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ARTICLE XIX.

Veratrum Viride in Chorea and other Convulsive Diseases. By
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QUININE in its early history, was known as an agent only valuable in paroxysmal fevers; in these latter days, of a more advanced Pathology and enlightened Therapeutics, its application has been so far extended, that in Southern latitudes especially, it has become almost equally a specific in many affections, not at all characterized by febrile action. Somewhat analogous, I believe, will be the history of *Veratrum Viride*.

The chief object of this communication, therefore, is not to set forth the value of this agent as a controller of arterial and vascular excitement; for, it is believed that this power is now recognised and conceded to it generally, by the medical Profession. Indeed, a consideration of this property would not be here entered upon at all, except for the reason, that the writer's attention has been recently attracted by an article, in which it was declared that "*Veratrum Viride* had seen its day," that its glory had departed!

Now, if this communication succeeds in recalling one wanderer from the path of truth, or induces one skeptic fairly to test the fact he disbelieves, then, will the writer be amply rewarded. His only object is the advancement of his profession, by de-

fending and sustaining the truths that render it a useful and glorious science.

Those who deny the virtues of the *Veratrum*, object to it, as they say, because, though it reduces the heart's action, it does not cure disease; and moreover, it is a dangerous remedy, which cannot be entrusted to nurses in general.

To the first of these objections, viz: that though the *Veratrum* controls the heart's action, it does not cure disease, it may be replied, that no Doctor nor remedy ever cured a disease; and that the only province of the one, or power of the other, is to lend assistance to nature, for it is she at last, who alone can work the cure; this is plain when it is remembered that disease is only perverted healthy action. Moreover, when one reflects and understands what an inflammation is, and properly comprehends its commencement, progress, and results, having a clear perception of the local changes, wrought by it, (which may be called the mechanical difficulty,) and an adequate idea of the necessarily irritable and excited state of the nervous system during its progress—it is indeed wonderful that he could throw aside, untested and uncared for, an agent which it is acknowledged has power to control the circulation; for, always let it be remembered and never forgotten, that *Veratrum Viride* controls the circulation by its sedative influence upon those nerves, whose “aberrated action” forces the heart and arteries into such an unnatural and dangerous turmoil; and this quieting of the great force-pump of the circulation, prevents the rushing current from impinging with destructive force upon the inflamed and suffering organ, and from too rapidly crowding globule upon globule, upon its already congested and stagnant capillaries, and thus, by affording what may be considered a mechanical assistance, gives the sanative powers of nature time and opportunity to remove the obstruction and work out the cure. Let it also, not be forgotten, that all the while the hitherto excited and irritated nervous system, is primarily calmed into normal quiet, and that consequently, all the workings of the organism, as they are under its “superintending influence,” must move on in their natural course; thus is it, that the equilibrium is restored, which ensures that harmonious order which characterizes and makes perfect, the grand and beautiful works of God.

Thus is it, that the *modus operandi* of *Veratrum* in inflammatory diseases, accomplishes all, and more, than can be claimed for the lancet. Venesection is alone beneficial in such cases, in two ways; first mechanically, by lessening and weakening the force of the circulating current, and thereby guarding against the dangers of its too frequent and forcible impulse upon the seat of inflammation; and secondly, by its sedative influence upon the nervous system, whose excitability is largely and irregularly developed, in proportion to the violence and extent of the phlegmasia; and under which condition of the nervous system, the bloodvessels, whose function is controlled by it, tend to continued and augmented action. But, let it be remembered, that the *Veratrum* accomplishes, as has already been shown, both these results, only in a far more safe, permanent and satisfactory manner. That this assertion may be proven to the unbeliever's mind, let the relative virtues of the two agents be tested by the touchstone of comparison.

The mechanical benefits of venesection are accomplished at the expense of an abstraction of a portion of the life's current from the body, the ultimate consequences of which waste is always to be deplored; in addition to this, the immediate advantages obtained by the operation are only temporary, since, whatever fluids may be subsequently taken into the stomach, pass directly into the circulation, and soon the bulk and force of the current is as full and strong as before, without, however, being as nutritive and sustaining.

On the other hand, the same advantages are attained by the action of the *Veratrum*, but in a different way; by its influence, the heart's action is simply held in check, and the force of the circulation reduced to nature's standard, every drop of the precious *pabulum vitæ* is preserved; and thus, after the storm is over, a more speedy and perfect convalescence is ensured.

The sedative influence of venesection upon the nervous system, is also too often merely temporary, and, in individuals whose nervous system is very impressible, is often followed by reaction to such an extent, as to produce an exaltation of organic action, even perhaps to a greater degree than before the operation. In such cases, the remedy is at an end, a repetition

of the bleeding would be dangerous in the extreme, as it would only increase the existing difficulty.

