Essay upon the Presence of Sugar in the Urine, and its Connection with Respiration. By Alvaro Reynoso. (Translated for this Journal, by Wm. J. Holt, M. D., of Georgia, now in Paris.)

The memoir that I have the honor to present to the Academy, is no more than the development of three notes that I have already submitted to their judgment. I have, since the publication of these notes, repeated a number of times my experiments, and found some new facts which come to the support of the theory that I had published.

Life is an assemblage of successive formations and decompositions: during its existence our organs are continually being destroyed and reformed, although at certain epochs each one of its actions can be augmented or diminished separately.* We have a proof of the decomposition that our organs undergo, in the continual need of nourishment; a want which does not depend entirely upon the diminution of the liquids, for the solids contribute their share. In fact, these parts, especially the muscles, diminish, and their normal composition is ultimately changed, when nourishment is wanting. An addition of new materials supposes a corresponding consumption, and as the

body remains the same when nutrition undergoes no alteration, this ought to have as antagonist a resorption, the proportional quantity of which is too great in atrophy and too feeble in hypertrophy. And this renewing of materials ought to accompany all the acts of life; for the increase of activity in a function induces, as a consequence, either the want of greater nourishment and repose, or wasting and reduction. It is what we observe in fevers, after violent exercises, continued watchings, constant labors of the study, and commotions of the passions. After that, our body is subjected to a continual change of its substances, so that at the end of a certain number of years there remains not an atom of the matter of which it was formed.

The animal then has need of matters to repair the losses that the vital energy causes to his organs—matters which ought to present the same composition as the organs themselves.

Besides, we know that life is always accompanied with a certain disengagement of heat; and whether we consider this heat (after the manner of the ancients) as the source of life, or only as the result, it is proved that the disengagement of heat is in proportion to the energy and activity of life. Again: we see that respiration exercises an influence upon the production of heat, and that these two functions are in direct connexion in the animal series, in the different circumstances and periods of life. We know, for example, that respiration is the condition of muscular force, and that the development of the respiratory system in the animal series is in direct connexion with the facility and velocity of voluntary motion.

The heat disengaged is in proportion to the respiration.

It is equally incontestable that the quantity of heat developed is generally in proportion to the oxygen inspired and the carbonic acid expired. Thus, at a high degree of organization, when life enjoys great activity, and when the development of heat is considerable the consumption of oxygen is greater.

Substances proper to combine easily with oxygen and to develop the most heat in this combustion, will then be necessary for an animal. For aliment, then, two kinds of substances are necessary: one kind destined to identify themselves with the parts of the organism, to repair the losses which accompany the
exercise of life, and to develope our organs; others destined to furnish heat by their combination with the oxygen in the act of respiration. The decompositions which accompany the exercise of life, and the heat which is the cause or effect of it, being both in direct connexion with vital energy, it is clear that the quantity of aliment ought to be proportional to vital activity. Liebig calls the reparatory aliments, plastic aliments; and under this denomination he ranks albumine, caseine and fibrin, vegetable and animal. These substances are, in fact, the only ones furnished by these two kingdoms which can be capable of giving origin, in the process of nutrition, to the essential parts of the blood, which nourishes our organs. We understand, also, that we should rank among the plastic aliments, the different mineral salts, which contribute to the formation of the solids and to the composition of the liquids, of the economy.

Liebig designates under the name of *agents of respiration*, the aliments destined to combine themselves with the oxygen, to develope heat, and ranks in that class all of the non-azotized matters—*sugar, starch, fat*. Among these several agents of respiration, the most suitable is fat, inasmuch as it burns more easily and produces more heat.

As long as harmony exists between the proportions of these substances, in the mixed nourishment and vital energy, each one accomplishes its end; for, in the circulation, the plastic aliments are preserved from combustion by the presence of non-azotized substances; but as soon as they are wanting, there is a certain proportion of the plastic aliments destroyed, although they burn with difficulty and produce little heat. It is probable that these plastic aliments are not burned, until after having been changed into other substances, into fat for example.*

When, on the contrary, the plastic aliments are wanting, the

* Even during a mixed regimen we see appearing in the urine principles such as urea, uric acid, &c., azotized substances which cannot proceed but from burned azotised matters. Their origin is very easy to explain. We have already admitted that life is an uninterrupted series of decompositions and formations. The azotised substances of our organs, which have been modified during the exercise of the latter, and which are no longer proper to contribute to their structure, are destroyed by the economy, burned by the oxygen, and rejected under the form of urea, uric acid. The sulphur and phosphorus contained in these substances are transformed into sulphuric and phosphoric acids, and rejected under the form of sulphates and phosphates.
animal wastes away and dies; for animals have not the faculty of transforming sugar, starch, and fat, into plastic aliments, a property which vegetables alone possess.

If the animal is subjected to a very abundant, mixed regimen, he acquires a plumpness, that is to say, the organs grow, on account of the accumulated plastic substances and the deposited respiratory agents, as for example, fat.

Let us examine the part that the aliments take in the formation of fat.

We have already said, that fat is a substance destined to contribute to the production of heat; its accumulation in the organism can not proceed, but from want of respiration, an excess of nourishment, or both of them together. Fat can either arise from the aliments, or form itself in the economy; it can also be deposited in the organism, in these two manners together. Almost always the economy produces the greater part, especially when the nourishment is rich in plastic substances. When an animal is submitted to a regimen, poor in plastic substances, then the accumulation of fat is sensibly equal to the ingesta of the animal. When fat predominates in the aliments and the plastic matters do not suffice for the formation of cells, the muscles are resorbed and fat deposited; but a disease follows and the animal dies. If he is submitted to a mixed regimen, rich in plastic principles, then we find that the quantity of fat accumulated is superior to that which was contained in the nourishment. Moreover, it is a curious fact that there ought previously to exist in the food a certain quantity of fat in order to determine the rapid formation of a greater quantity of this substance in the economy. Thus, for example, rice, which we may consider a grain without fatty principle, does notfatten, while Indian corn, which contains a small quantity of it, is very proper for that purpose.

What are the aliments which produce fat? In what organ is it formed? The first question is resolved—the second not as yet.*

Fat can proceed either from azotised or non-azotised aliments. The azotised aliments, according to M. Wurtz, by putrefaction, divide into ammonia and fatty acids, (butyric and valerianic,) so that we understand that fat is drawn off or derived from these azotised matters. If we recollect that sugar produces butyric acid, when in the presence of caseum in a state of putrefaction, (Pelouze and Gélis,) and besides that, this same sugar, in presence of particular ferments, which are found in potatoes, beets, &c., produces amylic alcohol, from whence arises valerianic acid, an acid found by M. Chevreul in the fat of the cetaceae, we will easily conceive of the formation of fat, by the means of sugar. If it is so, says M. Boussingault, animals would share with vegetables, the faculty of creating fatty bodies, and that, probably by analagous means. We see, in fact, that starch and the saccharine substance gradually disappears in plants as the fatty substance accumulates in the seeds.

According to Liebig, we would be able to deduce the formation of fat from starch, by the following division:

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\begin{align*}
\text{Starch} & \quad \text{Fat} \\
\left( \text{C}^{12} \text{H}_{10} \text{O}_{10} \right) & \quad \left( \text{C}^{11} \text{H}_{10} \text{O} - \text{C} \text{O}_2 - \text{O}_7 \right)
\end{align*}
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As we know that starch is never absorbed but in the state of grape sugar, it would be necessary, in order to explain this formation, to establish the following formula: \( -\text{C}^{12} \text{H}_{14} \text{O}_{14} = 4 \text{H}_2 \text{O} - \text{C}^{11} \text{H}_{10} \text{O} - \text{O}_7 - \text{C} \text{O}_2 \).

The oxygen set free in this reaction, would combine with other substances, and would be rejected under the same form, as the oxygen introduced into the system by respiration. It would necessarily follow that there would be in the organism, a source for oxygen, independent of that of the air, so that sometimes the quantity of carbonic acid eliminated, would be greater, proportionally, than the inspired oxygen.

The air, in which animals live who are subjected to the process of fattening, has not as yet been analysed, so that we cannot affirm, whether this hypothesis be true or false. However, I will recall a fact which I think comes to the support of it. MM. Regnault and Reiset have often proved, that in chickens submitted to a regimen of grain, the quantity of carbonic acid expired, is superior to the oxygen furnished by respiration.*

If we admit that sugar and starch can be transformed into fat by the above equation, we will be able to understand its formation by the proteic matters. According to M. Hunt,* protein, which is the normal kind of albuminous matters, would be derived from the cellular tissue, and would be an amide of this latter substance. He supposes that the small quantities of sulphur and phosphorus met with, take the place of the oxygen and normal azote. M. Hunt proposes as the formula of protein and the explanation of its formation, the following equation:†

Protein—
$$2C^{12}H^{10}O^{10}-3NH^3-12HO=C^{24}H^{17}N^3O^8$$

Gelatine—
$$2C^{12}H^{10}O^{10}-4NH^3-12HO=C^{24}H^{20}N^4O^8$$

The reaction observed by M. Gehrardt, gives probability to the formula of gelatine; when isinglass is boiled with sulphuric acid diluted with water, a large quantity of sulphate of ammonia and sugar is formed, which ferments with the leaven of beer and produces carbonic acid and alcohol.

Thus, then, fat would be derived from protein by a reaction analogous to that which explains its production by means of starch; only the oxygen set free, would be absorbed by the residues of carbon and azote, to form the uric compounds found rejected by the urine.

To complete the enunciation of theories on the use of sugar in the economy, we must recollect that, according to Tiedemann and Gmelin, it contributes to the formation of bile. Besides, it would form the lactic acid found in the economy. According to Berzelius, lactic acid would also be the product of the spontaneous decomposition of the animal matters in the interior of the body.

Whatever may be the modifications that sugar undergoes, before it is destroyed in the economy, it is shown that it always finishes by being completely burned, producing carbonic acid

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* Comptes Rendus de Travaux de Chimie, par MM. Laurent et Gehrardt, 1850, p. 317.
† The formula for protein, of M. Hunt, makes—Carbon, 53·93; Hydrogen, 6·36; Azote, 15·73; Oxygen, 24·34
MM. Dumas and Cahours have found for albumine—Carbon, 53·59; Hydrogen, 7·27; Azote, 15·72; Oxygen, 23·52.
and water. Besides, it is certain that a large part of the sugar disappears through the lungs in the act of respiration; for the blood which leaves the liver and continues towards the lungs, contains sugar, and that which leaves the lungs is completely exempt from it, or sensibly so. I say sensibly, for it is probable that the arterial blood contains sugar in such small quantities that analysis cannot disclose it, and which is destined to be burned in the rest of the economy; for combustion not only takes place in the lungs, but in all parts of the body.

If respiration is enfeebled—if the economy cannot produce the quantity of heat indispensable for its normal support, then a part of the sugar which exists in the economy, escapes through the secretions, not being able to be burned in order to produce the heat, nor to undergo the other transformations to which it is subjected in the normal exercise of our functions. For the formation of fat, for example, although indicating an excess of combustible food, a good alimentation and assimilation of azotised principles is imperiously required. Now, a good alimentation and assimilation of the azotised principles, can not take place except when the nutrition of our organs demand them, and when the heat disengaged in respiration second them—conditions which require the complete exercise of our functions.

Thus, whatever may be the form under which the sugar disappears in respiration, it is certain that it is destroyed, either in being burned directly; or converted into other substances. Besides, whether this transformation or combustion of sugar is connected with respiration itself, (and primitively as I think) or whether the respiration does not contribute to it except after the vigour of life, by its connexion with other functions, and by exercising a superior and by its importance a much more marked influence, in either event, the destruction of sugar is in proportion to the degree of respiration.

To resume, my position is as follows:

_It being shown that the sugar is destroyed in respiration, it remains to prove that this destruction is in direct proportion to the respiration._

The sugar which is not destroyed in the respiratory act or metamorphosed in the economy, passes into the urine and it is by its presence in this liquid that we judge of its non-destruction.
or nonmodification; for in a physiological state, sugar is not found in the urine.

Let us glance at the theories, that we can produce upon this phenomena.

Firstly: The respiration remaining normal; the sugar appearing in the urine, would be there, because the liver secretes a greater quantity of it than the economy can consume.

Secondly: My theory is as follows: the quantity of sugar consumed by the animal in a healthy state, will be diminished when the function of respiration is no longer exercised, as in the normal state.

Since the experiments which have led to admit a glucogenic power in the liver, it has been imagined, that whenever sugar appears in the urine, its presence is due to the fact that the liver produces a greater quantity of it than the animal destroys. Without wishing to note all that this proposition presents as positive, we will say, without denying either the importance or exactness of the work,* which has served as the basis of this theory, that this function of the liver appears to us insufficient to explain this phenomenon.

If we admit that sugar is destroyed during respiration, we must necessarily admit, either that the sugar increases beyond the force which destroys it, or that the respiration diminishes and can no longer destroy the quantity which disappears in the normal condition. The two circumstances might take place separately or conjointly. I only believe the second, for it is the only one that we can prove. It would be necessary to prove numerically that the quantity of sugar produced by the liver can surpass greatly the enormous quantities that we destroy every day. It appears at first sight incredible, that the quantity of sugar produced in the liver when an animal respires ether, can be greater than that which he can ingest at one meal, either of sugar itself, or of aliments susceptible of being transformed into sugar, a quantity which nevertheless is entirely destroyed.

Without enumerating the numerous cases in which sugar

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*I think he probably alludes to a recent memoir, by Mr. Claude Bernard, upon the nouvelle fonction du foie considéré comme organe producteur de matière sucrée chez l'homme et les animaux.—HOLT.
appears in the urine when the first proposition is incapable of explaining its presence, there are circumstances of the passage of sugar which contradict it entirely; while on the other hand all the facts are well explained by the second, as soon as we admit of a modification in respiration. We will have occasion to return to this subject.

The first note that I had the honour to present to the Academy was thus conceived:

"The functions of the medulla oblongata have been studied by different physiologists, all of whom agree in considering it as the focus and regulating organ of the respiratory movements. Besides M. Flourens has found, that there is a part of the bulb, very circumscribed, which is the veritable seat of respiration. This point is found in rabbits, immediately above the origin of the eighth pair of nerves and its inferior limit almost underneath that origin. M. Bernard, by pricking rabbits near the origin of the pneumo-gastric, renders them diabetic; and he explains this phenomenon by saying, that under the influence of the excitement produced, the liver secretes so much sugar that not being able to be consumed by respiration, it passes into the urine. I thought to be able to explain the phenomena by admitting that, under the influence of the lesion caused by the puncture, paralysis of the respiration ensued, if not complete at least partial, and that consequently the normal sugar not being able to be burned, passed into the urine. In order to prove it, it was necessary to find a means to obstruct respiration by causing asphyxia; the experiment has proved to us, that by means of anesthætic agents, we succeeded in producing sugar in the urine.

"Supposing our explanation to be right, we ought to find as much more sugar, as the animal subjected to the etherisation had a more active respiration and as his food contained more sugar; for there passed off more of unburned sugar. We have observed, in fact, that in herbivorous animals and those subjected to a mixed regimen, more sugar passes than in carnivorous animals nourished entirely on animal food: in the case of two men subjected to etherisation, the more vigorous gives more sugar.

"In short, it was curious to see whether in other circum-
stances of asphyxia, we would find the animals becoming diabetic. Rabbits strangulated and drowned gave us sugar in the urine; but it must be added that we did not obtain it in every case, probably because the means of asphyxia, induced numerous disturbing causes in the economy.

"Thus, a living animal which did not breathe, would normally present sugar in his urine. M. Bernard has, in fact, proved that in the foetus sugar is always found in the urine.

"We think that it should be looked for also in persons subjected to a hyposthenic treatment.

"A word upon the manner of making the experiments. We may operate upon animals; or better, upon a vigorous and healthy man. We first make them urinate; afterwards etherise them. We collect the urine, treat it with the subacetate of lead, filter and precipitate the excess of the salt of the lead by the carbonate of soda. It is in the filtered and concentrated liquor, that the presence of sugar must be looked for, with an alkaline solution of the tartrate of potassa and copper, or in placing it in contact with the leaven of beer, which transforms the sugar into alcohol and carbonic acid.

"We think that these experiments will render clear the disease of the diabetics; for they establish the relation which exists between the respiration, nervous influence and the sugar of urine."

