., were used. Desirous of finding a certain means of provoking uterine contractions without inconvenience to the mother, and without danger to the infant, and persuaded that artificial premature labor is one of the most useful and valuable resources of obstetrics, the professor of Würzburg continued his researches, and in the interesting practice which the maternity of Würzburg furnished, occasion soon offered upon which to experiment. Taking the observation of M. Brown-Séquard as a starting point, which shows that carbolic acid provokes the contractions of the muscles of organic life; that the genital organs for a long time exposed to the action of this acid become the seat of very severe congestion; and that it is even a sure means of curing amenorrhœa, M. Scanzoni resolved to employ this acid to arouse the contractile power of the uterus, and to excite it so as to bring about labor.

The apparatus employed was as follows:—A flask holding about a quart, hermetically sealed, with a stopper having two openings, by one of which a tube penetrated to the bottom of the flask, to the other orifice was fitted a horn pipe which connected with a caoutchouc tube about a foot long, ending in a canula of an ordinary injecting syringe. Bicarbonate of soda, and then some acetic acid is introduced by the first tube, a conical glass speculum is placed in the vagina. The caoutchouc tube inserted in a cork is introduced into the speculum, which it exactly fits. The carbolic acid is increased or diminished at will by the addition or not of acetic acid.

The following is the report of the case in which it was first employed:
D. S——, 26 years old, primapara, menstruated for the last time May 26, 1855; entered the Maternité of Würzburg Jan. 29, 1856. Pelvis low and narrow. The antero-posterior diameter, ordinarily from 4 to 4½ inches, is only 3½ to 3¾ inches. The vaginal portion of the neck is from five to six lines in length, and the external orifice firmly closed. The head of the foetus was felt above the anterior portion of the vaginal arch, the beatings of the heart were heard to the left, and the extremities of the foetus were felt to the right and high up, near to the bottom of the womb. The mother thought she was in the thirty-second or thirty-fourth week of gestation, and the examination of the genital organs confirmed this opinion. The narrowness of the pelvis preventing natural labor from taking place, and furnishing the indication for the induction of premature labor, M. Scanzoni resolved to try carbonic acid.

Feb. 2. At eight o'clock in the evening, the apparatus was applied for twenty minutes for the first time, without provoking any notable modifications.

Feb. 3. At eight A. M., application for twenty minutes, and at eight P. M. for half an hour. The mother felt while the gas penetrated into the vagina a disagreeable sensation of painful prickings, and during the day darting pains about the umbilicus. Evening—The vaginal portion of the neck was slightly softened. After a good and tranquil night the pains about the umbilicus recurred.

Feb. 4. The apparatus was used half an hour morning and night. The same prickings sensations during the application. The neck became dilated during the day so as to permit the finger to feel the inferior segment of the membranes. During the night, severe and darting pains in the groins and back; towards evening the hand placed upon the abdomen followed the evident contractions of the uterus, which, to tell the truth, soon after ceased.

Feb. 5. In the morning another application for half an hour, followed by the ordinary prickings sensations. The orifice was of the size of a two franc piece, yielded easily, and was readily dilated by the finger. The vaginal secretion is very much increased. In the afternoon the painful contractions of the uterus appeared, which increased in intensity by degrees. At half-past six in the evening the membrane broke, and an hour after a living child was expelled, which weighed 1850 grammes. During delivery a slight hemorrhage appeared, which necessitated the removal of the placenta a quarter of an hour after the birth of the child. The sequel of labor were not at all troublesome.

Reflections.—With the exception of the vaginal prickings, which seemed to continue only during the application of the current of gas, the employment of carbonic acid is followed by no serious inconveniences, and acts with sufficient energy, since its application during 3½ hours was sufficient to provoke the expulsion of the foetus. Unfortunately there is but one case, and it may be that this process may have some unpleasant results in nervous
women, and cause uterine spasms rather than normal contractions. The vagina may become irritated, and it is not clearly proved that the increased vaginal secretion was not caused by a commencement of vagiuitis. To decide upon the value of this method, the author himself calls for further experiments, and it is desirable that the demand of the distinguished physician of Würzburg should meet with some replies.—[Weiner Medicinishe Wochen-schrift, from Amer. Med. Monthly.

Is it always necessary to resort to Amputation when a Limb is attacked with Sphacelus?

Prof. Bardinet, of Limoges, has brought this important question before the Academy of Medicine of la Haute Vienne, and has answered it in the negative.

We are too ardent partisans of conservative surgery, having ourselves sufficiently often protested against the excessive tendency to operate everywhere and at all times, not to hasten to submit to our readers the reasons adduced by M. Bardinet in support of his opinion.

The following is the résumé of his memoir:

1st. In this memoir I report eight new cases of sphacelus (two of the finger, three of the forearm, and three of the leg,) in none of which amputation was performed. The task of eliminating the dead parts was intrusted to nature, except that her operations have been actively aided by the employment of the ordinary disinfectants, and especially by the early resection of the dead parts near the eliminatory circle.

In these eight cases recovery took place.

Had amputation been performed, it is, on the one hand, extremely probable that a certain number of patients would have died; on the other, several of them would have been deprived, in consequence of the necessity of amputating above the eliminatory circle, of a portion of their limbs (the knee, for example, or the upper part of the forearm,) which they are fortunate in having been able to preserve.

It is, therefore, not always necessary to amputate in cases of sphacelus.

2d. We should, above all, be extremely cautious in having recourse to amputation in cases of spontaneous gangrene—first, because in such cases, whatever we do, and even after the establishment of the eliminatory circle, we can never be sure that the gangrene will not reappear, and that we shall not thus needlessly add the pain and dangers of a serious operation to those of the original disease.

3d. Because the fear of amputating in parts whose vessels are diseased, obliges us to carry the section up to a considerable
height, and thus involves, sometimes very uselessly, the sacrifices of parts which might have been preserved, and the loss of which is to be lamented.

4th. Because the gangrene may attack several limbs in succession, and even all the limbs, of which I have quoted two examples, and we should then find ourselves compelled to perform a series of sad mutilations.

5th. Because, on the contrary, in confining ourselves to cutting away the dead parts near the circle of elimination, we perform an operation which is always practicable and always useful, as it liberates the patient from a focus of infection.

6th. Because we avoid the risk of performing an amputation, all the benefits of which will be lost if the gangrene makes fresh advances.

7th. Because, in adopting the new mode, we do not unnecessarily remove parts which the patient is much interested in preserving.

8th. Because we have still the power of performing amputation, if it should become necessary.—[Dublin Medical Press from Presse Médicale Belge.

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**On Liquidambar Styraciflua.** By Charles W. Wright, M.D., Professor of Chemistry in the Kentucky School of Medicine.

Liquidambar Styraciflua, commonly called sweet-gum, is indigenous to nearly every part of the United States, and constitutes one of our largest forest trees. When an incision is made through the bark of this tree, a resinous juice exudes, which possesses an agreeable balsamic odour. When this substance first exudes, it is of the consistence of turpentine, and possesses a stronger smell in that condition than it does after it has become resinified. Contrary to the statements made by Wood and Bache, in their *Dispensatory*, this tree furnishes a considerable quantity of resin in the Middle States, particularly in the States of Ohio, Indiana, and Kentucky, bordering on the Ohio River. It is annually collected in those States, and sold under the name of *gum-wax*. It is a much more agreeable masticatory than the spruce-gum, and is chewed in the West by nearly all classes. By proper incisions, one tree will yield annually about three pounds of the resin.