Even in persons who endure well the abstraction of blood, venesection is a remedy that cannot be persisted in. If it conquers, it must do so at a blow, which blow, though a victory, is yet, too often, like Pyrrhus' hard fought battle, almost a defeat, and a repetition of which, would result in utter vanquishment. On the contrary, the sedation procured by the action of *Veratrum Viride*, is perfect and without the danger of inordinate reaction, as the remedy may, without evil consequences, be continued for days, and, if necessary, even weeks, as is proven by my having, in a case of typhoid fever, with excessive arterial excitement, kept the heart's action controlled for thirty days, merely with the view of saving its machinery.

Let the question of the "danger" of *Veratrum*, be now considered. From a tyro-experience, I can readily comprehend how the idea might originate, and be, too, so impressed upon ones mind, as not to be easily eradicated. It was the writer's fortune soon after entering upon the practice of medicine, to have a case in which it was determined to test the efficacy of Norwood's tincture of *Veratrum Viride*, the object being to control the circulation. The subject was a lady laboring under a high grade of fever. The medicine was administered, according to the directions upon the bottle, by giving eight drops every three hours, increasing each dose by one drop, until the effects were produced. The result was, that suddenly and shortly after the third dose, there ensued the most distressing nausea and vomiting, together with frequent purging, (this last occurring from relaxation merely,) and a most excessive and alarming prostration—a degree of muscular relaxation, that seemed to portend a fatal collapse, and an icy coldness which simulated the chill of death itself—sorely was my mind oppressed, for I felt sure my patient was dying; terrible and afflicting was the grief and consternation of the family, one of whom gave free vent to her belief, that, the "Doctor had killed her mother." Friends and relatives were hurried for to see her die, but the tumult was, after a while, quieted, by the application of sinapisms, and a few portions of morphine and brandy, and the lady sank into a sweet sleep, with a soft, slow and regu-

lar pulse; the fever seemed jugulated, but after a lapse of eight or ten hours, the heart's action began to increase, and the fever was allowed again to develop itself, because, at that time, all the demands of Golconda could not have induced me to resort to the *Veratrum* again; I thanked God that my patient had not died, and resolved that, if ultimately she did not recover, she should expire under such circumstances, that disease should share with me the responsibility of her death. She recovered.

I have related the above experience, not only for the purpose of showing how a belief as to the danger of the *Veratrum* might originate, but also, because, from a constant recollection of the phenomena, produced by its action upon that occasion, and a continued reflection in reference to them, I have been taught much of the value of the remedy, its mode of administration, and its unequalled power to assist nature in working cures. I also record it, for the reason, that a recollection of it, suggested to my mind years after, that Therapeutical power, the publication of which, is the chief purport of this article. The above experience, corroborated by subsequent observation and experiment, has also taught me these additional facts, viz: that though to Dr. Norwood the honor and gratitude of the Profession is due and cheerfully rendered, for having furnished us with this most invaluable agent of the *Materia Medica*, yet, that the directions that he has labeled upon the vials, and which are to be found in the last editions of the U. S. Dispensatory, have done more to retard and prevent its employment, than any other thing. The doses mentioned, are too large for general use, and their effects have intimidated many practitioners, to the extent, that they refuse to administer it. And even when this is not the case, the nurse or family to whom its administration is committed, in consequence of their uneasiness and alarm at its effects, cease to give it with any certainty or regularity. The Physician consequently being disappointed in its results, leaves off the remedy, declaring that though it was pushed to the most distressing vomiting, it ultimately did no good. All this is the more probable, as the *Veratrum*, besides being exceedingly energetic, is also, accumulative in its effects. The truth of this, may be recognized, by the sometimes sudden and violent outburst of symptoms, as well as from the fact, that when the sys-

tem has once been impressed by it, its effects may be permanently retained by the occasional administration of a drop or two of the medicine, at long intervals. All these facts have convinced me, that when it is desired to continue the remedy for any length of time, it should be commenced in much smaller doses, and that so far from increasing the quantity each time, it should, on the contrary, be diminished, and the intervals even lengthened. In this way, the proposed result may, though slowly, be unfailingly obtained, without too, the slightest untoward symptom.

When I have twelve or fifteen hours to go upon, and desire to obtain the specific effects of *Veratrum Viride*, I usually administer it to an adult, in five drop doses every two hours, until two or three portions have been taken, after which, the interval is lengthened to every three hours. Before each and every dose, after the first, I always carefully note the pulse, and if it exhibits the least tendency to depression, the dose is diminished or the interval still further prolonged. This is done for the reason, that some constitutions are extremely susceptible to its action, as was the case of the lady already mentioned. This caution, is, however, preserved only to avoid unpleasant symptoms, and unnecessary alarm on the part of the patient and friends, and not by reason of any apprehension as to the danger of the remedy; for, I have been convinced that the utter prostration capable of being induced by it, is without any greater degree of danger than that condition, which the steamers produce by the abuse of lobelia, and which in their jargon is styled "the state of alarm." Still it is not easy to convince the friends and relatives of this, and if the Physician allows his patient to pass into such a "state," he will certainly find himself hurried after with the greatest dispatch, and with the startling announcement, that his patient is dying!—and though he may find him quiet and comfortable, it will be difficult to have his directions carried out in reference to a continuation of the medicine.