Effect of Inspirations of Ether.

As I said in my first note, whenever an animal breathes ether, sugar appears in the urine. I explain that fact, by the trouble that the respiration undergoes, which being diminished, can not destroy all the sugar that the blood furnishes.

It has been attempted to explain this phenomenon in a different manner. It has been pretended that the vapors of ether, upon arriving at the lungs, produced an irritation, which would be transmitted by the pneumo-gastric to the encephalon, and thence reflected upon the great sympathetic, which, irritated in its turn, would increase the production of sugar in the liver and to such a point, that the economy being no longer able to destroy all, would reject the excess by the urine. Nevertheless, I have made an experiment which does not accord with this ingenious explanation.
I take a rabbit and make it urinate. Afterwards, I cut the two pneumogastrics at the neck, taking away at least two centimetres (about an inch) of the nerve of both sides, and then make the animal respire ether. I commence at first by placing it in a complete state of anaesthesia. When he is entirely aroused, I make it again respire ether during ten minutes, but without placing it in a complete state of anaesthesia. Then I examine its urine, which is very clear and contains sugar in as great quantity as if the pneumogastrics had not been destroyed.

Thus, whenever we make a rabbit breathe ether, whether he possesses his pneumogastrics, or whether he has been deprived of them, there is always a passage of sugar in the urine.

When rabbits breathe chloroform, Dutch liquor, hydriodic, hydrobromic, chloramylic, nitric,* and acetic ether, aldehyde, benzine and acetone, we obtain also the passage of sugar in the urine. The same result is obtained by slowly putting them in a state of asphyxia by means of sulphuretted hydrogen and carbonic acid or with the vapors of hydrocyanic acid.

I think, then, I may conclude that:

All substances which induce anaesthesia, gases or irrespirable vapors, cause sugar to pass into the urine; and that this passage is independent of the integrity of the pneumogastric nerves.

I will cite a fact discovered by M. Bernard, after the publication of my work, and which enters naturally into this same class.

According to him, when an animal respires chlorine, a passage of sugar into the urine takes place. Here, the chlorine acts, firstly, because it is respired in the place of air and thence diminishes the quantity of oxygen inspired, afterwards, because it disorganises the pulmonary vesicles which become unfit for their exercise.

Almost all of my experiments upon the inspiration of anaesthetic agents have been made upon rabbits. The best conditions to obtain a satisfactory result are the following. The rabbits that are bought in market are generally fatigued and badly nourished; it is necessary to let them rest twenty-four hours and feed them abundantly with carrots. At the end of that time, we commence the experiment and obtain the sugar

* The nitric ether was rather a mixture of nitric and nitrous ether.
in a quantity proportional to the length of time that they breathe the anaesthetic agents. If one has the patience to make them breathe the ether an hour and a half, a very good result will be obtained. We commence by putting them in a complete state of anaesthesia and allowing them to recover, recommencing again the anaesthesia, and so on for five or six times successively. With simply one etherisation, a good result may be obtained, but it is better to etherise them five or six times successively.

**VITAL POINT (noeud vital?) of M. Flourens.**

I promised in my first note that the *vital point* ought to play an important part in the passage of sugar in the urine, being the motor point of the respiratory mechanism; since then, thanks to the kindness of M. Flourens, who has opened the doors of his laboratory and lavished his wise counsels upon me, I have been able to assure myself that I was not mistaken in my premises.

At the posterior extremity of the fourth ventricle, between the two posterior pyramids of the medulla, there exists a small V in the gray substance, inscribed at or in the bifurcation of these two pyramids. This V, is the continuation of the gray substance of the medulla, and is also called the *calamus scriptorius*. It is in this V of the gray substance, and between the two sides of the angle formed by the V, that the vital point of M. Flourens is found.* According to him, "the superior limit of the vital point is formed by the foramen cœcum, the inferior at the point of junction of the posterior pyramids; between these two limits is found the vital point, the distance between them being scarcely a line."

M. Flourens was not contented to determine the position and limits of the vital point; he went further, and thanks to a vigorous and rational analysis, in this point hardly as large as a line, he has distinguished the demarcations. He exposes the result of his researches as follows;

"I often make the experiment, proceeding by transverse sections.

"If the section passes in front of the foramen cœcum, the res-

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piratory movements of the thorax continue, while those of the face cease.

"If the section passes behind the point of junction of the pyramids, the respiratory movements of the face (the movement of the nostrils and gaping) continue, while those of the thorax cease.

"If the section passes upon the point of the V of the gray substance inscribed in the V of the pyramids or point of the pen, (calamus scriptorius ?) the respiratory movements of the face and thorax cease immediately, and at the same time.

"I often make the experiment in another manner: I use a small punch, the opening of which has a diameter of scarcely half a line; I plunge this punch into the medulla oblongata, being careful that the opening of the instrument corresponds to and embraces the V of the gray substance. I thus isolate suddenly the vital point from the rest of the medulla oblongata pyramids, restiform bodies, etc., and immediately the respiratory movements of the trunk and face are abolished.

"Thus by experiment, the limits of this point are marked—below, by the persistence of the inspiratory movements of the head; and above, by the persistence of those of the trunk."

When the vital point of M. Flourens is pricked, there is always a passage of sugar in the urine; I have repeated frequently the experiment in the presence of M. Flourens, and I have constantly obtained the same result.

I took a rabbit and made it urinate. The urine was troubled as usual and did not contain sugar; I pricked the vital point, and as soon as it could urinate again, I obtained a clear urine and containing sugar in considerable quantity, and easily determined by every test.

It is an error to think that I meant to say, in my first note, that the point of the bulb that M. Bernard pricked, in order to cause a passage of sugar in the urine, was the vital point of M. Flourens. I by no means meant that, but I thought, and still think, that the point which M. Bernard pricks is under the dependence of the vital point, which has also been called by M. Flourens, central, motor point of the respiratory mechanism. It

*Flourens recherches experimentales sur les proprietes et les fonctions du systeme nerveux, p. 201.
is at the same time the centre of action and co-ordinator; and by coordinated movement, M. Flourens means: "all (or any?) movement which results from the concurrence, union or grouping, so to say, of several other movements, distinct and separated one from the other, and which if grouped in another manner would have given another result total."

The central point of coordination of the different acts of respiration, is the focus, so to say, from which emanate its coordinating actions, so that its action is multiple but unique. It presents unity in its multiplicity, simplicity in its complexity. I think, then, that although the point that M. Bernard pricks, be a half an inch above the vital point, the puncture made at this point intercepts some of the active and co-ordinating rays of the vital point, and that thus it is under its dependence, for it obstructs its action to reach the point where it ought to arrive in order to keep up the harmony in combining and rendering active the respiratory movements.

I forgot to remark that, when the vital point is pricked, not only the urine becomes limpid, but the quantity is considerably increased.

In my second note, presented to the Academy, the 10th of November, 1851, upon the connexion between the respiratory phenomena and the presence of sugar in the urine, I thus expressed myself:

"In a preceding note, we announced that there exists a connexion between the respiratory phenomena and the presence of sugar in the urine, in such a manner, that all substances which enfeeble the respiration in diminishing the haematosis produced in the lungs, are so many causes which might, in our opinion, determine the passage of sugar in the urine. We added that, according to this principle, sugar ought to be found in the urine of individuals subjected to hyposthenic treatments; to enumerate them in short, it will only be necessary to cite the generalization of M. E. Robin.

"According to him, the substances, which after death, keep up a slow combustion by means of the humid oxygen, are hyposthenics in different degrees during life. For example, the metallic salts, ethers, the salts of quinine and narcotics in general."
"Having examined the urine of persons subjected to treatments* of the bichloride, iodide and sulphuret of mercury, salts of antimony, opium and sulphate of quinine, we found sugar."

The third note presented on the 3d of December, 1851, is as follows:

"In my preceding notes, I endeavored to establish the connexion which exists between the presence of sugar in the urine and the respiration; in such a manner that all causes occasioning a hindrance in the accomplishment of this function, would produce a passage of sugar in the urine.

"We spoke of the hyposthenic treatment which obstructs a part of the blood from the action of the oxygen. I will add to the examples already given, that in the case of dogs subjected to a treatment of arsenic, lead, zinc, copper and the sulphate of iron, and also with patients treated with the carbonate of iron, I have always determined the presence of sugar in the urine.

"I now approach the second part of my researches. Whenever the respiration becomes troubled, either by a disease proper of the lungs, or by the effect of some other affection causing a disorder in its normal exercise, there will be sugar in the urine.

"I have determined its presence in the urine of tuberculous patients, and the quantity of it was proportionally considerable as the period of the disease was more advanced, and as the inflammatory symptoms were less intense.

"In pleurisy and chronic bronchitis there is sugar in the urine; the same in asthma.

"It is found also in hysteria and epilepsy after the attacks.

"In cholera there ought to be sugar in the urine, for, according to the experiments of M. Rayer, the air undergoes none or scarcely any change in the lungs. It would be desirable, that, the physicians engaged in the study of this disease, would look for the presence of sugar in the urine of the patients."

We also have sugar in the urine after drinking a strong infusion of coffee.

I had an occasion to examine the urine of two drunken men and found sugar in it. We know, that after the researches of

*I knew, after the publication of this note, that M. Chevallier, had found sugar in the urine during a mercurial treatment.
Prout, alcohol is a substance which diminishes in the highest degree the exhalation of carbonic acid and thus the respiration. Moreover, we know, that according to the experiments of MM. Bourchardat and Sandras, alcohol prevents the transformation of venous into arterial blood; the arterial preserves the colors proper to venous blood, and thus alcohol may determine all the accidents of asphyxia. These same observers have determined the presence of sugar in the venous blood of a drunken man. We comprehend then the causes which produce the passage of sugar in the urine of drunken persons, and I will add that it exists in a considerable quantity.

**The Habitual Presence of Sugar in the Urine of Old Persons.**

I made the following experiments together with M. Dechambre.

The modifications that respiration undergoes in old age, are so notable and important, that Réveillé Parise does not hesitate to consider them as the origin, starting point, and the organic cause of old age. The senile deterioration of the respiratory organs, impedes the accomplishment of hæmatosis in a proper manner, and also the production of a general calorification to the extent that a complete and normal exercise of our functions exacts.

The modifications which obstruct the respiration are: lateral depression of the thorax, anterior projection of the sternum, stiffness of the costo-vertebral articulations, induration or ossification of the cartilaginous appendices, rarified parenchyma of the lungs, thin or ruptured cellular walls, obliterated vessels. Except when, by one cause or another, the blood of old people does not contain sugar, or only in a small quantity, (which conditions were in my theory,) there can not be a more favorable condition to the production of glucosuria.* Having in view the verification of this conjecture, we made the following experiments:

**First Experiment.**—We chose at first, at the hospice of Salpetrière, a woman aged eighty-one, in the last stage of decrepitude. We assured ourselves that she was exempt from any

*Is the translation correct? — Holt.
cough or sense of suffocation, and exhibited no physical or symptomatic sign of pulmonary or cardiac affection, or any other disease capable of obstructing the respiration; so that if there was any insufficiency of combustion, it could only be attributed to the state of the lungs engendered by old age. The urine of this woman, collected in the morning, in quantity about two ounces, was at first treated by the acetate of lead, in order to separate the uric acid and other precipitable organic matters, and then placed in a filter. The filtered liquor was freed of the salt of lead which it might have retained by means of the carbonate of soda, and re-filtered. We poured in the saccharimetric liquor of M. Barreswil,* (cuprico-potassic,) and after a minute boiling obtained a very abundant reddish precipitate, (protoxide of copper.)

Second Experiment.—The same experiment was made upon the urine of five women, aged from sixty-eight to eighty-one years, in the surgical wards at Salpêtrière, one of them treated for an abscess of the arm, another for rheumatic pains, the third for a chronic affection of the skin, the two last for contusions; all of them otherwise in good health. The urine of these five women, also collected in the morning, gave a very characteristic precipitate.

Third Experiment.—In order to obtain more easily old people exempt from affections of the heart or lungs, we authorized the urine to be collected without the infirmary; we chose, on the same day, eight women who appeared to offer the requisite conditions, aged seventy, and already very decrepit; two gave only a slight yellowish shade, but little significative; six a veritable reddish precipitate.

Fourth Experiment.—Wishing to know whether the glucosuria was constant with these women or only momentary, at the end of a week, we took, a second time, the urine of seven of them comprising the glucosurics; with two there was no trace of a yellowish shade; with two others the shade was a little apparent; in the three last cases there was a precipitate.

The results so far obtained, left still some uncertainty; fer-

* Tartrate of Potassa and Copper.—Holt.
mentation offered us a more decisive means of verification; we had recourse to it with success.

**Fifth Experiment.**—The urine of four women, aged from seventy to ninety-two years, was collected, after having determined with the specimens that they gave, a reddish precipitate with the liquor of Barreswill. There was about the third of a quart of urine. At first treated by the acetate of lead and the carbonate of soda, as in the preceding experiments, it was reduced by evaporation to two or three spoonfuls, then placed in contact with the yeast of beer in a retort, the neck of which was adapted to a small receiver. We took no measures to collect the carbonic acid, the formation of alcohol sufficing to test the presence of sugar. By distilling at a gentle heat, about a scruple of colorless liquid was soon produced in the receiver. The receiver was then removed and slightly heated, while a burning match was presented to the orifice; a blue flame ran along the whole length of the neck, leaving an unequivocal odor of alcohol.

**Sixth Experiment.**—The same experiment was made a few days after, upon the urine of six women, aged also more than seventy years. The whole quantity was two-thirds of a quart, it was reduced by evaporation to four or five spoonfuls. This time, with the first product of distillation, equivalent to about three scruples, a bluish flame was obtained which did not cease to crown the neck of the receiver for eight or ten minutes, and which left a veritable odor of punch.

The alcoholic fermentation having been evident, therefore the urine experimented upon contained a considerable quantity of saccharine principle.

It was at first the intention of M. Dechambre and myself to find, whether there was any proportion between the degree of glucosuria and the age or decrepitude of the subjects. There very probably is; but the only tests which our time, so far, has allowed us to make, have not given a satisfactory result. In the third experiment, we took care to employ in each case the same quantity of urine and the same reacting portion, and classed them according to the abundance of the precipitates; doubtless a process but little rigorous, but, however, susceptible of furnishing some valuable signs. But, this classification was not at all
conformable to the advancement in age or degree of decrepitude. Certain women, very well preserved, notwithstanding their great age, having the skin still pliant, breasts well developed, and thorax but little deformed, gave a great deal of sugar—while others, entirely dried up, gave none at all, or in very small quantity. We understand, moreover, how many difficulties such a research ought to present, when we reflect that, in the same subject, the glucosuria may disappear from one day to another, or vary in intensity, as we have seen above.

**Diabetes.**

I now arrive at the disease especially characterised by the presence of sugar in the urine. Certainly, one is not sick because he has sugar in the urine, but because it is only found there after a derangement of the vital functions.

We do not pretend to characterise a disease by one of its symptoms: for it has been known ever since the time of Hippocrates, who said, *una natura, una confluxio, consentientia omnia*, that the organism is a whole, an unique assemblage; that no system of the isolated parts is subservient to one function exclusively: that all the functions harmonize with each other and contribute to the same end—so that a derangement in one function reacts upon the others. However, there are some functions which are more important than others, and whose alteration causes more disturbance in the others. In disease it is to be determined, what is the function primarily altered and which has caused the other troubles. In diabetes, we think that the respiration is primarily altered, and that the other disturbances, which accompany it, are only the effect. Besides, although for the most part, diabetic patients only die from pulmonary tubercles, we often see, when they die before the period of marasmus, that the lungs are perfectly sound. We consider this tuberculisation as an effect of the profound alteration of the function, and we believe, that one of the most frequent causes of diabetes, is a derangement in the functions of the vital point of M. Flourens, it being the motor, the centre of action of the respiratory apparatus. We will afterwards examine the theory, which explains diabetes by a want of alkalinity in the blood, and also that which accounts for it by
admitting an augmentation of the production of sugar by the liver; but before that, we will show that there is really, always a disturbance in the respiration.

Firstly, the presence of tubercles.