The chemical composition of the specimens collected in this latitude correspond with that given by M. Bonastre, of specimens gathered elsewhere, viz: benzoic acid, a volatile oil, a semiconcrete substance separated by distillation and ether, an oleo resin, a principle insoluble in water and cold alcohol, termed *styracine*. The bark of the tree contains tannic and gallic acids, to which its astringency is due.

What I wish more particularly to call attention to is the employment of a syrup of the bark, of this tree, in diarrhoea and dysentery,
and more especially the diarrhoea which is so prevalent among children during the summer months in the Middle States, and which frequently terminates in cholera infantum.

The best formula for the preparation of this syrup is that given in the United States Pharmacopoeia, for the preparation of the syrup of wild-cherry bark, of which the following is a copy, the sweet-gum bark being substituted for the wild-cherry bark.

"Take of sweet-gum bark, in coarse powder, five ounces; sugar (refined) two pounds; water a sufficient quantity. Moisten the bark thoroughly with water, let it stand for twenty-four hours in a close vessel, then transfer it to a percolator, and pour water upon it gradually until a pint of filtered liquor is obtained. To this add the sugar in a bottle, and agitate occasionally until it is dissolved."

The dose of this syrup for an adult is about one fluidounce, to be given at every operation, as long as the operations continue to recur too frequently.

One advantage which this medicine possesses over most astringent preparations is that of having an exceeding pleasant taste, and of being retained by an irritable stomach when almost every other substance is rejected. Children never object to it on the score of bad taste. The resinous and volatile bodies which it contains, no doubt enhances its value. My brother, Dr. J. F. Wright, of Columbus, Indiana, has employed this preparation for the last three years in a great number of cases, with the most satisfactory results. He prefers it to any other article where there is an indication for astringent medication in the class of diseases before referred to. In the bowel complaints of children it has a decided advantage over all preparations containing opium, and I am always pleased with the happy results which follow its employment in that class of patients.—[Amer. Jour. of Med. Sciences.

Quinated Cod Liver Oil. By M. Donovan.

A preparation of cod liver oil, called oleum aselli cum quina, has been lately introduced into medical practice, and is favorably noticed by some practitioners. It is probable that the tonic effects of quinine, conjoined with the restorative powers of the oil, may afford a combination of greater efficacy than is possessed by either separately. To many persons, the mawkish taste of the oil modified into the decided bitter of the quinine is an improvement. I have been informed that the combination of sulphate of quinine with cod-liver oil is effected by exposing them in a state of mixture to a certain temperature: if the heat be too high or too low, the combination, it is said, will either not take place or it will be subverted. I have made some trials with very unsatisfactory results, the quantity of sulphate which dissolved being very small, as might be expected from the character of sulphates in general.

Aware that the alkaline basis of sulphate of quinine possesses
some of the properties of a resin, it seemed probable that it might dissolve in oil; and, on making the experiment, I found that this is actually the case.

The alkaloid quinine is known to possess little efficacy as a medicine on account of its insolubility in aqueous liquids; hence, it is always administered in the state of acidulated disulphate, or, in other words, in the state of sulphate. Oil, by rendering quinine soluble, develops the medicinal virtues of that alkaloid, and thus, for every useful purpose, acts the part of sulphuric acid.

A few trials convinced me that quinine may be dissolved in cold cod-liver oil in even greater ratio than it is ever necessary for the purposes of the physician. A solution of eight grains to the ounce is intensely and persistently bitter. When the mixture is first made, a very disagreeable and peculiar smell is developed; but by exposure to the air for an hour or two, or better by filtering, the smell exhales and is dissipated. The colour of the oil is deepened by the combination.

This compound, which may be briefly named oleum aselli quinatum, has this advantage, that two active medicines, of coinciding effects, may thus be administered at one dose. To some, it is a severe trial to swallow either of them; and to such persons it would be a relief, instead of taking two separate disagreeable doses at different times, to swallow both at once, and have done with them.

There are constitutions which will not tolerate the free exhibition of cod-liver oil, and patients of this class are precluded from availing themselves of advantages which might have been of the utmost value to them. Perhaps the quinated oil would agree better with such stomachs.—[Dublin Med. Press.

New Method of Treating Phagedæna.

Mr. Cock has recently been trying, in Guy's Hospital, a plan of treating phagedænic ulcers by constant irrigation. The method is, to have the sore well exposed, and the affected limb placed on some water-proof material; a reservoir above the bed is then filled with lukewarm water, and, by means of an elastic tube, a stream is kept continually flowing over the surface of the sore. By this means all particles of discharge, etc., are washed away as soon as formed, and the ulcer assumes the clean, pale appearance of a piece of meat which has been long soaked. In all the cases in which it has been practicable to employ the irrigation efficiently, a speedy arrest of morbid action has been secured, and the number has included several in which the disease was extensive and severe. The theory of the treatment is, that phagedænic action is a process of local contagion—the materies morbi by which the ulcer spreads being its own pus. Admitting this supposition—which there is every reason for doing—to be true, the object to be kept in view in curative measures is either to decompose or to re-
move the local virus. This end is accomplished somewhat clumsily by such remedies as the nitric acid, which, unless so freely used as not only to clear up all the fluid matters, but to destroy the whole surface of the ulcer to some depth, fails to prevent a recurrence. Mr. Cock's plan of subjecting the ulcer to a perpetual washing attempts the accomplishment of the same end by a more simple and direct method. It involves no pain to the patient, and does not destroy any healthy tissues. Its one advantage seems to be, that, excepting on the extremities, its use would be attended with some inconvenience, from the difficulty of preventing the water from running into the patient's bed. Should, however, further trials confirm the very favorable opinion which has been formed at Guy's as to its value, these difficulties might, no doubt, be surmounted by the contrivance of suitable apparatus. The directions as to temperature of the water are that it should be as warm as comfortable to the feelings of the patient; and, as preventive of smell, Mr. Cock advises the addition of a small quantity of the chloride of lime or of soda.—[Med. Times and Gaz.

Chloroform in Cynanche Trachealis. By J. Jeffery, M. D., of Southfield.