Thus it is, that I have endeavored to show the *modus operandi* of *Veratrum Viride*, and the proper method of its administration, in inflammatory diseases, designing, and hoping, to convince the timid doubter of its incomparable virtues and entire safety.

Did my space admit, I could here record scores of cases, demonstrating the wondrous powers of this agent; proving it a specific in Pneumonia, and showing it capable of robbing *puerperal fever* of its deadly terrors. But, I trust that I have written that which is sufficient to convince the Pathologist that it is, at least, a remedy not carelessly to be discarded.

In leaving this portion of the subject, I would remark, that the virtues of this agent are not alone manifested in sthenic inflammations, but that its administration is equally applicable in conditions where the very opposite obtains. I have often derived the utmost satisfaction from its use, in the very worst forms of Asthenic Pneumonia, and I have now in my mind, such a case, wherein the administration of five drops of *Veratrum* combined with five grains of Quinine, every three hours, wrought in the space of twelve hours, a change that seemed almost a miracle.

I come now, to direct special attention to the most important truth, which my experience and observation have developed in reference to the *Veratrum Viride*, and that, which is *the truth* I propose to offer on these pages. I have hitherto declared that it exerted a sedative influence upon the nervous system; I say now, that it is eminently a *nervine*, whose primary influence is spent both upon the cerebro-spinal and ganglionic systems, and that all the manifold indications, so successfully fulfilled by it, are alone dependent upon this fact. Now, all that has been published, either in the U. S. Dispensatory or elsewhere, concerning the remedial powers of this agent, was written in reference to its value in the treatment of febrile and inflammatory diseases. Indeed, in Dr. Norwood's pamphlet upon "the Therapeutical powers and properties of *Veratrum Viride*," its virtue as a controller of vascular and arterial excitement, constitutes the chief burden of his song, and, but an incidental mention is made of its other equally prominent and valuable powers. It is true, that Dr. Norwood has said that *Veratrum* was *nervine*, not *narcotic*, that he also remarked that Prof. Frost of Charleston, had employed it with benefit in *Cancer** and epilepsy, also, that its *emetic* effects *relieved* the symptoms of *acute chorea*, and that it was valuable in the convulsions of

* The Italics are my own.

children accompanied with *febrile* action. Yet all these remarks were made so disconnectedly with each other, so incidentally, and in such connections, as clearly to show that, the Doctor had no adequate idea, of the *real* nervine properties of his remedy, or just conception of the full therapeutical powers of the priceless boon he was offering the Profession. Under the force of such facts and circumstances, I hope that I am justified in recording an experience, which tends to show that *Veratrum Viride* is as valuable a nervine, as an antiphlogistic, and that in the treatment of certain neuroses, it stands as prominent and as unrivalled, as it does in the management of inflammatory diseases.

On the 19th of February, 1857, I was called to a stout, healthy man, of sober habits, reported to be having "fits." I found him sitting on the side of the bed, seemingly well and perfectly intelligent, unaware, however, that he had had convulsions; all that I could ascertain of his previous history, was that he had been similarly affected in childhood. While conversing with him at his bed-side, he was suddenly, and without apparent premonition, seized with a frightful convulsion, occasioning frothing at the mouth, and the most violent jactitation of all the voluntary muscles. I immediately opened a vein and bled him profusely, but without the desired result, for, after the lapse of a certain period, with as perfect a return of consciousness as before, there occurred another convulsion of equal severity. In this emergency, the excessive muscular relaxation capable of being produced by *Veratrum Viride*, occurred to my mind, and I reflected that such an effect could only be produced by an influence primarily exerted upon the cerebro-spinal system of voluntary nerves. I instantly determined to act upon the reflection, and administered the *Veratrum* in full and frequently repeated doses, desiring and confidently expecting to produce the same train of distressing symptoms that so alarmed me some years previously. In this, I was disappointed, for, though the convulsions were arrested, there occurred no other symptom than a relaxed skin with profuse perspiration. In this case, I do not remember the number of drops given, but I do well recollect that the doses were large, and that the same quantity, in a less excited state of the nervous system, would, beyond all

question, have produced the effects exhibited in the case of the lady, already related. Since then, I have administered *Veratrum Viride* in numerous cases of eclampsia in children, with such satisfactory results, as have established beyond all doubt, the power of this agent to ARREST CONVULSIONS. Indeed, *I feel perfectly assured that it is a physical impossibility for convulsions to continue, after the system has been fully impressed by the remedy.* My experience is, that in these cases, *the medicine is tolerated in much larger doses than under ordinary circumstances.*

I have had but one opportunity to test its powers in puerperal convulsions, from the fact, that I have met with but one case occurring *after* delivery had been accomplished, and I have always withstood the temptation to administer it *before*, in consequence of a strong belief, if not certain conviction, that its action would arrest uterine contraction.