The quantity of carbonic acid expired, diminishes in diabetes, (Coindet,) also the animal heat becomes less (Bouchardat.) We know that the quantity of carbonic acid disengaged in respiration, is less during sleep; therefore the production of heat is less during sleep, and it is for that reason that we feel the want of better covering, and are more exposed to become cold.

All things equal in other respects, the quantity of sugar increases in the urine during the night, (Coindet,) so that if in the morning when the patient awakes, his urine is examined, much more sugar is detected than at any other time of the day. I said, all things equal in other respects, for it is evident, that after a meal at which a quantity of feculent or saccharine substances have been eaten, we find the greatest abundance of sugar in the urine.

We will now see that the quantity of sugar diminishes as the respiration increases. A moderate exercise accelerates the respiratory movements, increases the quantity of carbonic acid exhaled, and the absorption of oxygen is generally triple the normal condition (Prout, Scharling, Lassaigne.) Now, the quantity of sugar diminishes, according to Bouchardat, in the urine after a regular exercise, the labors of the field, in the free air, circumstances, which as we have seen, augment the combustion during respiration.

We know that the sugar disappears in diabetes, when they are laboring under an intense fever. This fact, so inexplicable before, is easily understood, when we recollect that the quantity of carbonic acid exhaled has been increased, so that the respiration being increased, the production of heat augmented, the sugar is destroyed as well as when the patient is subjected to exercise in the open air, and as much more, as the quantity of it is less on account of the diet which the patients are obliged to observe.

As in well characterised inflammations which do not disturb the respiration, the exhalation of carbonic acid increases, it is
not extraordinary, that when an inflammation takes place with a diabetic patient, his urine should not contain sugar. I think that this explanation is more satisfactory than to say, that in order to be diabetic, it is necessary to enjoy good health.∗

Let us examine the two principal theories, that have been produced upon the cause of diabetes.

Istly. That with diabetics, there would exist a continual source of sugar in the economy, independently of that taken in by means of the food. That this sugar would be formed in the liver, and its production would be so much increased, that the economy not being able to employ it, would reject it. There is an important fact against this theory, a fact which proves that the sugar rejected from the economy in the case of diabetics, proceeds from the feculent or saccharine matter ingested; for when these two substances are suppressed, the urine no longer contains sugar. All the physicians, who have had occasion to observe this cruel disease, agree upon that point. However, we will cite two authorities in support of our assertion:

"The proportion of sugar contained in the urine, is in constant conformity to the proportion of feculent or saccharine food." (Andral, pathologie interne, vol. ii., p. 447.) "It is a fact, evident to me, that the urine of almost all of the diabetics, subjected for several days, either to abstinence, or an exclusively animal regimen, exhibits no trace of sugar." (Loc cit.)

∗ We have already admitted, that the elements of our organs, which become unfit for their structure and functions because of their exercise, are burned by the oxygen and rejected under the form of uric acid, urea, &c. In diabetics, these substances diminish, to such a point in the urine, that for a long time their existence has been denied. That proves, that during this disease, the matters which give rise to (uric acid, &c., I suppose.—Hoff) them are not burned in the economy. Here, we can easily suppose two things which are equally true: 1stly, Because of the lessening of the disengagement of heat, the quantity of vital energy and also the nutrition of our organs diminish; consequently, the materials, which give rise to the uric compounds, diminish, and we easily understand, therefore, why they are found in such small quantity in the urine; 2ndly, The respiration being diminished, the combustion of the parts of our organs, unfit for life, can not take place as well as when the respiration is in a normal state. These elements, not being able to be rejected, remain in the economy, there undergo different transformations and give rise to the purulent, gangrenous &c., deposits, that we meet with so often in diabetic patients.
"It is a favorable circumstance, and I must say, the most common one to see the urine return to its normal quantity and composition, after twenty-four or forty-eight hours of a regimen from which feculent and saccharine aliments will have been rigidly excluded." (Bouchardat, du diabete sucré., p. 44.) This theory produces in its favor but one "exceptional" fact, reported by M. Andral, (Path. int., vol. ii., p. 450.,) in which, we see a patient, with whom sugar appeared in the urine, although subjected to an animal regimen. But if it is considered that this patient was at a hospital where he could not be watched, and therefore might easily have eaten the bread of his comrades, and that moreover he was only seventeen years of age, which will explain his discarding the regimen; this fact loses a little of its value. But, even supposing that the patient took neither feculent nor saccharine substances, should we base a theory upon one exceptional fact, expose a number of observations as exceptions? It appears to us more logical to wait until other like facts be acquired by experiment, before giving an opinion.

We have said elsewhere, and we repeat it here, that the sugar being normally destroyed in the economy, when it is not, we must admit, either that its quantity has increased beyond the destructive force of the organism, or that this force has diminished. If the quantity of sugar has increased, that may be accounted for in this way, that the economy receives by means of feculent and saccharine food more of it than it can destroy, and that the economy itself produces more of it than it can consume, independently of the feculent or saccharine aliments.

In the first place, there is a kind of instinct which regulates for us the quantity of respiratory food that we need; secondly, we have elsewhere said, that the sugar which is not immediately destroyed, may be converted into fat and deposited in the economy. Experiment has proved that, if in a healthy condition one eats a large proportion of sugar, it passes into the urine, and also that takes place when glucose is injected into the veins beyond certain limits. But that is not the case with diabetics. With them, the quantity of sugar which would be destroyed in a healthy condition, appears in the urine. We must therefore admit, that it is the destructive force which has diminished, and
we know, that this destruction is under the dependence of the respiration,

As to the excessive production of sugar by the liver without feculents, we have already said why we do not think it sufficient to account for diabetes.

2ndly. This theory has for its basis, the observations of M. Chevreul, upon the influence of alkalies upon the transformation of organic matters in the presence of oxygen. M. Mialhe has observed that grape or diabetic sugar has no reducing action upon the oxide of copper, either cold or warm, and that it does not acquire its disoxygenising property until after having been chemically influenced by an alkaline substance, free or carbonated; from that he has deduced, that it is by the alkalies normally contained in the blood and the animal liquids, that the transformation of the saccharine matter is produced. If the alkalinity is no longer sufficient, the transformation cannot take place; the sugar being no longer decomposed, or assimilated, is diffused throughout the economy, becomes a foreign body, and as such is rejected through the renal glands and all of the secretory apparatus.

"Diabetes then, recognizes as its cause, a defect in the assimilation of sugar, from a want of a sufficient alkalinity in the animal economy. In a healthy man, the blood is alkaline, and ought to remain alkaline, for the accomplishment of the inter-visceral functions. But the elements of acidity, constantly introduced into the economy, would tend to predominate, if they were not counterbalanced and eliminated by the especial secretions, the sweat and urine.

"These elements of acidity are:

"1stly. The ingestion of acids themselves.

"2dly. Exclusively azotised food; meats, on account of the albuminoid matters, contain a great deal of sulphur and phosphorus; these bodies, on account of their combustion in our organs, give rise to a great quantity of sulphuric and phosphoric acids, which are spread through all our humours, there saturate at first the alkaline bases which are there met with, and end by predominating.

"3dly. Defect in the perspiration of the skin, an emunctory destined to eliminate the acids of the economy."
M. Mialhe terminates the exposition of his theory by this phrase:—"So long as the presence of glucose in the urine, of herbivorous animals, normally alkaline, that is to say, the possibility of the existence of glucose in presence of an excess of alkali is not shown, I will remain unshaken in my convictions."

If we do not entirely share the views of M. Mialhe, we will at least avow that, this theory has led him to advise a treatment, the good results of which the physicians can daily determine.

In the following points I do not coincide with M. Mialhe. In order that the sugar should be destroyed in the economy, it is evident that the presence of alkalies is necessary, but there are also other indispensable conditions. If the oxygen is not of sufficient quantity, either because of the inspiration of irrespirable gases, or the impossibility of its entering into the pulmonary canals, if the structure of the lungs is modified, etc.; in short, if all of the normal conditions of respiration are not fulfilled, the sugar will not be destroyed, whatever may be the alkalinity of the blood. As an example: I made a rabbit respire ether; his urine contained sugar, and notwithstanding it was alkaline.

Among the elements of acidity, M. Mialhe placed meat, and, nevertheless, it is this element which, as nourishment, is most proper for diabetics.

An animal regimen is useful in diabetes, because the feculents and sugar, being absent in the food, can not remain in the economy, and thence we succeed in preventing a great many of the troubles, which are the effect of this abnormal presence of sugar in the blood. But the use of meat alone meets with only a temporary good; for as soon as feculents or sugar are again taken, the alarming symptoms of diabetes return. It is evident, that it is necessary to employ an animal regimen; but also it must be endeavored to destroy the cause which hinders the assimilation of sugar.

In the theory of M. Mialhe, if it is true that he has not expressed himself, contrary to the opinion that we have just announced, upon the animal regimen, it is evident, also, according to this theory, that with an exclusive animal regimen, the patient ought to grow rather worse than better, for this element of acidity would hinder the presence of alkalies, free or carbonated, and would thus render more abnormal the composition of
the blood, which would give rise to other troubles than those which are manifested when saccharine food is taken; so that, what we would gain by an abstinence of fœculents, we would lose by animal food.

The cause which determines the defect in the alkalinity of the blood, is, according to M. Mialhe, the suppression of the sweat, an emunctory destined to eliminate the acids of the economy, which, if they are not eliminated, prevent the presence of the alkalies in the blood, free or carbonated.

Under different climates, and in different periods of life, we see the sweat and urine always holding such a relation that they are always in an inverse proportion to each other. We always see that, when the sweat increases, the urine diminishes, and vice versa, and by this means the health is preserved, for always the same function of elimination is exercised, although by different organs. Besides the relation that is remarked in the production of these two secretions, we are convinced that they have a more intimate connexion, if the chemical composition, as well as the apparatus which served to separate them from the mass of the blood are examined and compared. In diabetes, the urinary secretion is to such an extent augmented, that we do not understand why this increase in the production of urine should not cause an equilibrium by the absence of the cutaneous perspiration. I think that this disappearance of the sweat, has been, for the most part, only the effect of the augmentation of the urinary secretion.

We can account for this augmentation of the urine, if we recollect a fact already noticed by Wœhler, that all the salts which are eliminated by the urine, render active the secretion of that liquid. The sugar, in diabetes, is a substance which can not be rendered useful by the economy and which is eliminated by the urine; it would render active the secretion of that liquid, as would any other diuretic whatever. The thirst, which accompanies the disease, would then be only the effect of the want which the economy feels for disembarrassing itself of this substance, and besides, it would be inevitable, because, in proportion as liquid is introduced, the liquid would be promptly eliminated on account of the activity of the urinary secretion. Moreover, the experiments of M. Bouchardat, prove that the blood of diabetics is equally alkaline as in the normal state.
But, even supposing that this theory should not be absolutely true, it will at least have served to show, that alkalies, in rendering active and facilitating the destruction of sugar, ought to be prescribed in diabetes. It is, perhaps, because they facilitate combustion, and thence the disengagement of heat, that the alkalies are general excitants.

The alkalinity of the blood, being one of the conditions in the destruction of sugar in the economy, may become, when it is altered, a cause of diabetes.

M. Bouchardat, has lately occupied himself a great deal with diabetes, especially with a view to its treatment. We will not examine his work; it is a little aside from our subject, which is to discover the causes of the disease.

In diabetes, the feculent and saccharine aliments are digested as in the normal state; only, in health, they are assimilated and destroyed during the act of respiration, while, in diabetes, they are not destroyed, because of a modification in the respiration. So that diabetes may be caused by anything which produces profound troubles in the function. But, almost always its origin is in the troubles of the functions of the nervous centres which preside over respiration.

We can go further, and we will be able to find other causes of diabetes, when we will understand better the usages and means of destruction of sugar in the economy, and then we will see that anything which interferes with the normal destination of sugar, may give rise to diabetes. As, in the actual state of the science, it is to the respiration that we attribute the destruction of sugar, and as we can explain, by the troubles in this function, the cases in which the urine becomes saccharine, we will therefore admit the alteration of the respiration as the cause of diabetes.

**Action of Curara.**

One of the most conclusive proofs of the theory that I have just exposed, was furnished me by a fact noticed by M. Bernard after my first experiments. This fact shows, that if the nervous system plays a part in the passage of sugar in the urine, we must consider its action as intercedent in the respiration, and not as exciting the glucogenic force of the liver.
M. Bernard found, that, when animals are killed with curara, there is a passage of sugar in the urine.

This fact enters naturally into the category of those that I have before observed. The curara, in effect, acts, as is proved by the researches of Munter and Virchow, in destroying, in abolishing the respiration, so that it kills rather by asphyxia than any other way. We can prolong life, for a longer or shorter length of time, by practising artificial respiration.

M. M. Pelouze and Bernard have proved on the other hand, that the curara destroys all the properties of the nervous system. We can not then say, that it excites them in order to react afterwards upon the liver. We might object, it is true, that before destroying, the curara excites to a high degree the nervous system; but then it would be necessary, to establish one hypothesis, to prove another.

The method of testing Sugar in the Urine.

We may employ three means to accomplish this end. The first, the most important, decisive and that without which we should never draw a conclusion, is fermentation, and I think it useless to relate here the procedure.

The second, is founded upon the property of the salts of copper to be decomposed by glucose in the presence of alkalies. This procedure is mostly employed, but it is also that, which may most easily lead to error; for in employing it we may suspect the presence of sugar where it does not exist, or remain doubtful when it does exist. We must then guard against these errors. For that, it is necessary to commence by separating the albuminoid substances; which also reduce the salts of copper in the presence of alkalies. For this purpose we add to the urine the tribasic acetate of lead; we pour into the filtered liquid the carbonate of soda, to separate the excess of the salt of lead; we filter again and concentrate the liquid. It is there that we should look for sugar, by means of the liquor of Barriswil, or the tartrate of copper and potassa. Unfortunately these precautions are always neglected, because one does not wish to devote the time necessary to it. Even in operating in the above manner, it is necessary to have recourse to fermentation, in order to draw a definite conclusion as to the presence of sugar.
The third procedure is, polarisation. To obtain good results, it is also necessary to precipitate the urine by the subacetate of lead, in order to discolor it and precipitate the albuminoid principles. But it will be necessary, again to control by fermentation the result thus obtained. The use of polarisation is somewhat delicate; but fortunately those, who would wish to employ it, have at their disposal the memoirs of M. Biot and the pamphlet of the Abbe Moigno.

I ought to make the general remark that, if sugar is looked for in urine which only contains a small quantity of it, it is necessary to operate upon at least three or four ounces of urine, which must be concentrated and then treated by the above procedures.

Note.—I thought that I ought to abstain from all indication as to the treatment of diabetes, and it is with regret that I have seen deduced from my experiments a method of treatment contrary to all scientific results. It has been reasoned in the following manner: "If diabetes proceeds from a defect of respiration, from a diminution in the combustion of sugar, nothing would be more easy, in order to render active this combustion, than to employ an air in which the oxygen would be in greater proportion than in the atmospheric air. And to obtain a much more active combustion it will be necessary to employ inspirations of oxygen. It is possible that the inspirations of this gas have produced good results in practice; but I am sure that these effects cannot be attributed to the fact that the combustion had been increased; for MM. Regnault and Reisset have proved that the respiration of animals, of different classes, in an atmosphere containing two or three times more oxygen than the normal air, presents no difference with that which is performed in our terrestrial atmosphere. The consumption of oxygen is the same, the connexion between the oxygen contained in the carbonic acid, and that consumed, undergoes no sensible change; the proportion of azote exhaled is the same; in short the animals do not appear to perceive that they are in an atmosphere different from the ordinary one. I think that if an augmentation of the respiration by changing the composition of the air was desired, it would be necessary to replace a part
of the azote by hydrogen and give to the patients to breathe a mixture of oxygen, azote and hydrogen, for then the consumption of oxygen is greater. But nevertheless I abstain from taking the responsibility of this indication, and if I have thought necessary to make it, it is because it is the only way in which the composition of the air can augment the consumption of oxygen.

**ARTICLE IX.**

*A Case of Spinal Meningitis, treated with large doses Quinine.*

By S. Z. Tatom, M.D., of Coweta county, Ga.

Mrs. B., æt. 34, was delivered of her sixth child, 10th June, 1853, by a midwife, and did well until the 14th, when she was attacked with epidemic dysentery. I was called to see her. Operations every fifteen or twenty minutes; muco-sanguineous; tormina and tenesmus great; pulse 110, quick, small; tongue coated around the edges and tip; skin dry and hot; hypogastrum tender; lochia scanty and offensive; strength exhausted.