As much has been written on the disease called Cynanche Trachealis, or Croup, and a variety of favorite remedies presented to the medical profession, which have excited the most ardent hopes for a short time, that something reliable had been discovered, which would not only inspire confidence in the physician, but remove the anguish of the sufferer, and dispel the terror and dismay that weighs down many an anxious parent; which hopes have mostly perished in embryo, it is with some diffidence that I present any thing on the subject fearing it may not sustain the confidence it has excited in my mind. I wish to report a recent case that came under my care, the treatment of which was entirely new to me, (but perhaps, not to others,) and so perfectly agreeable to the patient and satisfactory to myself, that the merits of the remedy may be duly tested by the medical profession. The patient, a lad between four and five years of age, had what the parents called whooping-cough, (but from their account rather a peculiar type or form, as also many cases that I have witnessed during the past winter,) while they lived in Redford. The family came here about the first of April, two of the children exhibiting some symptoms of the remaining disease. On the evening of the 23d ult., the patient came in from play, with a severe cough which alarmed the parents. I was absent from here. They got at my office a composition, prepared with Lobelia Sem., Ant. Tart., Lard and Licorice, administered it through the night with no benefit. On the A. M. of the 24th, I saw him and found him
with all the symptoms of severe croup, could only speak in a partial whisper; nervous system very irritable. Skin dry, considerable thirst, no cough of importance. The patient suppressing the effort to cough as much as possible. The shrill whistling sound of breathing could be heard for two or three rods from the house. Bowels had not moved for twenty-four hours; tongue with brownish coating. Gave him about 15 or 20 grs. of Hyd. Sub. Mur., followed with a teaspoonful of Sal Epsom. In the evening, bowels moved freely, and he appeared some better for a short time. Advised inhalation of vinegar and water, with the ordinary remedies. 25th, at 3 o'clock, A.M., was called to see the patient, as they thought he was dying. Found him unconscious, with the eyes half open and turned upwards, head thrown back, respiration feeble and exceedingly difficult, trachea apparently nearly closed up, pulse scarcely perceptible at the wrist, heart violently agitated, as if making its last struggle, extremities cold, &c. Under these discouraging circumstances, I concluded to try the effect of chloroform, in order to palliate the distressing symptoms and ease the patient through the portal of death. I put it on a handkerchief and placed myself by his side, allowing access to the air, holding it under the chin. In about ten minutes, the breathing was much relieved, the pulse became moderate and of fair strength at the wrist. The heart quite calm. I continued this about two hours, regulating the strength of the chloroform to the urgency of the symptoms. The patient seemed to fall into an easy slumber, the eyes closed; still the breathing was not natural, but so much improved that I began to feel some hopes of recovery. I left the chloroform in the care of the nurse, with instructions to use it sufficient to keep the patient quiet. Saw him again in the afternoon, found him with skin moist, and very much improved in strength, and all the urgent symptoms relieved, but coughing and raising his mouth full of tough phlegm, quite often, having the appearance of a pseudo-membranous substance, which continued for about twenty-four hours. The patient walking about the room part of the time. At the end of about thirty-six hours from the time of the commencement of the chloroform, he was able to eat nearly a full meal, and play around the house; a slight wheezing continued for about three days, since which he has been well. All the medicine I gave after the evening of the 25th, was a solution of Muriate Aminonia, half 5 to half pint of water and molasses, dose one to two teaspoonsful every two or three hours.

If you deem this of any importance, you can publish such part of it as you choose in your worthy journal, or dispose of it as you see fit. My only object is to aid the profession in their endeavors to benefit suffering humanity, and secure the confidence of community in medical science.—[Peninsular Jour. of Med.]
On Detection of Strychnia. By MARSHALL HALL, M. D.

The detection of strychnia as a poison is, at this moment, of deep public interest.

When the chemical test fails, there remains, I think, another—the physiological. Having long studied the effects of strychnia on the animal economy, (I have sent two papers on this subject to the Institute of France,*) I am persuaded that these effects on the most excitable of the animal species are at once the most delicate and specific tests of this poison.

I have just performed two experiments, and only two, for want of materials for more.

I requested Mr. Lloyd Bullock, of Hanover street, to dissolve one part of the acetate of strychnia in one thousand parts of distilled water, adding a drop or two of acetic acid.

I then took a frog, and having added to one ounce of water 1-100th part of a grain of the acetate of strychnia, placed the frog in this dilute solution. No effect having been produced, 1-100th of a grain of the acetate was carefully added. This having produced no effect, in another hour 1-100th of a grain of the acetate was again added, making the 3-100th, or about the thirty-third part of a grain. In a few minutes, the frog became violently tetanic, and though taken out and washed, died in the course of the night.

I thus detected, in the most indubitable manner, one thirty-third part of a grain of the acetate of strychnia. It appeared to me that, had more time been given to the experiment, a much minuter quantity would be detectable.

I placed the second frog† in one ounce of distilled water, to which I had added the 1-200th part of a grain of the acetate of strychnia. At the end of the first, the second and the third hours, other similar additions were made, no symptoms of strychnism having appeared. At the end of the fifth hour, the frog having been exposed to the action of 1-50th part of a grain of the acetate of strychnia, tetanus came on, and under the same circumstances of removal and washing, as in the former experiment proved fatal in its turn.

I thus detected 1-50th part of a grain of the poisonous salt by phenomena too vivid to admit of a moment's doubt; the animal, on the slightest touch, became seized with the most rigid general spasmodic, or, rather, tetanoid rigidity. And this phenomenon, alternating with perfect relaxation, was repeated again and again.

As the nerve and muscles of the frog's leg, properly prepared, have been very aptly designated as galvanoscopic, so the whole frog, properly employed, becomes strychnoscopic.

* See the Comptes Rendus for June 1847, and February 1853.
† These frogs were not fresh from the pools.
In cases of suspected poison from strychnia, the contents of the stomach and intestines, and the contents of the heart, blood-vessels, &c., must be severally and carefully evaporated, and made to act on lively frogs just taken from the ponds or mud. I need scarcely say that, taken in winter, the frog will prove more strychnoscopic than in summer, in the early morning than in the evening.

The best mode of performing the experiment also remains to be discovered, with all its details and precautions, an inquiry into which I propose to enter shortly. Meantime, this note may not be without its utility.

P. S.—I have repeated my experiment. I placed one frog, fresh from the pools, in an ounce of water, containing the 1-50th part of a grain of the acetate of strychnia; a second in the same quantity of water containing the 1-66th, a third containing 1-100th, and a fourth containing 1-200th. All became tetanic in two or three hours, except the third which was a female, (the other being males,) which required a longer time.

The 1-200th part of a grain of the acetate of strychnia is, therefore, detectable by means of this test conferred by physiology.

We now placed a male frog in 1-400th part of a grain of the acetate of strychnia, dissolved in six drachms of water. In three hours and a half it became violently tetanic.

The fresh frog is, therefore, at this season, strychnoscopic of 1-400th part of a grain of the acetate of strychnia, and probably to a much minuter quantity, which ulterior experiment must show.

In two other experiments the 1-500th and the 1-1000th of a grain of the acetate of strychnia were detected.—[Lancet.

New Form of Astringent Application. By Dr. William Bayes, Brighton.

Pure glycerine dissolves nearly its own weight of tannin, affording a very powerful local astringent application.

The solution of tannin in pure glycerine appears to me to supply a desideratum long felt, and capable of a great variety of useful applications.

The solvent property of glycerine over tannin, allows us to form a lotion of any desirable strength, as the solution is readily miscible with water.

The solution of tannin in glycerine, in one or other of its strengths, is peculiarly applicable to many disorders of the mucus membrane, readily combining with mucus, and forming a non-evaporizable coating over dry membranes; hence it may with benefit be applied to the mucus membranes of the eye and ear in many of its diseased conditions. It forms a most convenient application to the vaginal, uterine, urethral, or rectal membranes, where a strong and non-irritant astringent lotion is desired.
Radical Cure of Hydrocele.

A man, aged 31, has recently been under Mr. Lloyd's care, in St. Bartholomew's, on account of a hydrocele, which had been several times tapped, and on one occasion treated by the injection of iodine, with the hope of permanent cure. The latter expedient, however, had failed, the sac having re-filled. Mr. Lloyd adopted a plan which has long been a favorite with him, of introducing a little of the red precipitate into the sac. The fluid having been drawn off by a canula, large enough to allow a director to enter it, the latter instrument, oiled, and then dipped in the powder so as to carry a few grains adhering to it, was introduced and moved about in the cavity. The introduction was repeated two or three times; some inflammation followed, and a perfect cure ensued. The practice has the advantage over that by injection of not requiring any special apparatus. Mr. Lloyd believes it also to be more uniformly successful.—[Med. Times and Gazette.


Although we are not informed with regard to the number of the cases upon which Dr. Bamberger's remarks are founded, it is manifest that his experience is extensive, and his opinions therefore carry considerable weight. The cases which he does record are of much interest, and embrace almost the whole field of cerebral pathology. The following are the prominent points of his investigations to which we would draw the reader's attention.