About two months ago, I was called with my friend, Dr. Terry, to see a woman suffering with puerperal convulsions. We found labor advancing, and though the convulsions were severe and frequent, we determined to leave its accomplishment to nature, and to endeavor in the meantime, to check or control the convulsions, by the inhalation of chloroform, there being certain symptoms which seemed to contraindicate venesection. Dr. Terry having been called away, I remained to watch the case and administer the chloroform; but as the labor made slow progress, and the convulsions increased in frequency and severity, I bled the woman copiously and sent for Dr. T. to return and bring instruments. He soon came and the woman was delivered, by means of the forceps, of a dead child weighing eleven and a half pounds. I remained with the case an hour, when, being called out of town, I left the lady quiet and comfortable. Upon my return, four or five hours later, I found her in a most violent convulsion, which was reported to be the seventh since delivery. I immediately gave her *fifteen drops of Veratrum Viride*, and directed that she should take ten more in two hours, after which, the intervals should be prolonged to three or four hours, according to circumstances. *There occurred no more convulsions, and the woman recovered perfectly, she was not even nauseated, though the medicine was given at regular intervals during the whole night.*

I will now record my experience with the *Veratrum Viride*, in a case of *Chorea*.

On the 7th of June, 1858, I was called to a young lady suffering from a violent attack of Chorea. The mother informed me that, it had been very gradually coming on, for a month or two; her symptoms, when first visited, were distressing to the last degree; her entire muscular system was in continuous and tumultuous commotion, so much so, that it was with difficulty that she could be kept upon the bed. This case passed on from bad to worse, notwithstanding the most assiduous attention and energetic treatment; tonics, antispasmodics and anodynes, were exhausted without avail. The spine and nucha were cupped and blistered without benefit, chloroform was administered both internally and by inhalation—in fact, every remedy that could be legitimately suggested was resorted to, but without success, and it seemed at last, that the girl must die from exhaustion and want of sleep. Opium, and its various preparations, appeared to make her worse; once or twice I suggested the employment of *Veratrum Viride*, but it was postponed, until on the 18th of the month, when my partner, Dr. Thornton, under whose especial care the case had been, was called off to Georgia. On that day, just as I was starting to visit her, my friend Dr. Terry, hearing of the unusual violence and obstinacy of the case, stopped me and proposed that I would give *Veratrum Viride* a trial, assuring me that he had thrice used it in Chorea with the most satisfactory results. So, I concluded, both from my own experience, and from his positive statement, to withdraw all other medication and give the *Veratrum Viride* a full and fair trial. The family had given up all expectation of her recovery. I told them, however, upon my arrival, that I had come with a new remedy, the last and only one that had not been tested, and that I felt convinced that the girl would recover under its use. I at once commenced its administration, and as she was gradually brought under its influence, the turmoil began to cease; the face which had been worked by its muscles into the most ludicrous and horrible distortion, became placid and intelligent, the head had ceased its everlasting jerking, the extremities lay still, the body left off writhing, and the patient quietly passed into a peaceful and profound slumber. This sleep was

deep and long, as it was the first, with few and slight exceptions, that she had had in nearly two weeks, and the quiet that the muscles now received, was all that had occurred, save during those few and short slumbers. At a subsequent visit, I found the family cheerful and hopeful, and the patient quiet and sleeping, the pulse but little depressed; there had occurred no vomiting. I roused her, and, to my great satisfaction, when awake, there was no jactitation of the extremities, and but very little twitching of the muscles of the face.

In this case, I thoroughly tested the influence and power of *Veratrum Viride*, for the first few days; if its administration was withheld, the commotion began gradually to return, but all would again become quiet, upon resuming its employment. At first, so continually did she sleep under the quiet that it induced, that the family called the *Veratrum* preparation "the laudanum mixture," notwithstanding they were aware that she had taken large quantities of morphine, without benefit, in our efforts to induce sleep. The *Veratrum* was continued for several days, the convulsive movements ceased altogether, the muscles became completely obedient to the will, and the lady returned to perfect health and blooming beauty, under a judicious and properly regulated tonic course of treatment.

In corroboration of my experience, I will here relate three very interesting cases, politely furnished me by my friend Dr. Terry, of Eufaula, Alabama, formerly of Georgetown, Georgia.

"Case 1.—I was called in Randolph Co., Ga., to visit a child aged twelve years. It had been confined to bed for three weeks, and was reported to have been under treatment for about six weeks, first, for worms, with calomel, spigelia, wormseed, &c., and subsequently for chorea, (with which I found it suffering,) with cemicifuga, iron, quinine, and the usual routine treatment, until the child was apparently dying.