*Treatment.*—B. Opii., gr. ¼; Acet. Plumb., grs. 2; Camphora, gr. 2: to be given every two hours, according to circumstances. Enema tinct. opii, acetate plumbi, and starch; warm fomentations over abdomen; vaginal injections, with diluted milk 98° Fahr. Continued above treatment, with but little variation, until the 18th; dismissed case.

June 24th. I was called to see patient. Found the flexor muscles of extremities rigidly contracted, those of face occasionally twitching; tongue partially paralyzed; voice indistinct; formication, tingling, &c.; perspiration and deglutition difficult; drawing sensation in the region of diaphragm; countenance anxious; pulse 80, regular, small and soft; skin cool, soft; bowels regular; lochia natural; abdominal tenderness subsided; tongue good; three inferior cervical and two superior dorsal vertebrae very tender on pressure.

*Treatment.*—B. Opii., gr. ¼; musk, grs. 2; quinine, grs. 2; made into pill. One every 2½ hours. Cups over tender part of spine, followed by blister, and stimulating liniments to spine and extremities.

June 25th, 8 o'clock, A. M. Found patient as I left her. Continue same treatment.
June 26th, 9 o'clock, A.M. Patient is worse; says she will die. Diarrhoea copious; paroxysms of dyspnœa, resembling that of croup; croupal sound; excruciating pain in the region of diaphragm; “feels as if something was drawing her in two;” deglutition almost impossible; abdominal muscles contracted; can’t straighten herself; sits in half recumbent position; pants for breath; fist firmly clenched; legs flexed upon thighs; feet drawn; toes at almost right angles to feet; pulse 110, small and weak; tongue red; skin dry and hot. Prescribed, enema: Tinct. opii., acetat. plumbi. and starch, in cold mucilage, which arrested diarrhoea. Having exhausted my catalogue of narcotics and antispasmodics to no avail, I tried galvanism, the warm bath, and a host of adjuvants; but patient continued to grow worse until 10 o'clock, A.M., next day (27th). Another physician was sent for; but before he arrived, as patient was apparently near her dissolution, I determined on venesection and large doses of quinine, as a dernier resort (which was concurred in by the Doctor, on his arrival). She was accordingly bled from the arm 10 oz.; pulse began to fail. Prescribed: Quinine, grs. 15; sulph. morphia, gr. ½; in solution, every hour, which was continued fifteen hours; at the expiration of which time, every muscle was relaxed; breathes easy; says she feels well; but thinks she “can’t hear good; will soon get fat on that bitter medicine.” Continued the same treatment for two days, but in modified doses. The patient, however, swallowed an ounce of quinine in the space of three days, and with proper treatment directed to the spinal meningitis, (complicated, perhaps, with myelitis,) was restored to health.

Thus was a daughter, wife and mother, rescued from the iron grasp of death, by heroic, and, I might add, empiric doses of quinine.

 ARTICLE X.


Was called to Mrs. L., on the first January, 1854, about one o’clock. Found her in the first stage of labour, suffering very much from the pains attendant on this stage. Upon inquiry,
ascertained that her pains commenced about 8 o'clock, and had recurred regularly with diminished intervals. An examination per vaginam revealed the head of a child presenting in the first position, os uteri dilated to the size of a dollar, and mucous secretion from vagina profuse. Expulsive pains commenced about 7 o'clock, P. M., irregular and ineffectual for two or three hours, and then subsiding almost entirely. At 10 o'clock, gave her an opiate, and feeling unwell, retired to bed, leaving instructions with an old midwife to wake me up if any thing unusual occurred. Was aroused about two o'clock, and found my patient staring vacantly, with pupils much dilated, and slight twitching in the muscles of her mouth. She went off immediately into a convulsion, which was relieved by opening a vein in her arm and permitting it to bleed until she began to recover. Pains increased in violence and regularity, until she was delivered of a male child. The womb not having been diminished much in bulk, I was led to suspect a plurality; which suspicion was confirmed, by detecting on examination, per vaginam, another head, presenting in the same position as the first. She was delivered of a second foetus, female, about an hour and a quarter after the birth of the first, and appeared remarkably well, considering the length of labour and suffering. Expressed sorrow at the death of her children, both of which were still-born, but made no complaint of head symptoms—nor could I detect any thing in her appearance forewarning a return of the convulsions. I had retired but a few minutes, when I was summoned again to my patient, and found her going off into another convulsion. The external and internal recti muscles of the eyes were acting alternately, throwing these organs from one side to the other of the orbits. During this convulsion, I attempted the delivery of the placentas, and was constrained to detach the entire connecting surface of both, before I could succeed. This convulsion was undoubtedly apoplectic, there having been but little agitation in her muscular system. She soon passed into a comatose state, with stertorous breathing, and death closed the scene in about half an hour.

There was nothing unusual in the appearance of the second child, but the features of the first were very much distorted, the corner of its mouth being completely drawn to one side.
Professor Mettau er is an eminent and successful practitioner, whose views are entitled to high regard. The following contributions will therefore be doubtless found useful to many of our readers.—[Editor.

Contributions to Pharmacy. By John P. Mettau er, M. D., LL. D., of Virginia, Professor of the Principles and Practice of Medicine and Surgery, in the Medical Department of Randolph Macon College.

It will not be denied that the operation of therapeutical agents is essentially influenced by the mode by which they are prepared.

This fact, so generally true, is particularly exemplified in the preparations of cinchona, cantharides, colchicum, guaiacum, and several other medicinal substances of which I shall speak presently.

For more than twenty-five years, my attention has been particularly directed to this subject, and, during this period, I have adopted several new methods of preparing some of the articles of the materia medica, and have satisfied myself, by repeated practical trials, that these preparations possessed superior efficacy to those generally employed.

Many years ago I prepared an acetous infusion of cantharides,* for blistering purposes. This infusion was first designed for vesicating the scalps of infants, without removing the hair; and its action was very satisfactory. It was applied simply by wetting the surface of the head, and the hair nearest its roots, and then carefully covering the parts with a cabbage leaf, or oiled silk, to prevent the too sudden evaporation of the blistering fluid. When other parts of the body were to be blistered, a thin compress of bibulous paper, or cloth saturated with the infusion, was applied to them, and carefully covered with oiled silk. To insure speedy and effective vesication, I usually re-applied the tincture two or three times, after intervals of half an hour. I found this agent equally as efficient and certain in its action with adults as with infants. It rendered the removal of the hair unnecessary, as it blistered every part of the surface, even when a very thick head of hair existed. This preparation has been used by many of my medical friends, and with entire satisfaction. Within the last ten years, I was induced to prepare an ætherous solution of cantharides† as a vesicant, and have found it far more prompt

* Rx. Canth. contus., §iiis.; Acid acet., Oij. Digest for 14 days, and filter.
† Rx. Cantharid. contus., §iiij.; Spirit. æth. nitric, Oii. Digest for 8 days, and filter.
and certain in its operation than the acetous infusion. It may be applied in the same manner as the latter. Frequently, merely wetting the skin with the solution, without covering the part, will blister; especially in infants. When adults are to be blistered, the preparation should generally be applied with a thin compress, and carefully covered, as already suggested,—moistening the compress from time to time, until the skin is decidedly reddened. I have found this by far the most convenient and reliable means of blistering that I have ever employed. This _cetherial_ tincture of cantharides is also an efficient internal remedy. As an emmenagogue and diuretic it has greatly exceeded my expectation. The _cetherous_ menstruum seems not only to promote the operation of the cantharidin upon the genito-urinary organs; but at the same time to guard against strangury. I now use this preparation of cantharides almost exclusively, both externally and internally, when the _lytta_ is indicated, and have done so for seven or eight years.

The remarkable efficacy of the _cetherous_ preparation of the Spanish Fly induced me, five years ago, to employ spirits of nitric _cether_ as a menstruum for _cubeb_ , _colchicum_ , _guaiacum_ , _squill_ , _ergot_ , _gossypium_ , _sanguinaria_ , _ipecacuanha_ , _digitalis_ , _nux vomica_ , and some other articles of less importance. The _cetherous_ tincture of _cubeb_ is a most valuable remedy in all the sub-acute inflammations of the bladder, of the urethra, of the uterine cavity, and of the mucous lining of the stomach and intestines. It should be administered in some mucilaginous vehicle.

The tincture of _colchicum_ is applicable to the treatment of all of the cases demanding the use of the _colchicum_ , and is decidedly preferable to the vinous seminal tincture now in use, by reason of its tendency to act on the urinary system. It is very well adapted to the treatment of sub-acute rheumatism, _gout_ , _œdema_ , and neuralgic rheumatism, especially if the urinary secretion is materially diminished in quantity. In the bloating occasionally connected with _dysmenorrhœa_ , a combination of this tincture with the _cetherous_ tincture of cantharides, _sanguinaria_ and _gum guaiacum_ will be found a most valuable remedy. It should be taken three or four times daily in an infusion of pine tops, in doses of ten to twenty drops each. The same combination will also be found valuable in the sub-acute stage of _gout_ and rheumatism.

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*Ρ. Pip. cubeb. contus., ʒiv.; Spirit. _ceth._ nitric, Oij. Digest for 8 days, and filter.

†Ρ. Sem. colchic. contus., ʒiv.; Spirit. _ceth._ nitric, Oij. Digest 10 days, and filter.
The ætherous tincture of gum guaiacum* is superior to the preparations of that article now in general use in the treatment of rheumatism by reason of its tendency to act on the urinary system; and the same may be said of it as an emmenagogue when there is rheumatic irritation of the uterus as an associate cause of dysmenorrhœa.

The ætherous tincture of squills† is adapted to all cases in which squills are indicated, and is an elegant preparation. In dropsy, œdema of the mucous lining of the larynx, and of the lungs, in asthma, and as an expectorant and diuretic it will be found a most convenient and valuable preparation. A combination of equal parts of this tincture and of the syrup of lobelia inflata taken three or four times daily, in doses of 3 ss. to 3 j. each, is the most efficient remedy I have ever used in asthma.

The ætherous tincture of ergot‡ is best suited to cases of inaction or torpor of the uterus connected with debility or exhaustion; it may be used either as an emmenagogue or as a parturient. In uterine haemorrhage, or menorrhagia dependent on debility, or exhaustion of the uterus, it will be found a valuable remedy. Its action upon the uterus is greatly influenced by the ætherous menstruum. It is best to give it in some diuretic vehicle, such as pine tops tea, or flax seed or elm tea; and it may be taken in doses of 3 ss. to 3 ij. once in four or five hours.

The tincture of gossypium§ is possessed of properties very similar to that of ergot and may be employed in like doses with it, and in similar diseases.

The tincture of sanguinaria∥ is valuable when combined with the tinctures of cantharides, gum guaiacum, colchicum, cubebs, and indeed any other emmenagogue, in the treatment of dysmenorrhœa. It is also a valuable expectorant and diaphoretic in pneumonia, bronchitis, and œdema of the mucous lining of the air passages. It is administered in doses from 3 ss. to 3 ij., once in three or four hours. This tincture may also be employed alone as a diaphoretic and expectorant.

The ætherous tinct. of ipecacuanha¶ is so closely assimilated

* B. Guaiac. gum. resin, 3 iv.; Spirit æthe. nitric, Oij. Digest 8 days, and decant.
† B. Seill. marinim. contus., 3 iv; Spirit æth. nitric, Oij. Digest 8 days, and filter.
‡ B. Ergot. contus., 3 ij; spirit æth. nitric, Oij. Digest 10 days, and filter.
§ B. Gossypii. herbac, 3 iv.; Spirit. æth. nitric, Oij. Digest for 10 days, and filter.
∥ B. Sanguinar. canadens. contus. 3 iv; spirit æth. nitric, Oij. Digest 8 days and filter.
¶ B. Cephael. ipecac. rad. contus., 3 ij.; spirit. æth. nitric, Oij. Digest 8 days, and filter.
to the tincture of the sanguinaria in its therapeutical properties, as to be applicable to the treatment of the same diseases. It is an elegant and most convenient preparation. In typhoid fever it will be found far superior to the ipecac pill as a diaphoretic, especially when the tongue is dry and the thirst urgent. It may be used also in typhus fever, or indeed in any febrile affections during the sub-acute stage. This valuable preparation acts both as a diaphoretic and diuretic in these cases, as well as an expectorant.

The cethereous tincture of digitalis* is a far better preparation than the alcoholic, on account of its greater activity; and this it derives chiefly from the cethereous menstruum. In doses from $\frac{3}{5}$ ss. to $\frac{3}{5}$ j., in some diuretic infusion, taken three times daily, it will be found well adapted to all such cases as require the foxglove.

The cethereous tincture of nux vomica† is especially indicated in the treatment of seminal debility, or to speak more properly, debility of the generative organs. In this, the gravest of human ills, after such preliminary treatment as may be demanded for the correction of constipation, and prostatic tenderness, this tincture will be found a most excellent means of restoring the erections. It is also valuable in exciting appetite for food, and in the invigoration of the digestive organs. This preparation is well adapted likewise to the treatment of paraplegia, especially when the bladder and rectum are implicated, as well as such other forms of paralysis as demand the nux vomica or its alkaloid. It may be given in doses from $\frac{3}{5}$ ss. to $\frac{3}{5}$ iss. three times daily, before or after meals, in some bitter infusion. The cold infusion of wild cherry bark I have generally preferred as the vehicle for it.

The cethereous solutions or tinctures are more readily prepared, requiring to be digested for a less time than the alcoholic, and keep without the least deterioration. They are also adapted to those conditions of the constitution in which alcoholic menstrua would be objectionable.

*§* Hydrargyrum cum creta. This valuable preparation of mercury is usually formed by triturating $\frac{3}{11}$ j. mercury with $\frac{3}{10}$ v. of prepared chalk, until the globules are extinguished. This is a tedious process, and the resulting powder is not of uniform strength, nor is the mercury completely rubbed down. Indeed, it is questionable whether the powder, when apparent-

*§* Rx. Digital. purp. fol., $\frac{3}{11}$ ss.; spirit. ceth. nitric., Oij. Digest for 10 days, and filter.

†§* Nucis vomicae pulv., $\frac{3}{11}$ j.; spirit. ceth. nitric, Oij. Digest 10 days, and filter.
ly well formed, always contains mercury, as a compound may
be readily formed by uniting other colouring substances with
chalk, to imitate blue mercurial powder; and I think I have
met with such imitations several times. The blue powder that
I have procured from the shops has generally disappointed me;
and for a number of years I have prepared it myself according
to the following method:

Take one part of pure starch; eight parts of prepared chalk;
and sixteen parts of mercury. Reduce the starch to fine pow-
der. The chalk may now be added, and after being well
mixed, the mercury can be united. The powder must next be
moistened with water, but not to the extent of wetting it; and
the whole rubbed until nearly dry, when the mass should be
again moistened and rubbed dry. In this manner the process
must be repeated from time to time, as may be convenient,
until the powder assumes a uniform bluish appearance. After
the chalk seems to be saturated with the mercury, rub the
mass perfectly dry, and then moisten it sufficiently to make it
adhere to the surface of the mortar by pressing with the pestle.
By carefully passing the pestle over the adhering mass, so as
to render its surface smooth, the superfluous mercury will now
escape from it in small globules and fall to the bottom of the
mortar, and the separation may be facilitated by striking the
bottom of the mortar against the table repeatedly, and by pour-
ing the mercury over the surface of the mass where any glob-
ules appear. The mercury may now be removed from the
mortar; and as soon as the mass becomes sufficiently dry, the
trituration must be renewed and continued until the mass be-
comes a smooth, dry powder. Prepared according to this
method, I have used blue powder in my practice more than
twenty-five years, and have uniformly found it far more certain
in its operation than that obtained from the shops. I prescribe
it in the ordinary doses, or nearly so, and yet I am satisfied it
is stronger than that in general use. I invariably direct it to
be administered nearly dry, united with brown sugar, and to
be mixed in a cup by stirring the powder and sugar together
with a straw or the point of a knife. The dose may then be
taken into the mouth and swallowed, first with the saliva, and
afterwards with a mouthful of water. This powder should nev-
er be mixed in a silver spoon, or any other utensil possessing
an affinity for mercury, or the powder may be rendered entire-
ly inert; and such an accident once befell a patient of mine,
who nearly lost her life before the cause of the failure of the
medicine in producing its proper effects was discovered.