Apoplexia Nervosa.—Pathological anatomy has so much narrowed the limits within which it is possible to apply the term nervous apoplexy, that we now rarely meet with cases to which it may be fairly given—viz., those in which sudden death occurs with cerebral symptoms, and in which no palpable lesion is discoverable after death. It is probable that the microscope and pathological chemistry may reveal minute changes that have hitherto escaped detection, and that the term, in its present sense, may have to be entirely eliminated from nosology. Dr. Bamberger is of opinion that sudden death resulting from violent emotions, electricity, and
concussion, must be classed in this category. He quotes one case that fell under his observation. A girl, aged twenty, previously in perfect health, was admitted into the Prague Hospital in January, 1850, having the evening before been seized with vomiting, followed by universal convulsions and unconsciousness, brought on by the information received in the morning of the same day that her lover had proved faithless. The temperature of the surface was elevated, the pupils unaltered, the eyes closed, the face pale, respiration stertorous, and the pulse intermittent. There was occasional spasm of the extensors of the upper and lower extremities, and also of the abdominal muscles. The extremities, when raised and allowed to fall, descended as if lifeless, though not actually paralytic. There was no return of consciousness, and she died twenty-eight hours after the seizure.

_Necropsy._ The brain was pale and anaemic, the walls of the left ventricle of the heart were slightly hypetrophied, the aorta very narrow and its coats thin, the heart and large vessels were full of loose coagula. All other organs were perfectly healthy. There was no suspicion nor any evidence of poisoning.

_Apoplexia Serosa._—We are still on debatable ground; for although the occurrence of sudden death, with symptoms of apoplexy, and exhibiting serous effusion into the ventricles, the substance of the brain, or the meninges, is undoubted, the majority of observers (as Abercrombie, Dietl, Wunderlich, Leubuscher) are of opinion that these cases are rarely, if ever, idiopathic. Dr. Bamberger has frequently met with the varieties of acute serous effusion alluded to, but is of opinion that they are always the secondary result either of cerebral diseases and abnormal state of the cerebral circulation, or of an altered state of the blood induced by some other acute or chronic disease, as granular kidney, typhus, acute exanthemata, tubercular, cardiac, and other maladies.

_Meningitis._—Dr. Bamberger adverts briefly to a few points connected with this subject, one of which is the occurrence of inflammation limited to the ventricular lining membrane; he is of opinion that where the post-mortem appearances indicate such a condition, a previous inflammatory exudation on the surface has been reabsorbed, or overlooked as an unessential concomitant.

_Cerebral Hemorrhage._—The author refers all cases of hæmorrhage to increased pressure in the vascular system, or to an altered condition of the blood, but from the alterations previously induced in the coats of the vessels. He admits that the latter lesion has not yet been demonstrated. As but few authentic cases of passive hæmorrhage within the cranium are on record, he relates some that have fallen under his own observation in typhus (typhus peticchialis,) scurvy, and chlorosis. The rarity of the occurrence in typhus is shown by the fact that Dr. Bamberger has only met with it once in above a thousand cases of the disease. In that case, after death, which had ensued on the thirteenth day of the typhus,
in a boy, aged fifteen, a cavity of the size of an egg, containing blood that was slightly coagulated, was found in the right corpus striatum. This was also the site of the apoplectic spot found in a girl, aged twenty-five, who died suddenly while under treatment for intense chlorosis. In scurvy, which the author has repeatedly found almost epidemic, he has also met with apoplexy in a girl, aged twenty-three, in whom numerous small apoplectic spots were found closely aggregated in the right anterior cerebral lobe, besides another large extravasation on the convexity of the left posterior lobe.

We must pass over the author's observations on the uniform occurrence of the crucial paralysis shown with reference to the facial, fifth, oculomotor, optic, and acoustic nerves; on the rapid return of sensibility, compared with that of motility, in the paralysed half of the body; on haemorrhage into the pons, the sae of the arachnoid, into the tissue of the pia mater, and the grey matter of the brain.

Red softening occurs in three forms; it may be latent and accompanied with such trifling symptoms as not to induce a suspicion of a cerebral affection; it may be accompanied by symptoms of apoplexy; or it may manifest a very chronic form, in which we meet with the most varied symptoms of cerebral irritation and compression. It is only in the last variety that a diagnosis is possible, though even here there are numerous sources of error. A very peculiar case is detailed, in which the author assumes the conversion of the ordinary products of normal inflammation into tubercle—a view which is certainly at variance with the prevailing opinions on tubercle and the tubercular diathesis. The case is briefly this. A female, aged thirty-five, was seized in the fifth month of her seventh pregnancy with pneumonia, which lasted three weeks; about three weeks later severe headache was followed by sudden rigidity of the left extremities, the fore-arm and leg being flexed; severe convulsive movements of the same extremities ensued lasting a few minutes. There was no unconsciousness, though she was slightly giddy during the attacks. The rigidity and the temporary spasms continued for a week, when she was admitted into the hospital (November, 1851.) She was able to answer questions, but her memory was somewhat impaired. There was occipital headache, paralysis of the left side of the face, violent contraction of the right trapezius, of the left arm and leg; attempts to overcome the flexion caused severe pain. Sensibility of the parts unimpaired, total loss of motility; some improvement took place in the paralytic condition, but in December an epileptic seizure supervened; delivery followed in the same month; further epileptic attacks ensued, with pleursy in the right side, and advancing tubercular disease of the lungs. Death on the 27th January. The state of the brain was as follows:—On the inner and upper surface of the right hemisphere, a portion of the size of a desert plate exhibited
intimate adhesion between the membrane to the brain by means of a greyish-red cellular tissue, and a yellow cheesy friable mass; the subjacent gyri were converted into a similar substance to an extent of 9 to 10 lines, not circumscribed as cerebral tubercle generally is; the cerebral tissue in the immediate vicinity was reddened and softened, the more distant portions almost pulpy. Old and recent tubercles were found in the apices of both lungs; the liver and spleen also showed tubercular deposit. Dr. Bamberger argues that the symptoms showed that the cerebral disease commenced with inflammation, and that therefore the deposit in the brain was the result of a conversion of plastic exudation into tubercle; but it necessarily suggests itself that the tubercular deposit may have been long dormant in the brain, and that the inflammation was a secondary affection. Until such cases are multiplied, it appears illogical to adopt a theory which is opposed to the common experience of pathologists. Two interesting cases are given of encephalitis, resulting from plugging of the arteries by fibrine carried from other portions of the circulating apparatus.

With regard to cerebral abscesses, Dr. Bamberger only confirms the known fact of their remarkable latency. The details of three cases are introduced in evidence.

Paralysis Agitans.—In one necropsy of a female, aged forty-five who had been subject to constant tremors of both upper extremities and the head from her childhood, the meninges were found opaque, and infiltrated with serum of which two ounces were found in the ventricles; the brain was otherwise normal. The characteristic feature was found in the spinal cord, which was white and moist, and exhibited throughout the white matter numerous grey, gelatinous spots; from the middle of the cervical to the middle of the dorsal portion there was a central canal, admitting of the passage of a probe. Dr. Bamberger regards the gelatinous spots as the residue of previous inflammation, and the formation of the canal as the result of atrophy of the cord.