It is not in the power of language to convey a proper conception of the truly pitiable state in which I found this child; no description can afford any adequate idea of its appearance and condition. It had slept none, neither had it taken any nourishment for days; it was evidently dying from exhaustion and inanition, the muscular commotion was violent, universal, and unaffected by sleep; the lips embossed with foam, worked

up, by a continual champing of the teeth. I instituted the following treatment: three drops of *Veratrum Viride* were administered every three hours; the vehicle for each dose being a teaspoonful of gum water, a small portion of which was introduced into the mouth every few minutes, until the whole was given, the medicine being in this way, rather *absorbed* from the mouth probably, than swallowed. In twenty-four hours, I had the gratification to see the symptoms greatly improved. The muscles were much quieter, and the child could swallow without difficulty, (the trouble in this respect, had constituted the greatest embarrassment in the treatment.) I continued the *Veratrum Viride* in connection with iron and quinine. At the end of the fourth day, all convulsive action had ceased; the V. V. was still farther continued, though in smaller doses, and at long intervals. Quinine, iron and generous diet completed the cure promptly.

Case 2.—Girl, aged fifteen years. This was an ordinary case. She was purged freely; after which four drops of V. V. were administered every three hours. Under its use, the convulsive phenomena soon disappeared, after which the V. V. was continued for a few days at long intervals. This case also promptly convalesced under the use of iron, quinine and generous diet.

Case 3.—Woman, aged thirty-six years; had borne no children; was subject to menorrhagia, immediately after an attack of which, she was taken with chorea, marked by continued nodding of the head, and violent convulsive action in one arm, together with slight jactitation of one leg. In this case I directed six drops of V. V. every three hours; the fourth dose occasioned slight nausea, and after the fifth dose, the convulsive action ceased, when the V. V. was withheld. There followed in the course of eight or ten hours, a return of the symptoms; the medicine was again resorted to with the former quieting result. The doses were then reduced, but continued for several days at long intervals. This case, like the others, recovered under the use of quinine, iron and generous diet."

Such is the experience, and such are the facts, that have taught me that *Veratrum Viride* is the most valuable, safe and certain agent in the treatment of those neuroses characterized by

convulsive phenomena, and I here would suggest it to the Profession as a remedy to be considered, if not relied upon, in the management of those direful maladies, tetanus and hydrophobia. I have never had an opportunity to test it in either, but were a case submitted to my care, such is my faith in the *Vera-trum Viride* in all convulsive affections, that I would place more reliance upon it than upon any other agent of the *Materia Medica*.

ARTICLE XX.

Remarks on the Treatment of Inflammation and Ulceration of the Womb ; and more particularly on Irrigations in Ulcerated and Inflamed Os Uteri. By JOHN STAINBACK WILSON, M. D., of Muscogee County, (near Columbus,) Georgia.

IN the treatment of the above disorders, our means should be directed mainly to the removal of the local inflammation and congestion. To accomplish these ends, there is nothing more safe than the topical and general application of water. It is hardly necessary to say any thing to physicians as to the intimate connection between the skin and mucous membranes, or to remind them of the immense amount of blood that may be diverted from the internal organs, to the vast network of cutaneous capillaries. We have treated some very obstinate cases of chronic inflammation and ulceration of the uterus, which had resisted the persevering use of the ordinary remedies, after the following fashion, with the most satisfactory results. We began by sponging the whole body, daily, with tepid water, gradually reducing the temperature, as the patient was able to bear it, and following each application by active friction with the hand, or with a coarse towel, or a piece of grass cloth, which is excellent in such cases. This of course produced a strong determination to the skin, relieving the local vascular congestion of the womb; to say nothing of the equalizing, sedative and tonic effect on the nervous system, which is often so much disturbed and unbalanced in these affections. In addition to this general application, we have frequently prescribed a short

cold hip-bath, two or three times a day, to be followed by a good rubbing around the hips, back, loins, lower part of the abdomen, &c. This bath has but seldom been continued more than from one to three minutes each time, the object being to produce a revulsive reactive determination to the skin, over the affected part, rather than a profound, direct, sedative effect on the womb itself. Besides the sponging and hip-bath, we have been in the habit of prescribing a cold wet bandage over the womb, and not unfrequently over the lower part of the back. This, by its action on the capillaries, sometimes causes an eruption on the skin, almost as severe as that from tartar emetic, thus acting as a counter-irritant, and powerfully aiding the strong diversion already obtained by the baths, frictions, and the continuous action of the bandage which has been for some time preceding the eruption, in constant contact with the skin. This eruption is the "crisis" of the hydropaths, but its appearance can be readily explained without the absurd assumption that it originates from the elimination of drugs, poisons, the *materies morbi*, &c., which escaping in a tangible form, tear the body like so many devils. In using the bandage, we have generally continued it day and night, re-wetting it whenever it became dry or uncomfortably warm. For the removal of the headache and palpitation of the heart, which frequently attend this protean disease, we have found the following plan very efficacious: For the palpitation, we have prescribed cloths wet with cold water and applied over the region of the heart; and where there was much heat and excitement about the chest, the cloths have been exposed to the air without any covering, and changed frequently. We recollect a case in which the heart palpitated most violently, intermitted and cut up a great many strange capers, much to the annoyance of the poor patient, who sometimes thought that her hour had verily come. The wet cloth as above, and the hot foot-bath, which is a valuable adjuvant, never failed to give relief in this case, until the original cause of the palpitation,—inflammation and ulceration of the womb—having been removed, the cardiac difficulty gradually disappeared.