[Virginia Med. and Surg. Journal.]
Philosophy of certain Dislocations of the Hip and Shoulder, and their reduction. Read before the Detroit Medical Society. By M. Gunn, M. D., Prof. of Surgery in the University of Mich.

The views here advanced I have taught for the past two years to the gentlemen composing the Medical Class in the University; and I shall offer no apology for calling the attention of the Society for a few moments this evening to the subject of Dislocations of the Hip and Shoulder, and more particularly to that form of the accident, which, from the anatomical peculiarities of the joint, is one exceedingly difficult to reduce; and for the reduction of which Dr. Reid has but recently proposed a novel and efficient mode.

It is not my intention to discuss the question of priority which has been raised in reference to this subject, for there can be no doubt that Dr. Reid arrived at his conclusions by a course of reasoning and experiment; and that those conclusions were most essentially novel to a large majority of the profession. I propose rather, briefly to consider the prominent peculiarities of the joint, and the relation of the parts in a state of dislocation; the structures which oppose the return of the head of the femur to the acetabulum; the manner in which Dr. Reid's manipulations overcome this opposition; and lastly, the application of the principle involved, to the reduction of some other dislocations.

The encircling ridge which gives depth to the cotyloid cavity, presents upon its outer slope a plane, the inclination of which varies in different parts. At its posterior portion this inclination is very great, and it would seem in dislocation in this direction impossible to return the head of the bone to the cavity without lifting it completely over the ridge; upwards and backwards it is more gradual, and would seem to afford a much more easily surmountable obstacle; yet when we examine the relation of the parts in a dislocation in this direction, we find that applied to this surface, we have the anterior and inferior surface of the head and neck of the femur, the rotundity of the head corresponding with the curvature of the slope, while the edge of the acetabulum corresponds with the curvature described by the anterior and inferior surface of the neck. Although thus seemingly locked together, comparatively slight extension in the line of dislocation would cause the head to ride over the edge of the cavity, were it not bound down in this position by the surrounding tissues. Which particular tissue constitutes these bands is an important question to him who seeks to relax them. Dr. Reid, in common with
the profession generally, considers the muscles the agents which thus oppose our efforts at reduction, and his manipulations are conducted with a view to relax them, while the femur acting as a lever, the head of the bone is raised clear of the edge of the cavity. With this same view we have the directions of the books and public teachers to apply extension and counter-extension slowly and uniformly in order to tire out the rebellious muscles. Blood-letting, antimony, and the hot bath are also called in to aid in this laudable crusade against these wicked organs.

In this view, I would respectfully differ with Dr. Reid, the teachers, books and profession, and state my honest belief that the muscles oppose our efforts very little more than they do the progress of our earth in its orbit. This belief I have repeatedly verified by experiments upon the dead subject, and the members of the medical class of 1851-2 in the University will remember those conducted before them. A subject was placed upon the table, the lower border of the gluteus maximus was raised, and a scalpel carried through the subjacent muscles, and an opening made in the posterior and superior portion of the capsular ligament. The round ligament was then divided, and the head of the femur luxated upon the dorsum of the ilium. The usual indications of this dislocation were present. The subject was placed in the proper position, a counter-extending band applied to the perineum and fixed; the strength of two men exerted now upon the extending band, while endeavor was made to raise the head of the bone clear of the acetabulum with a jack towel, was insufficient to reduce the luxation. Reid's method of manipulation readily replaced the bone. This experiment was repeated many times, and uniformly with the same result. As muscular action could not have opposed our efforts and prevented success in this case, the question naturally presents itself, what structure stood between effort and success? I answer, the untorn portion of the capsular ligament. In support of this view, let us consider for a moment the position of the limb at the instant of escape of the head from the socket during the process of dislocation. To do this we must bear in mind that force applied to the knee or foot while the limb is in a state of adduction, constitutes the most frequent cause of this dislocation. Force thus applied adducts the limb still more powerfully before dislocation takes place, and at the moment of the escape of the head of the bone from the socket, the limb is in a direction which crosses the thigh of the opposite side. Immediately that the head of the bone has cleared the edge of the acetabulum, it settles into its position upon the dorsum of
the ilium, and the limb assumes she position and direction indicative of the accident. During the dislodgement of the bone, the superior and posterior portion of the capsular ligament is ruptured, through which the head protrudes; while from the position of the limb at the instant of protrusion, the anterior and inferior portion is very much relaxed, thus allowing the head to ride easily over the acetabulum. As soon as the head settles into its position upon the dorsum of the ilium, the direction of the limb is changed, and the untorn portion of the ligament becomes more tense, and for this reason the head of the bone cannot be readily returned to its place, till the limb is again placed in a position to relax it. Dr. Reid's method does this most effectually, and I conceive that any other plan which does not accomplish this, as for instance extension and counter-extension by the pulley, or Jarvis's apparatus, in the usual direction, succeeds only by lacerating much more extensively, if not by actually tearing the ligament completely asunder, before the head of the bone will ride over the edge of the cavity.

The principle, then, I would seek to establish, is this—that in luxations of the hip and shoulder, the untorn portion of the capsular ligament, by binding down the head of the dislocated bone, prevents its ready return over the edge of the cavity to its place in the socket; and that this return can be easily effected by putting the limb in such a position as will effectually approximate the two points of attachment of that portion of the ligament which remains untorn.

This principle can be successfully applied to the reduction of the backward luxation of the femur into the ischiatic notch, and also to the several luxations of the shoulder. It has several times been my guide in the reduction of the downward dislocation of the humerus into the axilla. The patient is seated upon the floor, an assistant slowly raises the arm to an angle of forty-five degrees to the plane upon which the patient is sitting; and now while the assistant makes extension in this direction, the surgeon makes pressure with the hand upon the top of the shoulder, the bone readily returns to its place, and the arm is dropped to the side and secured in a sling.

White's method of reducing this luxation, which is figured in Druitt, is essentially the same, the only difference being in the position of the patient. According to his plan the patient lies upon his back, the scapula is fixed by a counter-extending band applied to the top of the shoulder, or by the hand of an assistant, while "the arm is raised from the side, and drawn straight up by the head, till the bone is thus elevated into the socket." In either method it will be seen that the upper and untorn portion of the capsular ligament, by the elevation of the
arm is very much relaxed, thus giving a latitude of motion to
the head which greatly facilitates its return, and which could
not be obtained by any manipulation in which this relaxation
was less perfect. Nine-tenths of the force spent in extension
and counter-extension may be spared, in the reduction of all
those dislocations in which, by alteration of the position of the
limb, such relaxation is effected; and in the several luxations
above specified this end is undoubtedly attainable.—[Peninsu-
lar Journal of Medicine.

Two Cases of Traumatic Tetanus, successfully treated by Ice.
By B. D. Carpenter, M. D., of Patchogue, Suffolk County.
Long Island.

Case I. Aug. 22d, 1849.—E. G., aged 16 years, of good
constitution and habits, jumped from a fence on the stump of a
twig some half inch in diameter; which made a wound in the
ball of the right foot three-fourths of an inch deep. Twelve
days after the accident he complained of feeling lame and stiff;
during the night, was awakened by a violent spasm; the next
day, complained of stiffness and soreness of the muscles of the
neck and throat, and pain at the scrobiculus cordis; the fol-
lowing night, during sleep, was seized again with spasm; and
the next morning, when I was sent for, I found him complain-
ing of pain in the above region, great rigidity of the whole
muscular system, attended with difficulty in swallowing and
constraint in moving the head and jaws, and in articulating.
During the spasm, the body was curved backwards and thrown
to one side, the dyspnoea was considerable, pulse full and slight-
ly accelerated, skin warm and moist, bowels costive, urine
scanty and high-colored.

Administered a purgative, which was assisted by enemas.
The patient was then put upon the free use of opium in the
shape of Dover's Powder, and the bowels kept open by the use
of cathartics and injections of $\frac{1}{2}$ tinct. assafcetida, in half a
pint of soap suds, repeated as often as the preceding one came
away. This treatment was continued for four days, during
which time he gradually grew worse. The tetanic rigidity
and spasm increased until the sixth day; when, finding he must
die unless something farther could be done to allay the pain
and extreme spasm, and viewing the difficulty as being an ir-
ritation of the spine, perhaps connected with congestion of the
membranes covering the spinal marrow, I determined to apply
ice to the head and the whole length of the spinal column,
since the whole muscular system was affected. I did so, and
in ten minutes had the satisfaction of seeing the pulse come
Traumatic Tetanus.

down from 110 to 75, and all the urgent symptoms relieved; the rigidity was gone, and he had but one spasm after the ice was applied; his bowels were kept open, and assafoetida injections were continued twice a day, to allay the irritability of the nervous system, manifested by slight twitchings. No medicines were given by the mouth. The wound entirely healed, and in three days the patient was discharged cured; and his health since has been as perfect as before the attack.

Case II. Aug. 11th, 1853.—A. C., 21 years of age, a robust farmer, in good health, in assisting to remove some old lumber, stepped on the point of a rusty nail, which entered the hollow of the foot to the depth of three-fourths of an inch. The wound was not very sore, and was dressed with some simples by himself; and he remained at work moderately until the 16th, five days after the accident, when he complained in the afternoon of twitching in that foot and slight pain in the region of the wound and leg of that side. Was quiet the rest of the day, and retired early to bed, but slept none from restlessness, anxiety, and slight pains and twitching of the nervous system. On the 16th, felt some pain in the head and through from the lower end of the sternum to the back. I saw him at 6 P. M., and found him complaining of pain as above mentioned, which had gradually increased at the sternum, great rigidity of the muscles of the left side of the neck, accompanied with slight dyspnœa and some difficulty in swallowing. Even at this time there was present the peculiar expression of countenance found in tetanus. Pulse 100 and hard; bowels costive; had eaten nothing; the wound had not commenced to heal, and was covered slightly with a thin serous discharge. Made a free incision into the wound, and dressed it with bread and milk poultice, to which tinct. opii was added; ordered ten grs. of calomel with ten of rhei, to be followed by pil. colocynth. comp. until the bowels were freely moved, and enemas of tincture of assafoetida, ʒ j every three hours, or as often as the preceding one should be voided, large doses of Dover’s Powder by the mouth, and to have the neck bathed in camphorated oil and tinct. opii. 18th, 7, A. M., found that the bowels had been freely moved, and that spasm of the whole muscular system had commenced. About 3, A. M., pain in the neck and at the sternum increased, and there was great rigidity of the muscular system generally, dyspnœa great, much difficulty in swallowing and articulation, jaws partially closed, entirely so during the spasm, pulse 120; indeed, all the symptoms increased in a marked degree, with slight delirium. Ordered one-fourth of a grain of morphine every hour, and to continue the assafoetida injections. 6, P.M., all the symptoms greatly aggravated, pulse so small and fre-
Aphonia, cured by Electro-Magnetism. [March,

quent that it could not be counted, jaws closed, breathing extremely difficult, body almost constantly drawn backwards or forwards and to one side, face pale, skin moistened with clammy sweat, and perfect rigidity of muscular system. Had slept none for 48 hours. Applied ice to the head and whole length of spinal column; in twenty minutes the pulse came down to 100, the skin was covered with profuse perspiration, the muscular system relaxed; in short, there was a perfect yielding of all the urgent symptoms, and the patient slept soundly and pleasantly for the succeeding two hours, during which time the breathing was natural, and there was neither tetanic rigidity or spasm. When he awoke there was still some delirium, the pain in the region of the sternum was very great, and for half an hour the tetanic rigidity and spasm were considerable. The ice was again applied, when the symptoms immediately yielded, and the patient (with the exception of short intervals) slept quietly the balance of the night.

17th, 6, A. M., the bowels were moved by the assafetida injections, the delirium had passed off, the tetanic rigidity was gone. Pulse 80, breathing natural but said there was great soreness of the chest and all the muscles of the body. Drank some soup, continued the ice and injections as before. 11, A. M., there was some slight twitching of the muscles, without rigidity; from this time the patient continued to improve without either tetanic rigidity or spasm until, on the 25th, he was discharged cured, with the wound nearly healed.

The ice was applied from ten to thirty minutes each time, with intervals of from two to eight hours.—[Ohio Med. and Surg. Journal.

Aphonia of Four Years' Standing, cured by Electro-Magnetism. By F. K. Bailey, M. D., Almont.

Mrs S——, aged 79, in the spring of 1849, had a severe attack of Bronchitis, which was relieved by appropriate treatment.

On regaining her general strength, however, her voice was at times very hoarse, and at the close of the day it was difficult to speak loud at all. In the course of six months from the first attack, there was a complete Aphon a, which continued until last April.

At that time she was induced to make a trial of Electro-Magnetism. In a few days after this means was tried, her voice became more distinct, but very rough at first. In the course of a week or two, speech was natural, and has continued until the present time.
The favorable result in this case may lead to the use of Electro-Magnetism in other affections produced by want of proper innervation. I will add, the apparatus used was one manufactured by Charles Crosman, Detroit.—[Peninsular Journal of Med.

EDITORIAL AND MISCELLANY.

An Inquiry into the Nature of Typhoidal Fevers; based upon a consideration of their History and Pathology. By Henry F. Campbell, M. D. Presented to the American Medical Association, at its session of May, 1853.

Typhoid fevers have at all times engaged a large share of attention, and there is perhaps no disease which has enlisted more talent and patient investigation than this. Yet its pathology is the subject of as great discrepancy, and its treatment of as much empiricism, as any in the nosological catalogue. Whether the essay before us will furnish the profession a satisfactory solution of the long mooted questions or not, remains to be determined. The argument is certainly very plausible, and cannot fail to arrest the attention of pathologists. We regard the appearance of any thing new upon Typhoid fevers as peculiarly opportune, since they are becoming daily more and more within the domain of Southern practitioners, who constitute the great mass of the readers of this Journal. It affords us pleasure, therefore, to present the following notice of Dr. Campbell's very interesting work.

The design of the essay is to demonstrate the dependence of Typhoidal fevers upon an abnormal condition of the ganglionic or sympathetic system of nerves. In the accomplishment of this purpose, the writer first presents a critical analysis of the symptoms and pathological lesions observed in these fevers, and then studies the anatomical and physiological relations of the nervous system of organic life. From these premises naturally flow the pathological deductions which are, however, so ingeniously wrought out that, in order to do full justice to the author, we append them in his own language.

"Having thus carefully recounted the more important features in the anatomy and physiology of the sympathetic system, it remains but to consider them in connection with the phenomena of typhoidal fevers. Such a review will be attended with the following results: In the first place, the essential symptoms of typhoid
fever are located in organs deriving their innervation principally, and in many instances entirely, from the ganglionic system. In the organic or involuntary muscles—as for instance the heart's—of which, during life, we find the frequency increased, the force diminished, and the regularity impaired—all of which effects must be plainly attributable to the altered innervation of the organ. After death, we have seen it the subject of very material alteration; its substance is flabby, pale, and much softened, so that it breaks readily under the fingers. Instance again, the muscular coat of the intestine; we have meteorism—an almost invariable symptom in typhoid fever—and which we may legitimately refer to the loss of tonicity in the muscular coat of the intestinal canal, from impaired innervation of that coat, by which condition, together with the altered state of the secretory surfaces, the passive accumulation of gas in the intestines is allowed, and hence the tympanites.

"That this altered condition in the innervation of the organic muscular fibre does exist, is shown most remarkably in the mode of dying in some cases, viz., that mode termed asthenia, 'occasioned by causes acting directly on the circulatory forces, affecting the vis nervosa, upon which the contractile property of the heart depends."* and farther, that this depression in the involuntary muscles has no invariable correspondence in the state of the voluntary muscular system; as we shall find remarkably illustrated in the observations of Dr. Flint.† 'In some of the cases attended with most danger, and some of them ending fatally, the muscular strength was retained in a surprising degree. In two fatal cases of the typhoid type, characterized by active persistent delirium, the muscular efforts were almost constant and quite strong up to a few hours before death. One of these cases terminated on the ninth day, and the other on the third day after coming under observation. The mode of dying in each was by asthenia, or, perhaps, more properly, necromia; the system of involuntary muscles exhibiting reduction of force to a degree incompatible with life—the voluntary muscles remaining active. This is a curious fact.'