Encephalic Tumours.—The diagnosis of encephalic tumours still remains, to a great extent, a matter of guesswork, the symptoms being mainly those of compression, which they share equally with other affections. Of 17 cases observed by Dr. Bamberger, 11 occurred in men, 6 in females — ratio established by Lebert and Friderich. They were distributed over the different periods of life as follows:—Under ten years, 1; ten to twenty, 3; twenty to thirty, 4; thirty to forty, 4; forty to fifty, 2; fifty to sixty, 2; sixty to seventy, 1. Six were large tubercular or tuberculoid masses; 2, cancerous; 2, fibrous tumours; 2, simple cysts (not apoplectic); 1, echinococcus; 1, extended hard masses, of an undefined character; 2, osseous tumors in the cerebral tissue; and 1, cholesteatoma. In 10 cases the cerebrum, in 5 the cerebellum, and in 2, both were affected.

The most uniform symptom was cephalalgia: this was absent
Obliteration of the Thoracic Aorta. [August,

only in two cases; it was severe and paroxysmal in 6. Paralytic affections occurred next in order of frequency—viz., 10 times; in 5 gradually, in 5 suddenly. Convulsive attacks were met with 8 times; 7 in the form of epilepsy (6 of these with cerebral, 1 with cerebellar, tumours;) 1 in the form of convulsive affections of one side of the face. Derangement of the intellectual functions occurred in 8 cases.

The details of 3 cases of encephalic tumours, for which, however, we cannot make room, conclude Dr. Bamberger's interesting communication.—[Brit. and For. Med. Chir. Review.


At a meeting of the Medical Society of Vienna, held on the 19th October, 1855, Professor Skoda introduced a man affected with obliteration of the thoracic aorta. In illustration of the lesion, the Professor exhibited preparations of a five-months' foetus and of a new born child, in which he indicated the point at which alone this anomaly can take place or has hitherto been observed. It is the point at which the ductus botalli communicates with the aorta and the short space intervening between this point and the origin of the left subclavian artery. During foetal life, this portion is commonly narrower than the remainder of the aorta, and only acquires the same calibre after birth.

The individual in question was a man, aged forty-seven; a jeweller; of normal complexion, and throughout well nourished. On the whole, he enjoys good health, and has only come under clinical observation owing to his having, for three years past, suffered from some dyspnoea in making violent exertion. This is due to an insufficiency of the tricuspid valve, which has only been established for three years.

The following are the grounds upon which Professor Skoda has diagnosed a co-existing obliteration of the aorta:—In addition to the blowing murmer coincident with the impulse, and which indicates the above-mentioned insufficiency, a "peculiar vibration or whirring (schwirren) is to be perceived over the greater part of the thorax, partly by palpation, partly, as in the course of the intercostal arteries, by auscultation; it follows the impulse, and for that reason has its seat in the arteries. The vibration of the arteries of the thorax is due to their dilatation, as may be shown by touching the superficial epigastric arteries, which are much dilated and very tortuous. The beat of the crural arteries at the groin is very feeble, and no pulsation can be felt in the abdominal aorta."

These are the indications characteristic of obliteration of the thoracic aorta; the collateral circulation is carried on by the branches of the subclavian arteries, which must therefore be dilated.
A large volume of blood passes from the anterior intercostals to the posterior intercostal, and by centripetal movement reaches the descending aorta, which is thus filled with blood sufficient to supply the arteries of the intestines, but not sufficient to produce distinct pulsations. The inferior extremities probably also receive a supply by the the anastomosis of the superior and inferior epigastric arteries. No cyanosis is observed, because nowhere venous blood is introduced into the arterial system.

In connection with this case, Professor Skoda made the following remarks:—1. That in examining the heart, we occasionally perceive murmurs which give rise to the assumption of valvular disease, while the heart is afterwards found healthy; and that the murmur was produced in the coronary arteries or in other arteries, in the vicinity of the heart. Such errors can only be avoided by carefully attending as in the case detailed, to the coincidence or non-coincidence of the murmur with the movements of the heart. 2. The circumstance that the nutrition of the individual was unimpaired, although the circulation in most of the organs must be, doubtless, shooken, proves that the deranged nutrition, so frequently coinciding with impediments in the circulation, does not depend solely upon the latter.

Professor Skoda was of opinion that the obliteration of the aorta was due either to a complete obliteration or absence of the corresponding portion of aorta in the foetus, or to the contraction of the latter coincidently with the ductus botalli, owing to the exceptional extension of the tissue of this channel into the coats of the aorta. Professor Skoda maintained that the obliteration could not be set down to inflammation, as arteritis led, not to obliteration, but to aneurism. He referred to an analogous case which had occurred in his wards some years previously, where no disturbance of function was manifested until, accidentally, endocarditis supervened. Death occurred later from pneumonia; and the obliterated aorta has been preserved in the anatomical museum of Vienna.—[Ibid.

Case of Punctured Fracture of the Cranium, and Wound of the Brain, with loss of Cerebral Matter, without the occurrence of corresponding serious symptoms. By M. Morton Dowler, M. D., of New Orleans.

Instances of recovery after the most formidable injuries of the brain are not frequently recorded, and have, in some cases, not a little contributed to overthrow the theories of physiologists and psychologists, demolishing, at once, as with a "knock-down argument," the skullbump psycholgy. The crowning case of Gage, related in the July, 1850, number of the "American Journal of the Medical Sciences," affords an exemplification, which coming from a less reliable source, would be regarded as almost incredible.
It has been seen in this case that a tapering iron bar, of the length of three feet seven inches, and of the diameter of one inch and a quarter, may enter beneath the zygoma, and pass out at the junction of the sagittal with the coronal suture, passing through the anterior lobe of the left cerebral hemisphere, and that the subsequent report may be, as in this case, that "the patient has quite recovered his faculties of body and mind, with the loss only of the sight of the injured eye." Nevertheless, whatever may be the deductions afforded by exceptional and extraordinary cases such as this, all surgery gives us emphatic warning that in cases attended with any manner of lesion of the brain, its blood-vessels, its meninges, or its bony protection, the gravest and most serious results should always be apprehended and guarded against, on the part of the attendant. A patient whose brain has been laid open, and the proper substance of the same wounded, should be considered as being in both immediate and ultimate peril, and should no urgent or alarming symptoms whatever occur during the treatment of such case, it may be considered as a remarkable exception, and the more especially where the patient is of tender age, and has received a severe punctured wound. Of such exceptional kind is the following case, which is not like the case of Gage, given as an extraordinary case of mere recovery, but as exemplifying recovery without any symptom corresponding to the gravity of the injury sustained, being in this respect the most remarkable I have ever witnessed.

On the 3d day of September last, a little boy, Louis, son of Mr. R. D. Maclin, of the Fourth District of this city, received a punctured fracture of the skull, and penetrating wound of the brain, under the following circumstances: a negro servant girl ascended a shed, about 12 feet from the ground, for the purpose of driving a nail, using, in place of a hammer, a large male hinge, weighing nearly two pounds, which had been drawn from the post of a wide gateway; and after effecting her object, without taking the precaution to look downwards, she threw forcibly from her hand the hinge, which descending, struck the child on the parietal bone of the left side, an inch and three-fourths from the coronal, and one inch from the sagittal suture, the post-spike of the hinge presenting, and entering the brain. The child was at the time sitting with the head erect, and the iron entered in nearly a perpendicular direction. The spike of this formidable iron is a four-sided body, six inches long, gradually tapering on all sides, but so flattened latterly as to triple the width of the horizontal surfaces, thus terminating in a wedge, the edge of which is half an inch long, and which is dull and battered. The iron penetrated about an inch, passing into the medullary matter of the brain, making by the tapering spike, an external opening three-fourths of an inch long, and one-fourth of an inch wide. The great weight of the butt end of the hinge, and its slight deviation from the perpendicular-
lar direction of the spike, caused it to be swayed over across the sagittal suture, the thin parietal bone affording no other resistance than as a fulerum on which the whole iron became a lever of the first kind, to injure the brain in the direction of the parietal protuberance, and the child’s body was thereby drawn over to the right, and he was found with the right side of his head on the ground. Mrs. Maclin ran to the child’s relief, and drew out the huge spike from his head, and she saw particles of cerebral matter adhering to the rough, rusty iron, and also escape from the wound. The blood at first escaped pretty freely, but soon ceased to flow. The force and weight of the iron was such, that it produced a simple oblong opening the exact shape of the spike, without there occurring any surrounding depression, or radiating fracture, the displaced bone being comminuted into small particles, as is believed. But few of these latter were ever found, and must have cleared the wound during suppuration, otherwise they involve a mystery. After the transient primary shock had subsided, none of the symptoms of concussion or compression of the brain manifested themselves; nor did they subsequently, the child relating to his father, in an hour afterwards, how the accident happened, and inquiring “if he must die” from the injury.