Headache is best relieved by the hot foot-bath, by strict attention to diet, to the bowels, and to the state of the mind. We

have commonly succeeded in regulating the bowels by simple enemata and a laxative diet. This is far preferable to the habitual use, even of mild purgatives. But the main object of this paper, is to call attention to a remedy in inflammation and ulceration of the womb, which, if we may decide from our limited observations, is inferior to none in safety and efficacy; and in some cases is superior to all others.

Uterine Irrigation is recommended by Colombat in cancer of the womb; (Meigs's Translation, page 344,) but we are not aware that the remedy had been used to any extent in this country, in cancer, or any other form of uterine disease. The apparatus for this womb-bath, is quite simple, and can be fitted up extemporaneously by any one. It may be prepared by having a common funnel, a stool, of the proper height, and long enough to stand astride the patient, with a hole through it for the funnel; and all that is necessary besides this, is a gum elastic rectal tube which may be fitted to the pipe of the funnel with a little bee's wax. We have used such an apparatus as this in a very severe case of ulceration, involving both lips of the os uteri, and which had resisted the repeated use of that excellent remedy, nitrate of silver. In this case, the mouth of the womb was not only ulcerated, it was swollen and livid; so much so, as to excite the most serious apprehensions of malignant disease; besides this, the patient was two or three months advanced in pregnancy, and her general health was very much impaired; indeed she had an almost endless list of disagreeable symptoms, such as palpitation of the heart, headache, dyspepsia, with all its horrors, mental hallucinations, gloomy forebodings, and in short, all the undescribed, and *never-to-be-described* symptoms embraced in the comprehensive and vague term, nervousness. Yet under the use of cold uterine irrigations, the inflammation subsided in a few weeks, the ulceration healed in due time, and her general health became better than it had ever been in any of her previous pregnancies, she having had four children before. And all this without a single dose of *medicine*, using the latter word according to its conventional and popular signification. Yet if *medicine* means any thing that cures, water must be a most excellent medicine indeed, to accomplish such results.

And in this connection we may be permitted to add, that

even we physicians are too much prone to fall into the popular error, that medicines are confined to the shelves of the apothecary, that they consist exclusively of the simples and compounds of the druggist and chemist. And thus are we liable to forget those great medicines manifested in nature's grand laboratory, and which, in many cases, are far more safe and efficient than the most subtle and refined productions of art.

In the case under consideration, the irrigations were used for awhile, as long as six hours in a day, with short intervals of rest. The rule should be to continue the application as long as possible, each time, so as to produce a decided sedative and constricting effect on the excited and congested vessels of the affected part; and as soon as reaction occurs, which will be manifested by increased pain, the irrigations should be repeated until the effect becomes *permanent*, until the vessels contract to their proper size, and the inflammation subsides.

Since her confinement, the subject of this case has had a return of the ulceration, and as the inflammation seems to be more indolent, the cold water has not succeeded so well as before, and as she is strongly prejudiced against the nitrate of silver, from her former experience, a very strong decoction of red oak bark has been applied, by pouring it into a glass speculum, and letting it remain in contact with the ulcer from three to ten minutes. The immediate effects of this, are pains in the back and considerable local irritation, but in a few days there is a marked improvement in all the symptoms. What will be the alternate result of this treatment, time must determine, as the case is still on hand.

In conclusion we would remark, that we know how strong popular prejudices run against the use of cold water in uterine affections, and we are not sure that the profession does not participate to a considerable in these feelings; yet we cannot possibly see any ground for fear in the use of uterine irrigations in inflammation of the os uteri, in the absence of any special contraindication, of which the physician is to judge. In cases where the local and general symptoms are decidedly asthenic, the remedy might be inappropriate; and the same thing might be true in cases where some pulmonary or other complication exists, or where there is extreme sensitiveness to cold. But in

cases where the excitement is great, where the inflammation is as an acute or sub-acute character, we cannot possibly imagine any remedy that would be more likely to produce the most happy effects, than the direct and continuous application of cold water to the inflamed part. And even where inflammation is more indolent, where the symptoms of excitement are slight, our observation has taught us that nitrate of silver and such like remedies, sometimes produce a morbid irritation, and aggravate the disease. The only disagreeable symptom that we have seen from cold uterine irrigations, was a muscular soreness, or a rheumatic affection of the muscles, and fascia of the abdomen. This readily yielded to the application of warm cloths over the painful parts.