"This relative condition of the voluntary and organic muscular systems, appears to impress even the observing and philosophic mind of Dr. Flint as almost inexplicable; and it is not surprising that it should, when we consider that his views of the pathology of typhoid fever have no fixed or definite reference to the organic system of nerves; but, on the admission of the ganglionic pathology of the disease, the full interpretation of these phenomena, besides many other similar facts (meteorism) before inexplicable, become easy and natural.

"II. Besides the organic muscular system, which we have just shown to be under the influence of ganglionic nervous aberration, we find that the other characteristic phenomena of typhoid fever refer to the functions of nutrition and secretion, both of which important processes depend upon the vascular system, which, especially in the viscera, is admitted to be under the sole dominion of the sympathetic nerve. And, what is more remarkably illustrative of this fact, is, that

* Flint, p. 125.
† Reports on Continued Fever, p. 59.
there appears to be a very close relation between the amount of disease observed in any particular portion of the organism—the alimentary canal for instance—and the degree to which it is indebted to the ganglionic system for its innervation; thus we find but a small amount of disease, congestion, seldom any ulceration, in the larynx; ulceration is somewhat more common in the pharynx, oesophagus, and stomach, though still not abundant. It disappears in the duodenum, which receives but few sympathetic filaments, and again appears in the upper portion of the ileum, increasing, as we descend, in direct proportion to the amount of ganglionic fibres the part receives, till it reaches its maximum in the lower portion, where the nervous supply is very abundant; after which, we find ulceration occasionally in the cæcum, still less frequent in the colon, till in the rectum, whose innervation is principally from the cerebro-spinal system, it is never observed. So, likewise, with regard to the other organs; we find the liver, lungs, and spleen are all subject to congestions, which can be referred to the same abnormal innervation of these viscera.

"From the relative unfrequency of disease in certain portions of the abdominal viscera, and elsewhere, Louis, as we have seen, though admitting their diagnostic importance, is disposed to view them as results, secondary to the lesion in the ileum; we cannot, however, agree with him, but are compelled to regard them as the common primary results of a common cause which exists in the ganglionic system, and that the frequency or the gravity of disease in any one of these organs is determined alone by the amount to which the ganglionic ingredient mixes with, or enters into its innervation, and that disease in these localities has no etiological reference whatever to that in the ileum; but, when it exists, is as significant of the true pathology as is the ileitis—for it invariably indicates, both by its location and character, that its origin is abnormal innervation.

"So far then as regards the localities in which the manifestations of typhoid fever occur, we have found an exact correspondence with the distribution of the sympathetic nerve, as likewise between the amount of disease and the proportion of this kind of innervation in any given parts. Now it will be our object to examine carefully, in order to ascertain if there is any analogy between the character of these typhoidal phenomena, and those results which have been obtained by experiment upon this system of nerves. In this interesting department of physiological inquiry, there have been many engaged, but a few will answer very well the purposes of our comparison. As early as the year 1732, Pomfou du Petit found that the division of the trunk of the sympathetic, opposite the fourth or fifth cervical vertebra in dogs, was very rapidly followed by great disturbance in the circulation of the eyeball, producing inflammation, flattening of the cornea, retraction of the eyeball, with protrusion of a fold of the conjunctiva and a flow of tears, and ultimately ulceration and destruction of the organ. The experiments of Dupuy upon horses, wherein he extirpated the superior cervical ganglion, were followed by the same results with regard to the local effect in the eye, but also, with the more ap-
posite and corroborative consequences, that there was an eruption over the whole cutaneous surface, with emaciation and an oedematous state of the limbs. Dr. John Reid, has also experimented on the sympathetic nerve in the neck, and found the eye similarly affected with the above, the conjunctiva becoming red and congested in a few minutes, while in other experiments* the eye presented an ecchymosed or bloodshot appearance. Each one of these conditions of the eye must be borne in mind, in order to appreciate the comparison; inasmuch as, on account of the great difficulty of making such experiments on other portions of the sympathetic system, we can find none on record which will serve as reference; for it will readily appear that, from the remote position of these nerves, it is impossible to make their section without so materially deranging other important parts of the organism as to render the results valueless in deduction.

"Now, a reference to some of the pathological phenomena of typhoid fever, will discover a close analogy, if not identity to the above results; in the first place, the conjunctival congestion; its character, the attendant suffusion, together with the entire freedom from pain, even on exposure to the strongest light; while, at the same time none of the symptoms of true inflammation are present; all indicate the seat of the nervous derangement upon which it depends to be the ganglionic system† and not the cerebro-spinal, the analogous derangements of which are invariably of a sthenic character, and attended with acute pain in the region in which they occur. Again, an attentive consideration of the character of these congestions will show that they do not vary in any respect, except in degree, whether occurring in the mucous membrane of the eye, that of the stomach, pharynx, small intestine, large intestine, or bladder, in the typhoid type, or on the cutaneous surface in the typhus. In all the above localities, and under all circumstances, we find the capillary congestions wearing the same aspect, assuming invariably a passive character, often approaching the condition of true stasis, but never attended with the florid redness, the pain or the swelling of active inflammation. Lastly, in the cutaneous petechial eruptions or maculae of the typhous type of continued fever, we can also detect the same character of passive congestion from deficiency of nervous energy carried to a still greater degree; in this type, the nervous power of the cutaneous capillaries is so far diminished, that it amounts to a state of actual paralysis, allowing such distension of the capillaries that their rupture and a subcutaneous effusion is the result.

"We have thus far endeavored to show that typhoidal fevers result from alterations in the condition of the ganglionic nervous system, by comparing the typhoid phenomena with the normal action of the sympathetic system; and we have found that the analogy is com-

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* Arneman's Experiments on Nerves. Gottingen, 1787.
† Dr. A. Billing remarks, in relation to this subject: "Without, therefore, at present seeking for farther proofs, I deduce from blushing, and from the effects of electricity, fire, and cantharides, that the capillaries are dependent upon the nervous system for that tone or energy which preserves them from over-distension."—The First Principles of Medicine, p. 44.
plete, and that typhoid phenomena are but the result of aberrations in the normal action of these nerves. Their action may be either exaggerated or diminished; for instance, a portion of this system controls the action of the heart, and in health endows this organ with a frequency of action amounting to 60 beats per minute in the adult; and without disturbing causes, this number will continue unvaryingly in its regularity till the close of life. But we know that this regularity is liable to many interruptions and disturbances; some of them but momentary, as from the emotions; others disturb it for many hours by increasing its frequency, as in paroxysmal fever; and lastly, in typhoid fever, we find this increase of frequency kept up for many weeks, but still retaining the remarkable feature of continuousness, which distinguishes the normal action of the sympathetic system from that of the cerebro-spinal system, which is intermittent in all its phenomena, whether normal or abnormal. Were we now requested to explain the difference which marks the increased frequency of pulse in these three instances; to answer why is it evanescent in one case; of but a few hours’ duration in the second; and yet continue many weeks in the case of typhoid fever?—we think, we should do it thus: The heart, being under the dominion of the ganglionic system, performs its normal action through its influence; but from the intimate connection between the sympathetic and cerebro-spinal systems, especially in this organ, it is very liable to be affected by emotional causes acting in the brain; these, of course, will be evanescent; or it may be affected by causes acting in the spinal marrow, which may be more durable, as would be the case in a paroxysm of intermittent fever;* but it will be observed that, in these cases, the organic system is only secondarily implicated, and so soon as the mental emotion subsides, or the spinal irritation is removed or has exhausted itself in a paroxysm, the excitor being withdrawn, the sympathetic becomes again normal, and the action of the heart consequently natural. But in continued fever, the case is quite different; the irritation is now located in the ganglionic centre itself, which supplies the heart, and consequently the increased frequency continues as long as this irritation remains, which is coeval with the duration of the disease. There is one feature of continued fever, of which, heretofore, we have seen no satisfactory explanation, that we think can be accounted for rationally according to the pathology here suggested; we refer to the paroxysms and exacerbations which frequently complicate the course of continued fever. We would attribute these paroxysms to the extension of the irritation from the ganglionic to the cerebro-spinal centres, and we conceive that it is only under such circumstances that we find these intercurrent paroxysms masking the course of the typhoid affection. By this means there is effected a true blending of types from a

* We have adopted the pathology of intermittent fever advocated by Maillot, viz., that it consists in cerebro-spinal intermittent irritations. Prof. J. F. Lobstein remarks: “The paroxysms of intermittent fever are tied down to a regular rythmus, in consequence of being radicated in the nervous system, upon which nature has impressed a law according to which they must perform their functions periodically.”—Sympathetic Nerve, p. 121.
blending of proximate causes, and the two sets of phenomena exist in combination; a continued fever characterized by paroxysms of exacerbation. In the same manner, on the other hand, can we conceive of paroxysmal fever becoming continued under favorable circumstances, by the transmission of irritation from the cerebro-spinal to the ganglionic system.

"As the abnormal innervation in the sympathetic system can produce a continued accelerated action in the heart, in typhus and typhoid fever, so, likewise, can the diarrhœa of the typhoid type be shown to be a result probably of a similar condition. The normal action of the ganglionic system endows the intestinal canal and other viscera with the function of healthy secretion; but during the existence of the typhoid state this proper action is destroyed; in place of the organs being the seat of a normal and proper circulation, which is necessary for the due exercise of their function—secretion, the vessels become the seat of obstructions and congestions, the secretions become more abundant, but abnormal and vitiated; we have then diarrhœa. As these congestions become more marked, we find the paralyzed and over-distended vessels giving way, allowing submembranous effusions of blood, and occasionally considerable hemorrhage—all being the result of the altered condition of the innervation of these organs. Thus, what in its beginning was a purely dynamical affection, soon becomes organic; for without the proper supply of ganglionic nervous influence, we have seen, from the above-mentioned experiments, that the circulation ceases, the capillaries become turgescent, especially in highly vascular secretory organs (as the mucous surfaces, or the glandular plates of Peyer, for instance), effusion of lymph takes place, and, as above stated, finally, the capillaries are ruptured, and the tissues in certain places rapidly disintegrate by the process of ulceration—which is the actual condition of things in the intestinal disease of typhoid fever.

"From what has already been said in relation to the distinctive features of the two types of continued fever—typhoid and typhus, their interpretation, according to the pathology herein argued, will have been, doubtless ere this, naturally suggested. We have seen that typhoid fever is marked by an accelerated pulse, more or less nervous derangement, an altered condition of the blood, frequently a mild cutaneous eruption, diarrhœa, and meteorism. On post-mortem examination, we find lesions; viz. congestions and ulcerations of a peculiar character, that is, simulating those produced by experimental sections of the nerves in all those parts supplied by the visceral portion of the ganglionic system of nerves. On the other hand, we have seen that typhus fever is characterized by a somewhat more accelerated pulse, much more marked nervous derangement, the same altered condition of the blood, well-marked and always serious alterations in the capillary circulation of the skin, amounting often to actual ecchymosis, while the post-mortem examination shows almost entire exemption from lesion in the abdominal viscera. Thus, we find that while both diseases have all their general symptoms so exactly similar that
we are forced to acknowledge their identity, and see in them what is essentially but one disease, yet typhoid fever has its principal and most prominent manifestations in the abdominal viscera—internally; and typhus fever manifests itself in aberrations of the circulation, very analogous to those of typhoid, but occurring in the capillaries of the cutaneous surface.

“Now the ganglionic pathology is the only theory by which such marked incongruities in the two forms of what, to the observation and scrutiny of every one, must ever appear as one disease, can be perfectly and satisfactorily reconciled. We cannot deny that the two are but types of the same disease; yet how incongruous and strange it is that, in certain cases (typhoid), diarrhoea and intestinal lesion should appear the main—the most important features; while in the other cases (typhus), undeniably of the same disease, even more grave and threatening, there should be not a trace of diarrhoea, and on post-mortem examination no intestinal lesion whatever, but, instead, serious disease and congestion of the skin, with subcutaneous sanguineous effusions, similar to the submembranous sanguineous effusions of the typhoid type. Indeed, from the very smallest degree of attention must result the conclusion that, in the two cases, the disease is one and the same, but seated in different portions of the organism; and this conclusion will accord exactly with what we have considered the distinctive pathology of the two forms, viz. that in each type the disease is located in a different portion of the ganglionic system. There are certain parts of the system which are affected in both forms of the disease—as the ganglia supplying the heart; but after this, the two types have entirely distinct and separate sets of ganglia, the morbid action in which gives rise to their respective manifestations. In typhoid fever, the internal or visceral isolated ganglia, such as the semilunar, &c., are the seat of the morbid action; and these supplying mainly, we may say entirely, the abdominal viscera, and having but little connection with the other or external portions of the organism, these viscera become the seat of nearly all the morbid phenomena; while that little implication of the cutaneous surface and general nervous system which we often observe, is entirely due to the remote and very obscure connection which their isolated ganglia may have with the nerves supplying these parts.

“Now, in the typhus type, the same disease, or morbific agent (its exact nature we do not pretend to define), affects an entirely different set of nervous centres—a set of ganglia which, by their anatomical position, their internal and universal relations with the anterior and posterior roots of the spinal nerves, are plainly destined to preside over the capillary circulation of parts more superficial—the cutaneous surface. We have reference to the vertebral sympathetic ganglia; and, in attributing the location of typhus fever to these ganglia, we have a ready and satisfactory interpretation of all its distinctive characteristics. The skin becomes congested and ecchymosed (petechial) because its circulation is dependent upon and controlled by innervation derived from these vertebral ganglia; which ganglia being the
The seat of abnormal action (perhaps paralysis), innervation is deficient, the cutaneous circulation is retarded; in certain places there is obstruction, with actual rupture and effusion, giving rise to petechiae. The general (cerebro-spinal) system is more seriously involved in typhus than in typhoid fever, because the connection is more direct between the vertebral ganglia—which are the seat of typhus—and the cerebro spinal system. In a word, then, we would locate *typhoid fever* in the *visceral* portion of the ganglionic system, and *typhus* in the *vertebral* portion.

"We are fully aware that our views of the pathology of typhoidal fevers would be greatly corroborated, could there be discovered any appreciable lesion in the ganglionic nervous centres, in subjects who have died during their progress; but, like the pathological anatomy of all the nervous system, this would be an investigation attended with many difficulties; for histological changes in the nervous centres are of such a character that though they may be competent to subvert the intellect, entirely paralyze or destroy the functions of a large portion of the organism, and ultimately, upon the most positive rational evidence, seem to be the cause of death, yet on examination the alterations observable in those centres are of the most insignificant and irrelevant character, pertaining only to the involucrum, while the centres themselves, which, from the previous symptoms, had been plainly the true seat of the disease, have been found apparently normal and intact. These changes, then, are probably molecular and inappreciable with our present means of investigation, and will require years, and far more perfect appliances, to incorporate them among the positive and demonstrable things of our science. Still, there are occasionally isolated facts, even in the *pathology* of the ganglionic nervous centres, which we may refer to in corroboration of our opinion that these centres are affected in typhoidal fevers, and that such affection gives rise to its characteristic symptoms, or to phenomena quite analogous in their nature. The case reported by Prof. Lobstein is of this nature. It was that of a young girl who had suffered from paralysis of the lower extremities for some time, but for three months previous to her death labored under the most incurable *diarrhoea* with tormina, &c. On making a *post-mortem* examination, there was found a large abscess on the left side, extending from the sixth to the tenth dorsal vertebra. On opening this abscess, it was found that the *trunk* of the left sympathetic nerve was entirely destroyed from the sixth to the twelfth vertebra, and in the *lumbar* portion the same nerve was in a state of inflammation.*

There are also two cases reported as occurring in the practice of Dr. Aronshon, of the Strasburg Hospital: The first, that of a man forty-seven years old, affected with *diarrhoea*. He died of a tumour in the abdomen, and it was found that "the semilunar ganglia were affected with distinct inflammation." The second case was a woman aged about thirty years, who, in her second pregnancy, was subjected to vomiting throughout the whole of her gestation. She was also

* Structure, Functions, and Diseases of the Sympathetic Nerve, p. 147.
affected with a furfuraceous eruption upon the breast and arms, with swelling of the limbs, and with diarrhoea. On a post-mortem examination, the villous coat of the stomach was found inflamed and thicker than usual, especially towards the pylorus, 'and the semi-lunar ganglia were found in a state of genuine inflammation.'