Dr. W. P. Sunderland, the family physician, was sent for, and was soon in attendance. Very reasonably regarding the case as one likely to be attended with the gravest consequences, it resulted that I met him in consultation, and was fully impressed with the justice of his apprehensions. He had sponged the wound, and made the only topical application subsequently resorted to—a simple compress saturated with cold water. We engaged to meet twice a day and watch the progress of the case. The patient never at any time labored under any apparent urgent symptoms, excepting during the second and third days; nor was any medical treatment found necessary, or resorted to, excepting the administration of an occasional saline aperient. Excepting during these two days, there was but little febrile irritation or pain; there was freedom from delirium, from coma, and the intellectual manifestations were unchanged, the wound soon beginning to suppurate, and to rapidly heal.

During the second and third days there was considerable nausea and uneasiness of the stomach. The patient was kept for many days strictly in the recumbent position. I discontinued visiting him at the end of ten days, and he was subsequently under the care of Dr. Sunderland. Towards the close of December the wound completely healed, and a firm membranous cicatrix now shows the seat of the injury. The patient is a child of great intelligence, and his faculties have in no way suffered from a wound in which there has been a loss of cerebral matter amounting, as Dr. Sunderland and myself both estimate, to at least a drachm in weight.
In neither the effects of injuries nor from the effects of remedies can we calculate on uniform results. The most inexplicable peculiarities and individualities interpose themselves, so as to render an ordinarily salutary remedy pernicious and an ordinarily fatal injury a thing of ready cure. Much here remains to be elucidated before the depths of pathology and therapeutics can be considered as explored.—[N. O. Med. and Surg. Journal.

On Simple Ulcer of the Stomach. By M. Cruveilhier.

M. Cruveilhier has recently presented two papers to the Académie des Sciences upon this subject, and the following are the general conclusions:—1. There exists a disease of the stomach that may be anatomically characterised as simple ulcer of the stomach, usually chronic. 2. This lesion, which is far more common than is usually supposed, differs from cancerous ulcer, with which it is generally confounded, in its curability. 3. It is susceptible of complete cicatrization, this being accomplished by means of very firm fibrous tissue, differing essentially from scirrhus, with which it has been confounded. 4. When the ulcer penetrates through the whole of the coats of the stomach, the loss of substance is repaired by surrounding organs, which also sometimes participate in the ulceration. 5. Danger may continue even after the cure of the ulcer, as the cicatrix often becomes the seat of consecutive ulceration, with all its attendant accidents. 6. It is one of the most frequent causes of blackish vomiting and defecations, and the most frequent one of hæmorrhage of the stomach whether accompanied by hæmatemesis or not. 7. Simple ulcer is the most frequent cause of perforation of the stomach. 8. The two principal accidents are haemorrhage and perforation, which take place more commonly consecutively, i.e., by the erosion of the cicatrix, than primarily, or during the period of formation of the ulcer. 9. This ulcer, or ulcerative gastritis, may be always suspected, and almost always positively diagnosed. 10. It is distinguished from idiopathic gastralgia by the permanence of the symptoms it gives rise to, although these have alternations of exasperation and remission. Gastralgia is only temporary, comes and goes suddenly, leaving no traces of its presence, and may be suddenly relieved by opiates. 11. It is distinguished from non-ulcerative gastritis and gastralgia by black vomit and stools. It is very probable, however, that simple ulcer may exist without these discharges, and then its diagnosis from gastritis would be difficult. These black discharges are not characteristic of cancer; and, to some extent, are more inherent to simple ulcer than to it, for they belong to all periods of simple ulcer, of which they constitute the first symptom, while cancerous ulcer is not attended with them until the last stage, and sometimes not at all. 12. The
distinctions between simple and cancerous ulcer are founded on, first, the physical signs, there being no tumor in the former; and, next, on the pain which is often absent in cancer but never in ulcer. The pain in the latter is like that of an open wound or burn, opposite the xyphoid appendix, striking through to the spine. In cancer there are cramps or spasmodic contractions, with induration of the stomach. 13. The true touchstone is the effect of alimentary regimen, which completely fails in cancer, but succeeds surprisingly in ulcer. 14. The great object in treating the disease is to find an aliment that is tolerated by the stomach without pain, for then the cure may soon be effected. In the immense majority of cases, milk diet induces improvement from the very first day, and sometimes operates like magic; but when it ceases to be agreeable to the patient, or fatigues the stomach, we must unite it with other substances, in the choice of which the instincts of the stomach must be consulted. Alimentary regimen, in fact, constitutes the entire treatment, but nothing can be more difficult than the direction of this, according to quantity, quality, repetition, preparation, and temperature. 15. Medicinal substances, whether general or topical, are quite secondary in importance. Iron and bitters are quite contra-indicated; and opium only succeeds when gastralgia is associated with the inflammatory action. Gaseous waters, ice, alkalis, and especially phosphate of lime prepared by the calcination of bone, alkaline and gelatinous baths, cold ablation of the entire surface, (in some cases very hot ablations,) cold baths, and, in some cases, very hot sitting baths, stimulant frictions, with shampooing of the entire surface, derivatives or revulsives applied to the epigastrium—are the means which have seemed to exert most influence on the progress of the disease. 16. It must never be forgotten, that this ulcer is very liable to relapse, such relapse sometimes going on to hemorrhage or perforation. Such relapse may be certainly prevented by a good alimentary hygene, and avoiding medicinal stimuli.—[Comptes Rendus. Med. Times and Gazette.

Treatment of Typhoid Fever with Tar Water. By Dr. Chapelle, of Angouleme. (Translated for the Charleston Medical Journal and Review.)

Having observed the favorable effect of tar in a certain case of Typhoid fever, Dr. C. was induced to pay particular attention to this remedy, in a series of cases occurring during the typhoid epidemic of 1854, 1855. His conclusion is, that liquid tar, if not an absolute specific, is yet incontestably the most efficacious agent yet discovered for the treatment of the above mentioned disease. The tar should be administered internally, as a drink, and in the form of an injection.
The drink is prepared in the following manner: About $\frac{3}{4}$ ij. of liquid tar are put into a vessel, containing nearly a quart of hot water; after it has stood a few hours, the patient commences to drink it, filling up with ordinary water after each draught, so that the same dose of tar will last during the whole treatment. The injection is prepared by rubbing up the yellow of one or two eggs with a table-spoonful of liquid tar, and diluting with a little more than a pint of warm water; this serves for two injections.