ARTICLE XXI.

Ergot and the Tampon in Placenta Prævia. By WILLIAM A. MATHEWS, M. D., of Fort Valley, Georgia.*

I have had it in contemplation for a length of time, to report through the pages of the "Southern Medical and Surgical Journal," my views of the treatment of "*Placenta Prævia*," or "unavoidable Hæmorrhage;" illustrated by the details of one or more cases. My only motive in making this report, is to save the young and inexperienced members of the profession, from, at least, some of that intense anxiety, always attendant upon the management of those alarming cases. Hoping to make some suggestions that will render the treatment simple, and at the same time safer, to the unfortunate woman.

I do not presume to cast any additional light upon the *nature* or *causes* of the hæmorrhage in these cases; nor make any suggestion as to their prevention; neither do I claim for the mode of treatment much, if any thing, original. There are but few Physicians who have been long in practice, who have not met with one or more cases of "unavoidable Hæmorrhage," and who have not felt the want of that clear and satisfactory conviction, as to what course of treatment would best subserve the safety of his patient, and been subjected to that tantalising

* The above communication was sent on the 15th of May, 1859.

uncertainty, which sometimes results, in either too much temporising on the one hand, or temerity on the other. Trusting too long to astringents and the tampon, or resorting too early and unnecessarily to manual aid or delivery by *turning*.

It has been my misfortune to meet with a number of cases of "Placenta Prævia," and some of the most painful reminiscences connected with my past professional life, are associated with this always precarious condition. I have seen some in my earlier practice, die, who might, I honestly believe, have been saved by the practice which has been pursued by me in several other cases, within the past nine years. And if I can aid the young and inexperienced in safely conducting such cases, and thus contribute to saving the life of one dear devoted mother, one beloved, affectionate wife, I shall be abundantly compensated for making this communication.

Wishing to occupy as small a space as practicable in your valuable Journal, I will proceed to give the history from memory, of two cases which came under my observation and treatment, within the five past years.

During the winter of 1854, I visited Mrs. S. of an adjoining county, who I had learned some weeks before, had occasional attacks of "flooding," each attack more violent than the preceding. On arriving I found Dr. H., the family physician, present. The day being cold, I went into an adjoining room to warm, and while Dr. H. was relating the condition of his patient, a messenger came in and stated that Mrs. S. seemed to be in great distress, was very restless, and seemed worse. On going into her room we found her as represented, presenting all the symptoms of great exhaustion from loss of blood, which seemed to have come on within ten or fifteen minutes, as the Doctor had discovered no cause for great alarm when he left his patient's room, about the time of my arrival. A very large quantity of blood was found in the bed, and the discharge still profuse.

Upon examination, the os uteri was found *quite soft*, and *dilated* to the size of a dollar, and a small part of the placenta lying in it. She was complaining, and had been for some time, of rather irregular pains.

In a few words, I explained the peculiar and very precarious

condition of the lady, and feeling the absolute importance of checking the loss of blood, suggested the tampon, which was forthwith applied. We then commenced the administration of *ergot* in free doses, which in less than an hour, brought on active uterine contraction, and in two and a half hours, the fœtus was expelled, preceded by the placenta and tampon.

So great was the exhaustion, that syncope ensued soon after delivery, and continued to recur, rendering it necessary to use stimulants internally and externally, and to place the patient's head below the line of her feet, by elevating the foot of the bedstead.

Mrs. S. gradually recovered, and has borne one or two children since. She was delivered at about the end of the eighth month, as well as I recollect.

Case 2d. Sometime in the month of March last, I was requested to see Jane, a servant of Mr. M., near this village, who, I was informed, was suffering with a painful swelling of the veins of her legs. Jane was about six months and a half advanced in pregnancy, about twenty-five years of age, and the mother of three children. Finding her laboring under severe febrile excitement, I bled her moderately, advised some aperient, and rest in a recumbent posture, &c. Some four or five weeks after the above, I was called to see the woman in some haste, and found her laboring under "uterine hæmorrhage," not however, at all alarming in character. I learned, on enquiry, that Jane had been attacked without any premonition, while upon her feet. The history of the case was such as satisfied me, that it was one of "placenta prævia." I explained to her mistress her peculiar situation, that nothing could be done only to palliate, advised some astringent and *absolute* rest, with an anodyne at night if she did not sleep, or suffered increased uneasiness in her back, of which she was then complaining, and requested if the hæmorrhage *considerably* increased, to be notified immediately.