"'In the body of a boy ten years of age,' says Lobstein, 'who had died from the retrocession of a military eruption, attended with symptoms of great anxiety, oppression of the chest, and distension of the epigastrium. I found a place in the left trunk of the intercostal (which is the old name for sympathetic) nerve, highly inflamed between the eighth and tenth ribs, with a phlogosis of the ninth and tenth thoracic ganglia, and their two anastomotic branches with the costal nerves.'

"The following observations are quite pertinent to the state of congestion in which the lungs are almost invariably found, to a more or less degree, in typhoid and typhus fever. 'On examining into the condition of the nerves in diseases of the lungs, I discovered another alteration which is peculiar to these organs; to wit, in that species of peripneumony, in which the lungs become red and slightly indurated,* and which, I think, should be called spleenification; the nervous filaments attending the ramifications of the bronchia were found equally red, a little more tumid, but much more tender than usual; so as to be broken by the slightest degree of force.'† And, lastly, the same author‡ quotes a case still more in point, from the writings or Professor Autenrieth, of Tubingen, wherein it is asserted, though he does not seem to connect the circumstance with the pathology of the disease at all, that he has seen the abdominal nerves of the ganglionic system somewhat changed in subjects who have died of typhoid fever.

"The above cases, though not conclusive, are at least strongly corroborative of our view of the pathology of typhoid fever; for while we must admit that the diarrhoea, the eruption, the pulmonary congestion, oedema, &c., might have been produced by other causes than disease of the ganglia found inflamed or divided, still, when these coincidental circumstances are viewed in connection with the known result of artificial section of accessible portions of the sympathetic nerve, as those about the neck and eye,§ and also in view of the entire dearth of experiments and facts bearing upon this portion of the ganglionic system, we must regard them as significant and valuable, even though they afford what we may term only a legitimate suspicion of the correctness of our pathological views.

"We have now completed our investigations in relation to this intricate, but, at the same time, most interesting topic of pathological inquiry; we have reviewed its history, and collected from every source within our reach as complete a delineation of its prominent and characteristic phenomena as has been necessary for their full development.

* Exactly what is described by Dr. Flint as the pseudo-pneumotis of continued fever.
§ See experiments of Panfour du Petit, Dupuy, John Reid, and others, already referred to.
In so doing, we have been struck with the vast number of reliable and significant facts our science is in possession of, in regard to this disease. No disease in the catalogue is more invariable in its characteristic manifestations; no disease has been more diligently studied, or has enlisted in its investigation such faithful observers. Our knowledge in regard to its observed phenomena and facts is clear, well defined, almost certain; to complete the science in regard to it, it has but remained that these cognate, well-ascertained facts and phenomena be rationally and correctly interpreted, that its true pathology might be deduced. To this arduous, though not unpleasing task, not without many misgivings, we have earnestly and diligently devoted ourselves, more with the hope that our labors would prove suggestive to others of the true mode of arriving at the real pathology, than that we should be able to supply the want or remedy the deficiency.

"Starting with what we considered the rational assumption that the pathology of typhoidal fevers is in the ganglionic system of nerves, we have compared their characteristic traits and phenomena, with first, the normal action of this portion of the nervous system, then with the known and well-established results of experimental irritation and action of various portions of these nerves, and we have found that the analogy is sufficiently close to admit the legitimate inference that the symptoms and pathological lesions of typhoid and typhus fever are produced by abnormal action in certain portions of this system of nerves. First, because no typhoid or typhus phenomenon ever occurs, except in regions supplied by this system; secondly, because the peculiar phenomena of these diseases occur in a more marked degree, in those parts most abundantly supplied from this source; and thirdly, because the nature of these symptoms is always found more purely and characteristically typhoid in those portions of the organism supplied exclusively by this kind of innervation. And farther, on the other hand, we are forced to admit the truth of these impressions, because we have hitherto had no theory or legitimate and consonant combination of theories, to our own mind, as competent to the full and rational explanation of all the phenomena of the disease as the one now offered.

"Practical Deductions.—Pathology is only valuable when it has a tendency to the prevention, amelioration, or cure of disease, and the results of our most successful labours in this department are but nugatory unless in them can be found a clue to a more rational and perfect management of the affections to which they refer.

"If the views embodied in the foregoing treatise be correct, the following practical inferences must present themselves as legitimate, if not inevitable: First, that in the management of this class of fevers the strictest attention should be paid to the improvement of the tone of the nervous system; all depressing measures, or such as are calculated to exhaust the nervous energies, as depletion by purgation or otherwise, should be scrupulously avoided; and secondly, that in their place means of an opposite character should be invoked. Indeed,
that treatment now most in favour, though but empirically* applied, will be found on the admission of the above pathology the most rational, and to offer the best hope of success. We have reference to that treatment which is addressed almost exclusively to the nervous system, and has for its object the sustentation and elevation of its energies. Some of the means employed have been attended in their application with the most marked beneficial results. Among these, we would mention the plan of Dr. Percival, wherein the disease was treated by frequent profuse cold affusions, especially in the case of children; which treatment we should rationally expect, from the known effect of cold water thus applied, to improve the condition of the depressed nervous centres. The administration of stimulants, as camphor, quinia, brandy, opium, turpentine, and the ethers, have all been favourably known to the profession as remarkably efficient in these fevers.

"In relation to the beneficial results recently obtained from large doses of quinia, by Dr. Dundas and others, we can readily appreciate what vast benefit may accrue from them, especially in cases where the cerebro-spinal system of nerves are extensively implicated, and where the disease is marked by regular obstinate paroxysms, for quinia, though it possesses, in our opinion, but little influence over the ganglionic system itself, still, would relieve these periodic exacerbations by its effects upon this system, through the cerebro-spinal nerves (which in these cases we regard as the instigator of the paroxysm), upon which most of its power is expended. That the doses required should be large, we can easily understand, as any effect produced upon the ganglionic system through the cerebro-spinal is only accomplished by powerful and long-continued impressions, on account of the comparative isolation of the two systems from each other.

"To those who are in the proper field for such experiments, and possessed of the proper facilities, we would recommend the trial, in typhoid and typhus fevers, of such agents as are known to possess a direct power to stimulate the nervous system, even when in a state of partial paralysis—such an agent is strychnia. This we would suggest as applicable in minute but efficient doses, with the view of waking up and restoring the diminished energy of innervation, upon which the impaired function depends, in the same manner that we would advise it in other similar cases where the cerebro-spinal system was implicated.

"The above is all we offer in regard to treatment; there are many measures of a like nature, which, were we writing a complete treatise on the management of these diseases, would require a full and extended consideration, as also the measures and applications which the emergency of each case will naturally suggest and demand."

* Of course, this term is not applied in its offensive sense.
Transactions of the Fourth Annual Meeting of the Medical Society of
the State of Georgia, held in the city of Savannah, April, 1853.

Not having been able to complete, in our last number, the notice of
the Transactions of the Medical Society of the State of Georgia, we
now resume it.

At page 65 we find a communication from Dr. Thos. W. Bell, of
Houston county, upon the use of the Sulphate of Cinchonia as a sub-
stitute for Quinine. The author states that he has used it in eleven
cases of intermittent fever with success, but has not tried it in remit-
tents.

"Its effects resemble very much those of quinine. When given in
large doses, it produces cerebral disorder, such as fullness of the
head with ringing in the ears. I do not know that in full doses, it
possesses the sedative properties of quinine.

"I first tried it, hoping to find a remedy more efficacious in the
treatment of the quotidian form of intermittents, which is so prone to
recur time and again, but I find that relapses are about as frequent as
after the use of quinine, or other remedies that I have tried.

"I gave about the same quantity in the intervals of the paroxysms,
that I usually give of quinine ; from 18 to 24 grs.

"Dr. Pepper gave 16 grs., and I have no evidence to prove that
this is not a sufficient quantity, even in this latitude.

"From this limited experience, I am induced to believe its anti-
periodic virtues are not much, if at all inferior to quinine.

"I hope other members of the profession, who live where autumnal
periodics are as numerous as with us, will test its virtue. If it
should prove as successful generally, as with Dr. Pepper, and with
myself, we will have a remedy, for the cure of periodics, at a little
over one-third the present cost of the sulph. of quinine."

From the biographical sketch of the late Dr. Wm. R. Waring, of
Savannah, by Dr. C. W. West, we cull the following characteristics
of the subject of the memoir:

"Like most of our professional men, Dr. Waring was too much
engaged with the labors of an active exercise of his profession to
write much, but the few articles from his pen would alone establish
the traits of his mind. One of them, an excellent description of
disease, clear, minute, and searching; the others entirely argumen-
tative. His report to the city council on the epidemic, yellow fever,
of 1820, contains much valuable information, though unfortunately,
his mind was too deeply imbued with the pathological views of the
French school to arrive at correct conclusions. Broussais had just
broken the bonds of the humoral pathologists, and finding the origin
of many continued and remittent fevers to consist in a diseased state
of the gastro-intestinal mucous membrane, led away into an equally
grave error, many of the medical profession of that day. Hence Dr.
Waring's error in fixing the pathology of yellow fever upon the diseased mucous coat of the stomach, and attributing all the fevers of our climate to the same cause. But when we remember the vagueness of the pathology of fever previously held, and the nearer approach to truth, confirmed by a more successful mode of treatment, which resulted from the change, we look with leniency upon an ardent reception of a theory, false in fact, and yet productive of so much good. Under the more enlightened pathology of the present day, no man would be excusable for holding the doctrines of Broussais, though at that time it was rather evidence of a mind keeping pace with the developments of science. Such strong impressions are not easily dismissed, and Dr. Waring continued to practice upon the theory of his illustrious teacher even after his dogmas had been exploded.

"Dr. Waring's mind was strictly philosophical; trained to reason closely and logically, he dealt principally with facts and their legitimate deductions. Hence he was deliberate, and even slow, in his diagnosis, but generally clear and positive, firm and intelligent in treatment, and very generally successful. If he formed theories, it was only after study and reflection, and they were the results of judgment, not of fancy. A mind thus acting must necessarily be acquisitive—he was a constant and unremitting student, not deterred even by the labor of an extensive practice. The midnight lamp was not a figure of speech to him, but a reality, which brought him constantly in close converse with the great minds of the age. Nor until disease had enfeebled him, did he relax from assiduous study.

"With little imagination or fancy, he was rather taciturn, and seemed indisposed to communicate, but when in the confidence of social life, he could be drawn out to unburden the accumulated stores of his intellect, they were found to be rich treasures, which instructed and delighted all about him.

"Tubercular phthisis, with its unerring certainty of march, gradually wasted him away, and he died in this city in the year 1842."

Dr. C. B. Nottingham also furnishes us an excellent biographical sketch of the late Dr. Ambrose Baber, of Macon.

"As a practitioner of medicine, Dr. Baber was prompt, decisive and energetic. His practice sharing largely of the anticipating method of cure; the temporising expedients of the expectant system of the distinguished French pathologist which led captive in those days many enlightened intellects in medicine, found little favor in his views. Clear and perspicuous in diagnosis, he no sooner arrived at a satisfactory conclusion as to the pathology of a case than he was prepared to invoke, from his ample store of therapeutic knowledge, such agents as were best calculated to meet the indications of treatment. He was however a prudent and even cautious man. Daring heroism or reckless boldness could not claim him as a votary. Although highly gifted in powers of observation and deduction, and consequently a ready diagnosticator—his tact, sagacity and quick
perception enabling him to learn more of a case ordinarily by a hurried glance, slight manipulation, and a few questions apparently carelessly propounded, than many men could by protracted investigation; yet he met with *many* cases in a large practice, which, in his judgment, partook of the character of abstruse problems—requiring for their solution, the most systematic investigation and careful reflection before therapeutic measures were instituted. In cases of such obscurity he ever made it a matter of scrupulous regard to truth, the interests of the sufferer and his own reputation, to pursue a course of calm, deliberate and cautious enquiry—bringing to the task scrutinising examination, patient analysis, and a general concentration of effort, that was sure in the end to result in views of clear and definite elucidation—alike creditable to himself and conducive to the welfare of his patient. Not unfrequently was it the case that he would visit a patient, the pathology of whose disease was not clearly manifest a second, and sometimes even a third time, before declaring his diagnosis or suggesting a prescription; permitting neither the anxiety of the afflicted, the personal labor or the apparent want of skill, which such a course might suggest to observers, to swerve him from his conscientious sense of duty and probity—an example worthy of imitation by those who consider themselves able by intuition to solve the hidden mysteries of the dark arcana of disease and arrogate to themselves the capacity of instituting the best means of cure without ever evincing any show of hesitation.

"In his intercourse with his patients, Dr. Baber did not indulge affability. On the contrary, he was usually blunt, stern and sometimes even austere—rarely unveiling to any the secret workings of the mind by which he wrought his diagnosis or determined upon the plan of treatment, and never tolerating the slightest suggestion or contradiction in any way affecting his opinions or measures. Being perfectly satisfied with his elucidation of the case, and having a definite object in view in making his therapeutic appliances, he moved with a firm and steady step, uninfluenced alike by the whims and caprices of the patient or the fears and apprehensions of friends. Dissatisfaction expressed or plainly implicated was but the signal for his withdrawal from the case—thereby giving the family an opportunity of seeking aid elsewhere. Let it not be inferred, however, that he was either unacquainted with the politeness and courtesies of the gentleman, or a stranger to the benignities of the profession. Dignified, urbane and benevolent, he yielded precedence to no one in the true sympathies of the man or the charities and kindly offices of the profession; but pursuing his labors with a due sense of their obligations, their responsibilities and their true dignity, he considered the non-professional, even among the more intelligent, incompetent to appreciate the pathological and therapeutic ratiocination of his mind, and that his own self respect and cherished reputation was always more or less involved in a faithful adherence to his well considered and well matured directions.

"Dr. Baber's bearing among his brethren of the profession, was
equally as dignified and in some respects as reserved as among his patients—having but little intercourse with any, except those whose intellect and acquirements, in his judgment, reflected honor upon the profession, and consequently commanded his respect. With such, at fitting times, he would unbend himself to free and even familiar intercourse. On such occasions his manners were bland, his conversation graceful and his whole bearing that of the educated physician and cultivated gentleman. With drivellers and pretenders—those who hang as an incubus upon the profession—courting popular favor by dissimulation and trickery, or by calumniating the fair character of their superiors—deriving honor by their connection with the profession without making any return—those drones and ignoramuses who look to the profession as a foster-mother—giving them some show of respectability and position, yet who do nothing to add to or support its dignity, its honor, its usefulness, but on the contrary whose daily life is one continual slander upon its purifying and elevating influences—with such he never associated, and when opportunity presented, rarely failed to impress them with a full consciousness of the ineffable contempt with which he viewed their unmanly and dishonorable conduct.

"For the junior members of promise, he cherished feelings of the most kindly and considerate regard. Cheerfully aiding them in the many ways in which the senior practitioner has it in his power to advance their views and forward their preferment, he was esteemed by the rising young men of the profession as the patron of merit.

"In consultation, he was ever a man of mark—expressing himself courteously, yet firmly, deliberately and fully—listening respectfully to the opinions of others, yet insisting boldly and zealously upon the correctness of his own, he exercised a power and controlling influence in the consulting room that rarely failed to make itself felt in the management of the case. Harmony was never so much the end sought, as the means of securing the welfare of the patient and the triumph of truth.

"Important elements in his character, those having much to do with his success, were energy, industry, decision, and firm determination of purpose. Viewed in relation to his profession, they made him diligent, faithful and untiring in whatever he undertook. Devoting himself with a spirit of self-sacrifice upon the altar of duty, he was ever ready to respond to the call of suffering, and when in the chamber of affliction and peril, was scrupulously studious to leave nothing undone that might conduce to the interests or properly to the comfort of the sufferers. Neither, considerations of personal comfort or bodily pain, the fatigue of sleepless nights or incessant toil, were permitted to interfere with the full discharge of his sacred obligations, to those who sought relief at his hands.