The patient should drink as much of the draught as he can; as to the injection, that should be insisted on in proportion as the drink disgusts, for the intestines should be always kept supplied with a certain quantity. Sometimes six, eight, and even ten enemata should be administered in twenty-four hours. Should the patient be taken with diarrhoea, these injections check it promptly.

This treatment, if continued for two or three days, generally triumphs over the typhoid state. Typhoid fever of ordinary intensity, called usually mucous fever, needs double that time; but typhoid fever, properly so-called, of whatever form, is vanquished in its essential phenomena in eight to ten days. Each day the skin loses its dryness and heat, the tongue becomes clearer, the abdomen presents less tension and susceptibility, the sleep is calmer, the fecal matter acquires a more normal odor, and the digestive functions recover strength. When there exists only a simple typhoid state, the tar draught alone is commonly sufficient; but when the general perturbation augments, the febrile re-action increases, and the functional disorder is excessive, a much stronger dose of the tar is required, and the injections are then indispensable. In all cases where the breast or the head has been affected with violent perturbation, the disappearance of the ordinary typhoid phenomena does not immediately produce a cessation of these complications. These functional disorders either disappear gradually of themselves, or need the application of treatment appropriate to the morbid state.—[Rev. Med. Chirurg.

Fungus Haematodes, cured by Chloride of Zinc. By F. J. Cogley, Madison, Indiana.

In February, 1856, I excised from Mr. P. M., aged twenty-four, an enormously enlarged testicle, which on examination proved to be, unequivocally, medullary sarcoma. There is not a larger, or purer specimen, of medullary cancer in the British or French museums. About the middle of January, the wound being nearly healed, he returned to his home; but soon found it would not entirely close. He returned to me the first of March, with a bleeding fungus, the size of a hen's egg, protruding from the small portion of the wound, which had refused to heal. It was, beyond doubt, fungus haematodes.

I transfixed the base of the tumor, with two sharp pointed
probes, crossing each other at right angles, and involving considerable sound tissue: then applied a strong ligature below the probes, in order to prevent hemorrhage; afterwards I protected the scrotum from the zinc, by muslin strips passed round under the probes. After removing a portion of the fungus with the scissors, I applied the chloride of zinc in its purity, and continued its application until the fungus was entirely destroyed. In a few days, a very deep eschar came away, and the ulcer healed very rapidly, so that in twenty days, he again went home with the wound entirely closed. It is my decided opinion, that if I had excised this fungus, it would have been rapidly reproduced; nor do I believe that any "combination" of "chlorides"* could have enhanced the value of the application.

This patient has had a slight cough, with bloody expectoration for more than a year; his general appearance cachectic; and it seems more than probable he has fungi in his lungs, but is enjoying tolerable health.—[Lancet.

Chloric Ether in Diarrhoea.

Diarrhoea of a painful character, and attended by spasmodic action, has been relieved in England by the use of chloric ether, after having resisted opium and a multitude of other remedies. "The effect of ether in every case was marvelous. The spasms and pains were relieved as by a charm; the diarrhoea ceased; warmth returned to the extremities; the pulse, before perhaps flagging, increased in force and volume. The relapses were unfrequent, and were generally checked at once by a single dose." The same treatment was found efficacious in an epidemic diarrhoea, which was supposed to be premonitory of cholera. "Hundreds of cases in which alarming cramps existed, were cured like magic.—[New Hampshire Jour.

Antidote to Strychnia.

M. Guiboust lately stated to the Academy of Medicine that, having observed a dog in violent convulsions, in consequence of eating one of the compound balls containing strychnia, he forcibly made it swallow powdered gall-nuts, when the convulsions ceased immediately. Ipecacuana was then given to the animal, but the latter could not vomit. The next day milk was given to it and manna, after which the dog recovered. M. Caventon said that the infusion of gall was a very effectual opponent to vomiting, and that he had observed it destroy the power of Tartar Emetic. M. Orrila had already advised the administration of this infusion in cases of poisoning by opium and salts of morphia.—[Bulletin Universe. Boston Med. and Surg. Journal.

* Lancet, June, 1855.
EDITORIAL AND MISCELLANEOUS.


The work before us has so generally received the approbation of the profession, that a new edition cannot be otherwise than welcome. Although treating of but one organ, a glance at the table of contents will show that the volume is not too large for the subject. After devoting some space to anatomical considerations, the author studies the congenital malformations, atrophy, and injuries of the testicle. He then devotes special chapters to hydrocele, hematocoele, orchitis, tubercular disease of the testicle, carcinoma of the testicle, cystic disease of the testicle; to fibrous, cartilaginous, calcaneous and other tumors of the testicles; foetal remains and entozoa in the testicle and scrotum; spermatocele. The nervous affections are comprehended under the heads of irritable testicle and neuralgia; the sympathetic and functional under those of impotency and spermatorrhœa. The affections peculiar to the cord are varicocele, tumors, and spasms of the cremaster. The work closes with a full review of the diseases of the scrotum; as injuries, prurigo, varicose veins, pneumatocele, œdema, diffuse inflammation, mortification, elephantiasis, tumors, cancers, &c.

This volume is essentially practical, and treats of diseases of every day occurrence. It should be in the hands of all physicians.


The publication of a new monograph on Digestion is rendered necessary by the developments of experimental and chemical physiology; and although we have not yet been able to study the work before us, we have seen enough to induce a desire to know more of it, and to advise others to read it attentively. We have no doubt that it is a very valuable addition to our stock of practical monographs.


The very interesting subject of this work, as well as the ereditable manner in which it is treated, will command for it a ready acceptance by the profession. It does great credit to American medical authorship, and
proves that good books may be made in this, as well as in any other country.


We are happy to find that the favorable opinion of this work, advanced by us upon the reception of the first edition, is fully sustained by the profession, as is evinced by the demand in so short a time for a third edition. The present volume contains much new matter of interest. We cheerfully reiterate our former commendation.


These tables will prove exceedingly valuable helps to the student, and may be consulted with advantage by practitioners who desire to refresh the memory, in the midst of professional engagements.


This little volume treats of "Headaches in childhood and youth," of "Headaches in adult life," including such as are dependant upon the circulatory, the digestive and the nervous systems; and finally, of headaches in old age. We would advise its careful perusal, for these affections are very little understood in general, and often wofully misunderstood.


This interesting contribution to the history of medicine, was made in the form of an anniversary discourse, delivered before the New-York Academy of Medicine, in November last. It is indicative of great erudition on the part of the learned author, and will be read with pleasure by the profession.

Pronouncing Medical Lexicon, containing the correct pronunciation and definition of most of the terms used by speakers and writers on Medicine and the Collateral Sciences; with addenda. By C. H. Cleaveland, M. D., &c. 2d edition. Cincinnati: Longby Brothers. 1856. 18mo. pp. 300.

The use of a phonetic alphabet in illustrating the pronunciation of words, renders it necessary to learn that in order to understand this. We have not, therefore, yet had leisure to enable ourselves to judge correctly of the
merits of the little lexicon. This is the work, sometime ago, stigmatized as a plagiarism. We know nothing about the correctness of the charge, but it would seem to be difficult to make a dictionary, especially a condensed one, that would be materially different from its predecessors.

Two pieces of Needle removed from the Thigh after a sojourn of forty-five years—On the 9th of February, 1856, Mr. Ebenezer Byram, aged 53 years, presented himself at my office for the purpose of having his thigh examined, which, he said, was swollen, as his physician told him, from the "fever settling in it," he having had a spell of typhoid fever some twelve months previous. On exposing the limb, I found the thigh, on its external side, much swollen, red and inflamed; the surrounding parts being quite hard, and about the centre of the thigh, on its external side, was a small soft space about the size of a silver dollar, which fluctuated on percussion, showing that a quantity of matter was contained within. I plunged a large abscess lancet into this soft space, and discharged about a gill of thin yellowish fluid. I then introduced the probe, and after searching for some time, felt the probe grate against some foreign body. I immediately withdrew the probe and introduced a small pair of forceps, and laid hold of the substance, and withdrew two bits of what appeared to have been a large sewing needle; each piece was near three-fourths of an inch in length. After introducing a tent, and dressing the wound, I made enquiry concerning his previous history. He informed me, that when about eight years of age, he had what his parents called white swelling; the symptoms then were great swelling, pain, and redness of the thigh, which continued to trouble and pain him for twelve months; but at no time did it ever discharge matter, but gradually subsided, and at the end of twelve months he suffered little or no inconvenience from it, except on sitting in "a certain manner" in a chair, or in riding on horse-back, if he pressed the parts "a certain way," they would feel as though something were sticking in him; these continued to be his symptoms up to September, 1855, when the pain, redness, and swelling increased and gave him so much trouble, as to induce him to seek advice; and the above is the history and the result.


Hydrophobia.—Dr. T. W. Blatchford, of Troy, in his paper on hydrophobia, read before the American Medical Association at its late meeting in Detroit, reports the following curious facts, as having taken place in Prussia:

In 1810 there were in that kingdom 104 deaths from hydrophobia; in 1811, 117; in '12, 101; in '13, 85; in '14, 127; in '15, 79; in '16, 201; in '17, 228; in '18, 260; in '19, 356, making a total of 1,658 deaths in 10 years in Prussia alone. It is mentioned also as a curious fact, that in Cyprus and Egypt hydrophobia never has been known to occur. It is believed also that the disease is incident to no particular month in the year, as statistics show on the whole as many deaths at one month in the year as any other—there being no real difference between summer and winter. Dr. B. believed that the constitutional irascibility of the dog was the true etiology of canine madness, and that excision is the only means now known which affords any reasonable hope of successful prevention. The report
pronounced as an utter fallacy the general idea that the dog-star has anything to do with the origin of virus in the dog, or that summer has any special preponderance over winter in the existence of cases of hydrophobia. The facts submitted, and which had been collected by the Committee, show the following facts:

Out of 72 cases, 54 were bitten by dogs, 6 by cats, 1 by a raccoon, and 1 by a cow. Out of 62 cases, 4 died the first day, 9 the second, 6 the third, 18 the fourth, 4 the fifth, 2 on each the sixth, seventh, and tenth days, and one on the twenty-first. That 22 cases occurred in March, April and May; 17 the next quarter; 18 the next; and 22 the last. The average of the time of sickness was 66 days; but this lengthy average was enhanced by two strongly marked cases, lasting 365 and 360 days respectively. The usual average is 41 days.—[N. Y. Med. Times.

A New Instrument for Indicating the Movement of the Heart.—Dr. Scott, Alison has exhibited an instrument to the Royal Society which he calls a sphygmoscope, and employs it to indicate the movements of the heart and blood vessels. The construction is simple: a small glass tube, about a foot in length, open at the upper end, and with a graduated ivory scale affixed, terminates below in a hemispherical or trumpet-mouth, bent to a right angle with a tube. This mouth is covered with a water-proof membrane, and, being filled with colored water, is to be pressed against the ribs where the movement of the heart is most sensible. At once the water starts up the tube, in which it is seen to rise and fall with every beat; and thus all the movements of the vital organ, whether regular or irregular, may be distinctly viewed and measured by means of the scale. A smaller instrument of the same kind will show the beating of the pulse or of any other blood vessel, however small; and the beats may be compared with those of the heart. They are perceptible even at the end of an India rubber tube two feet in length. Already some new physiological conclusions have been arrived at with regard to the circulation of the blood, and a further insight into vital action is hoped for from the general use of the sphygmoscope among medical practitioners.—[Boston Med. and Surg. Journal.

Liquid Caoutchouc.—This is said to be of the color and consistency of milk, and is preserved in the fluid state by the addition of free ammonia. As an external application, it has many advantages over both colloidion, and guta percha dissolved in chloroform. It is not stimulating and painful, as are both the others in certain cases; it does not contract, like colloidion; and on account of its elasticity, it allows entire freedom of motion. Water does not act upon or remove it; and it adheres closely to the skin. In the treatment of burns, erysipelas, and many other surgical diseases which require exclusion of the atmosphere, it answers the purpose so perfectly, as to render any other preparation scarcely desirable.—[Ibid.

After Pains and Hemorrhage.—These difficulties, which so often follow delivery, and are so little under the control of remedies, have been forestalled and prevented by a French accoucheur, by the exhibition of ergot during delivery, and injecting cold water into the umbilical vein immediately after delivery. The object is to produce such tonic contractions of the uterus, as will prevent the accumulation of blood or other fluids in the
uteroine cavity, the expulsion of which causes pain. Even cases in which ergot had not been used, it was found that the injection of five or six ounces of cold water into the umbilical vein, caused an immediate expulsion of the placenta; and the usual suffering of the woman from after-pains was avoided. The cord is cut to the length of some twelve or fifteen inches; and then a small nozzle of a syringe holding five or six ounces, or a small canula, can be inserted into the vein, and the injection made without difficulty—[16.

Consumption of Quinine.—The Philadelphia Medical and Surgical Journal says that 300,000 ounces of Quinine are annually consumed in the United States, meaning, it is presumed, imported, as there are two very large manufacturing establishments in this country which prepare it on an extensive scale, and which are not included in the computation of the Secretary of the Treasury, from which the above estimate is derived. It is worth, at the present time, from $3 to $4 the ounce.—[Peninsular Jour. of Medicine.

New Hemostatic.—Dr. Butler, of Ohio, recommends a scruple of tannic acid to be dissolved in an ounce of elixir of vitriol, and 15 drops to be given as a dose—in menorrhagia, etc.—[American Jour. of Pharmacy.

On a Composition for Attaching Labels. By Frederick Stearns, Pharmacist—Having noticed in the March number, the present year, of the American Journal of Pharmacy, an article upon "Unalterable Labels for the Cellar," it occurred to me that the method I have employed for some years, in giving adhesiveness to dispensing and other labels, might be of some service to the readers of the Journal. It is as follows:

Take of white glue (Cooper's best) three ounces, (avoir.); refined sugar one and a half ounces; water ten fluid ounces, or a sufficient quantity. Dissolve by the aid of a water-bath, and use while warm, applying it by means of a suitable brush to the reverse side of the labels while uncut or in sheets. After being dried and moderately pressed they are ready for cutting. A little experience will show the propriety of increasing or lessening the amount of water used; for instance, if the paper is thin and well sized, more may be added; on the contrary, if the paper be thick and without sizing, less is required; in all cases it should be quickly and evenly spread upon the paper.

It is not applicable to the purpose of a common paste, as it can only be used while warm.

I have found the use of it to possess these advantages: Labels prepared with it adhere more finely than when any other adhesive substance is used; it does not penetrate, and thus disfigure the label, and when applied to glass they never become loose, as is often the case when acacia and tragacanth are used, when moistened with saliva. No disagreeable impression is left in the mouth, as with dextrine, and it would well supply the place of that material upon Post Office stamps, gum tickets, etc.—[American Jour. of Pharmacy.

ERRATA.—In Dr. Kellock's article, June No., on page 333, seventh line from bottom, for "anus," read arms. On page 337, fourteenth line from bottom, for "anus," read arms.