"With an elevated ambition of excellence, a refined pride of character, an honorable, independent and chivalrous deportment, and an integrity of unspotted purity, was associated in his later years, the amenities and graces of the christian. Generous to a fault among
his friends, he was benevolent to the poor and liberal in his offerings to the various enterprises designed for the moral and social improvement of his race. Pursuing his profession under circumstances of unrivaled popularity that enabled him to command a full practice within a circle bounded by the selfish and sordid horizon of pecuniary emolument, he looked beyond the reward of the hour, and freely ministered its high charities wherever a claim presented for their exercises—responding, alike, promptly to the call of the widow, the orphan and the virtuous desolute of the sterner sex, as to that of those upon whom fortune smiled in the gifts of ease and affluence.

"Such was Dr. Baber. With a character distinguished by such ennobling traits, and a life illustrated by such deeds of usefulness, grace and charity, he could not fail to command the respect, confidence, admiration and patronage of the public who knew him."

The last paper in the work before us is the Address delivered by Dr. Juriah Harriss, of Augusta. It is a chaste and erudite production highly creditable to the author. We regret that our limits will allow us to reproduce only the opening and closing remarks of the speaker.

"'History is philosophy teaching by example,' and the great source of human knowledge. Embodying the past and the present, she will enable us to avail ourselves of the one as well as the other.

"Analogy is the first and most simple process of inductive reasoning, and if properly carried out, leads to positive and invariable results. If the data are ascertained, and the reasoning correct, the conclusion must inevitably be true. From history, we are enabled to collect the data and approve the truths, or demonstrate the errors in the reasonings or conclusions of our predecessors. We must of necessity profit by the experience of others, since life is too short for one individual to pass from the origin of the sciences to their present perfection without its aid. Difficult indeed would it be for us to acquire new facts, or make new discoveries, if we were unacquainted with the truths that have been determined by those who have preceded us. It is the records of these facts which benefit mankind, for otherwise how comparatively vain and profitless they would prove.

"But aside from facts; it is from a multiplication of theories, and theories oftentimes adverse in character, the offspring of individual minds, that we must generalize and reach primitive causes of natural phenomena.

"The history of medicine is its veritable flame of truth, and the most fruitful source of instruction, and he who is unmindful of its promptings, however intellectual, or however persevering in his labors, will never add very much to science. His labors may be incessant, but the field is too extensive to be traversed by him; he may be gifted in mind, but the subject is too vast and abstruse to be grasped by his unaided intellect. But this field must be cultivated, even to its utmost limit, which can alone be accomplished by toil—toil the most un-
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remitting—and when the energies are exhausted, the benevolent searcher after truth will record the result of his researches, as landmarks for those who come after him, and who will be thus enabled to profit by his experience. It is only by such means that science secures permanent progress, and mankind real benefits.

"But history has other advantages than that of giving us the experience of the past. It prevents us from coming to unjust or unfair conclusions, and its records prove that an impartial historian may find important truths, wrapped in the mysteries of conflicting opinions. Its study tends to divest us of our self-importance and egotistical pretensions. It inspires us with modesty, and demonstrates that a blind confidence, or pertinacious adherence to our own opinions indicates weakness. It disciplines the mind, enlarges the views, and refines the taste. 'The cold study of the scholastic, and the false philosophy of Talmud, can offer interest to no one save the true historian, who, in the midst of the greatest confusion, can unravel the mysteries, and draw forth a few sparks of light.'"

The speaker then proceeds in a very happy manner to establish a comparison between the state of medicine in the 18th and 19th centuries, and concludes thus:

"It is the glory and triumph of medicine, that she has been placed upon the immutable foundation of truth. For centuries she has been struggling for that honor, but until the advent of the era in which we live, she has been ranked as a mere art. Her disciples have nature and truth for their guide—their object, the amelioration of the sufferings of mankind. Up to this century she has had to contend with ignorance, overcome prejudice, resist contumely and ridicule, and fight against quackery and pretending artisans.

"It is at length and happily, the proud province of the scientific physician, to stand upon the high eminence of science, surrounded by an atmosphere of truth, and crowned with imperishable laurels, wreathed by the gratitude of the world. The remark of the illustrious Roman orator, 'homines ad deos nullâ re proprius accidunt, quam saltem hominibus dando,' is appropriate only to physicians of this century. He does not stalk forth as of yore, the unblushing charlatan, the unscrupulous and heartless quack, the boasting pretender, but walks forth conscious of his own skill and rectitude—a dispenser of health."

Singular Atmospheric Phenomenon.—The subject of quarantine regulations is being agitated in Louisiana, and a circular has been addressed to members of the Medical Profession in that state for the purpose of eliciting their views with regard to the origin and propagation of Yellow Fever. We have just read the reply of Dr. James S. McFarlane, who is a strong believer in the local origin of the disease, and in its non-contagousness, and who is consequently decidedly opposed to
quarantine regulations. We have never believed that yellow fever is contagious, or, in other words that it can be communicated from man to man; but we can see no good reason why an atmospheric disease or poison may not be carried about in a ship, and discharged upon a wharf with the cargo, and thus infect the atmosphere to such a degree that those who breathe it may take the disease. Nor do we think it unreasonable to admit that the poison may be propagated or increased in quantity, if thus deposited in a susceptible atmosphere, whereas it would prove inert if turned out in an atmosphere less congenial to its vitality, if we may so express ourselves. The history of the late epidemic would seem to establish almost beyond a doubt that the yellow fever was carried along the coast of the Mississippi by the steamers, however successfully it may be demonstrated that it was not brought to New Orleans from South America or the West Indies.

The following singular atmospheric phenomenon is related by Dr. McFarlane in his able article:

"Among many local phenomena which occurred, and which could have no relation to contagion or importation, and which may to a certain extent, account for the presence of an epidemic, were the following:

"A piece of fresh meat attached to a kite, and elevated a few hundred feet, came down in twenty or thirty minutes, completely covered with living, moving vermiciform animalculae, and this circumstance occurred throughout the whole epidemic, whenever the experiment was made, which was almost daily.

"The upper stratum of clouds never changed their position from the commencement to the close of the epidemic: they appeared "nailed to the sky," and this extraordinary phenomenon was observed in the interior as well as in the city; but what is most remarkable in the premises is the fact that on the very day that they were observed to move, for the first time in several months, the epidemic began to decline, and continued to do so until it ceased to exist, without the aid of cold, frosts or tempests.

"If these phenomena be corroborated by more extended experiments, in future epidemics, they may serve in part to account for the existence of a yellow fever epidemic, without any reference to contagion or importation.

"They demonstrate a horribly polluted condition of our local atmosphere, and show that that atmosphere was permanently established around us throughout the whole epidemic."

New process for coating Pills.—M. Calloud, (de Chambéry) in Journal de Pharmacie, xxiii. 301, treats of the subject of enveloping medicinal substances in a covering to prevent unpleasant taste. After having tried gum, starch and sugar without satisfaction, owing to the
hygroscopic tendency of the sugar and gum in moist air or with a moist mass, and their tendency to crack when very dry, he had recourse to the dried mucilage of flax-seed prepared with sugar, with success.

The following is his method:

Take of Flaxseed, one part.
White sugar, three parts.
Spring water, a sufficient quantity.

A thick mucilage is obtained by carefully boiling the seeds, the sugar is added, and the whole of the moisture evaporated by careful desiccation.

This mixture is but very slightly hygroscopic, may be reduced to fine powder, and employed for covering pills. This operation is effected extemporaneously with great facility. The pills slightly moistened, are rolled in the mucilaginous powder, by which they are coated with a layer of the compound.

M. Calloud has used this chiefly for carbonate of iron pills, but it may be applied to other kinds.

Garot's process of coating pills with gelatine is most applicable to disagreeably odorous substances, as assafetida, castor, valerian, etc., which are completely masked by it. When the gelatine is previously colored with carmine the pills bore the appearance of hawthorn berries.

M. Calloud suggests another process applicable in certain cases, which is the use of the butter of cacao as a covering for pills, where, owing to gastric irritation, the unmasked pills will cause disagreeable symptoms. The process is very simple: The prepared pills are thrown into melted butter of cacao, then removed with a perforated skimmer, and finally rolled in finely powdered sugar, or what is better, sugar of milk.—[American Journal of Pharmacy.

New Apparatus for extracting Drugs.—M. Schwaerzler, (Gazette d'Augsbourg, April 23, 1853,) has stated that if a flask is three fourths filled with water, and then closed with an air-tight cork through which passes a tube reaching to the bottom of the flask, and the latter is plunged into boiling water, it is well known that the dilatation of the enclosed air will force the liquid out through the tube. If a funnel-shaped vessel is attached to the top of the tube securely by a soft cork, the fluid will be driven up into it, and a portion of air will escape through the tube. If now the flask is lifted out of the water bath, the air in it contracts, and the water in the upper vessel returns to the flask. Taking advantage of this idea, an anonymous correspondent of the Journal de Pharmacie, (tome xxiv. p. 134, 3e serie,) has suggested a lixiviating apparatus which consists of a flask, a tubulated bell-glass inverted, and a suitable tube connecting them in the manner described. A diaphragm of perforated tin is placed within the bell-glass, and upon this the substance to be extracted is loosely put. The flask is now placed in a vessel of boiling water; the water in the flask soon commences to rise in the bell-glass until it has covered the ingredients. After contact a sufficient time, by lifting
up the apparatus from the water bath, the fluid retreats to the flask, and carries with it a part of the soluble matter of the substance. This is repeated several times until the substance is sufficiently exhausted.

We have tried this experiment with a Florence flask and an inverted bottle with the bottom removed, and find it operates very well. The writer suggests that it is equally applicable to extraction with alcohol and ether, avoiding the point of ebullition, providing the upper vessel with a cover, and, in the case of ether a condensing apparatus, to avoid loss. In our small experiment, the temperature of the liquid in the upper vessel, when the air commenced to escape, was about 180° F.—[Ibid.

Cause and Treatment of Prolapsus of the Rectum. By M. Duchausay.—In a short but interesting memoir, M. Duchaussay reviews the circumstances attending this troublesome complaint, and fixes attention in particular upon the loss of power in the sphincter ani muscle as the chief cause of the descent of the bowel. Moreover, he endeavours to show that Dupuytren’s operation, by excising the radiating folds of skin around the anus, and the operation by four touches with the actual cautery, practised by Guersant, act not by causing any subsequent retraction of the cellular tissue, skin, and mucous membrane, but rather by stimulating the sphincter muscle, so that it regains its contractility, and therefore its retentive character. How else, asks M. Duchaussay, do we explain the fact, that the prolapsus is often cured, or does not return after two days, or even after one day, or not at all, after the operation? He points out the fact, that in cases of this disease in infants, three fingers may sometimes be introduced without causing contraction of the sphincter, before the operation by cautery, whilst afterwards, if one be passed, a powerful contraction of the sphincter immediately ensues. As proof that this recovery of contractile power by the sphincter is the cause of cure, a case is mentioned in which M. Guersant had used the cautery too superficially, the sphincter failed to contract, and the disease returned. A second cauterization was followed, on the contrary, by return of the muscular contractility, and the cure was complete.

According to the author, the cautery acts as a stimulant to the paralyzed muscle, just as it will to the deltoid in a like condition. After pointing out the inconveniences and apparent severity of M. Guersant’s method, M. Duchaussay suggests that a slighter cautery, or some other stimulant to muscular contractility, might act as well, and he suggests strychnine. This, with M. Guersant’s permission, has been tried in the Hôpital des Enfants, in the case of a girl aged eleven years. The prolapsus here arose from obstinate constipation; it had lasted for four years; the bowel protruded at each evacuation about ten centimeters (=4 inches). During the first month of her admission she was treated by laxatives only, with no other result than that of diminishing the length of the protruded portion of bowel to about four centimeters (1½ inches). Strychnia was then employed endernically near the region of the sphincter; the next day there was
no evacuation; on the following day the bowels acted once, only a slight bulging of the rectum taking place; on the third day the protrusion was still less after an ordinary evacuation; and during the next thirteen days it did not occur again.

Blisters were made in the cleft between the nates, and on the right thigh close to that cleft; one-sixth of a grain of strychnia was applied the first day, one-third on the second, and one-third on the fourth day. On the fifth day, about half a grain of sulphate of strychnia was used, and this was repeated for the last time on the sixth day. In the case of a boy, it is recommended to be applied between the scrotum and anus, immediately over the anterior interlacement of the sphincter ani fibres. The remedy certainly deserves further trial.—[Archives Gén. de Méd. Brit. and For. Medico-Chir. Rev.

Infantile Leucorrhœa.—In several numbers of the 'Medical Times and Gazette,' for Sept. 10th to Oct. 29th, will be found the History of a recent Epidemic of Infantile Leucorrhœa, with an account of Five Cases of alleged Felonious Assaults. By W. R. Wilde, F. R. C. S., &c.—"Considerable excitement." Mr. Wilde states, "has prevailed among all classes in Dublin during the last month, owing to the circumstance of no less than three cases of felonious assaults upon children under ten years of age having been brought forward by the Crown at the late Commission before the Chief Justice."

A correspondence has been published in the 'Freeman's Journal,' between Dr. Ireland, Physician to the Police, upon whose information the cases were sent for trial, and Mr. Wilde, who had one of the accused persons defended. Most of the leading members of the profession in Dublin gratuitously tendered their evidence in court, "in what they considered the cause of truth, science, and humanity." The occurrence of this form of vaginitis, in an epidemic form, as shown by Mr. Wilde, is perfectly well known to most practical physicians and surgeons.

Mr. Wilde notices the delusion, which is extensively prevalent in Ireland, to the effect that a man can get rid of an obstinate gonorrhœa by having connexion with a virgin; and as the easiest and surest mode of effecting that, a child of tender years is selected; hence the felonious assaults occasionally attempted, and for which men have been most justly convicted, and most righteously punished. But in all such cases it has been proved, that the men, after the commission of the crime, still laboured under gonorrhœa or venereal, although the popular opinion among the lower orders is, that the disease is not only completely, but instantaneously, transmitted from the male to the female. A similar superstition, it appears, is found to exist among the Arabs, as stated by Mr. Duchesne, in his recent work on 'Prostitution in Algeria.' It is the knowledge of this wide-spread superstition, which leads the mother at once, on the appearance of vaginal discharge, to jump to the conclusion that impure connexion has taken place, and possibly she may be confirmed in this idea by some medical men not conversant with the true nature of the affection.
Mr. Wilde dwells upon the suggestions, insinuations, and threatenings, which are usually had recourse to in these cases, in order to extort a confession of connexion, and very justly remarks that it is not likely that a child, who has neither passion nor love to influence her, will conceal the fact from her parents or near relatives, when hard pressed. In Mr. Wilde's contribution it is stated that one mother, on her own statement before the jury, actually threatened to cut her child's tongue out if she did not confess to the connexion. Besides the three cases already referred to as having become the subjects of trial at the late Dublin Commission, Mr. Wilde cites six other instances of epidemic leucorrhoea that have been brought under the notice of the profession, either at public institutions, or through the medium of the medical journals. For the details of these cases, and of the trials, we must refer our readers to the 'Medical Times and Gazette.'—[Ibid.]

Monomania—Book-stealing.—An Englishman in Paris having been convicted of stealing books from a stall, and condemned to two years' imprisonment, pleaded monomania, or an irresistible impulse, as ground of mitigation of sentence. From the previous history, however, of the culprit, it was established that he must be held responsible for his acts; the plea was therefore negatived.—[Annales Medico-Psychologique, and Ibid.

A Case Book, to be used at the Bed-side. By G. F. Cooper, M. D., of Americus, Ga.

We have been favored with a copy of Dr. Cooper's case-book, and take pleasure in recommending it to the profession as admirably adapted to the purpose for which it is intended. With such a help practitioners can without difficulty keep an excellent epitome of their cases for subsequent reference. We hope that a supply of the work will be forwarded to our booksellers.

Husband's Isinglass Adhesive Plaster.—We acknowledge the reception of a specimen of the above article, which we have tried and find very excellent. We therefore cheerfully recommend it to the profession.

The State Medical Society.—The fifth annual meeting of the Medical Society of the State of Georgia, will be held in the city of Macon, on the Second Wednesday (12th) in April next.

D. C. O'Keeffe, Rec. Secretary.

Greensboro', Ga., March 1st, 1854.

Errata.—In January number, page 11, third line from top, for "complete," read complex. Page 13, tenth line from bottom, for "sustained," read retained. Page 17, twelfth line from top, for "individual," read undivided.