I visited my patient next day by request; was informed that the flooding was much less during the night, but had greatly increased since breakfast. I now for the first time, examined her per vagina, found a quantity of blood in the bed and a large coagulum nearly filling the vagina. The os uteri was considerably dilated, soft and flaccid, and at least two-thirds covered

by the placenta. She was suffering from pretty regular pains, and loosing blood constantly. Conceiving delivery absolutely necessary, I commenced with the ergot, giving it in free doses. In some forty or fifty minutes, the uterine contraction increased in force and frequency, expelling at almost every one, considerable quantities of blood. Watching my patient's pulse, I continued the ergot in increasing doses.

In about two hours and a half after commencing the ergot, I found on examination, the membranes entire and near the vulva, becoming during the uterine contraction very tense. Hoping to bring on more efficient contractions, I ruptured the membrane and gave exit to a large quantity of "the waters." The pains now entirely ceased, and my patient remained for about one hour free from all uneasiness.

After waiting some fifteen minutes for a return of pain, I examined again, found the os still more dilated, and nearly filled with the placenta, and blood flowing copiously.

Discovering some symptoms of exhaustion, and finding it necessary to save as far as practicable the loss of blood, I resorted to the tampon. Continuing the ergot in large doses, and using friction over the womb, I watched with great anxiety the strength of the woman. At the expiration of the hour, I had the gratification of seeing the uterus resume its action; the contractions became expulsive in character, and in twenty minutes more, the foetus was expelled, preceded by the placenta, and my patient was safe.

As in nearly all the cases I have seen, the foetus in both these instances was dead from exhaustion. I have thus in as condensed a manner as is compatible with perspicuity, given the details in the treatment of the above cases, upon which I will offer but few reflections.

When called to a woman laboring under "uterine hæmorrhage," about the seventh month of utero gestation, if the attack come on suddenly without any premonition, or if the woman be at any stage of pregnancy between the six and a half and eighth month, the physician may pretty safely diagnose "*Placenta Prævia*." If the loss of blood be not alarming in quantity, enjoin rest and prescribe some astringent, of which I know of nothing preferable to *alum*; and if the woman complains of occa-

sional pain, give an anodyne. If the loss of blood has been great, and the flooding continues, resort to an examination. Should the os be found undilated or not soft and dilatable, use the tampon. If the condition of the mouth of the womb be different, that is, soft and dilatable, or considerably dilated, the ergot may be resorted to with safety. Until, however, such is the condition of the os, the tampon is the only efficient remedy, and one that generally meets all the indications until the use of the ergot will be safe.

It will be seen that I rely almost entirely upon the tampon and ergot. The former is always important in excessive "uterine hæmorrhage," *except such as succeed immediately or within a few hours upon delivery*. Such is the relaxing influence of the loss of large quantities of blood upon the muscular system, it will very rarely happen, that the specific property of the ergot may not be called into requisition in ample time to save the patient.

I am aware that many of high authority object to the use of the tampon in such cases, supposing, that by causing the blood to coagulate about the bleeding vessel, the coagula thus formed, would still farther separate the placenta from the uteri, and thus increase the difficulty. With all due deference, I consider such objections altogether imaginary.

In the administration of ergot, two conditions are absolutely necessary; a proper condition of the soft parts, and such a presentation of the fætus as will admit of its expulsion by the contractions of the uterus.

Of delivery by turning, I will only say, as a *dernier resort* remedy, it can always be had recourse to, when the ergot fails. But if the ergot is good, and freely administered, it may be relied upon with great confidence.

[Professor Henry Miller of Louisville, in a recent excellent Treatise on Obstetrics, strongly recommends the Tampon and the rupture of the membranes. His remarks thus far, strongly corroborate the views of our correspondent; "The supervention of Labor—the evacuation of the liquor amnii—these, in their order, are the great bulwarks of flooding women—no matter *where* the placenta is implanted. A *contracting* uterus cannot bleed when emptied of its waters,—at any rate, if it bleed, the hæmorrhage is no longer dangerous."]—*Editors S. Med. and Sur. Jour.*

The Glycogenic Function in Fœtal Life. Two Communications by M. CLAUDE BERNARD and M. SERRES, to the Academy of Sciences, Paris, at its Sitting of January 10th, 1859. (Translated for the Nashville Monthly Record, by DANIEL F. WRIGHT, M. D.)

I.—*On a New Function of the Placenta*, by M. CLAUDE BERNARD. The object of my communication is to establish anatomically and physiologically that the placenta, with other uses which are undoubtedly varied and numerous, is destined during the earlier stages of foetal development to accomplish the glycogenic function of the liver, before the latter organ has acquired that development of structure which subsequently enables it to perform that function.

I have been for a long time led aside from the result towards which my researches aimed, through making my experiments upon the placenta of the ruminant animals, which are easily procurable at the shambles of Paris. During several years I fruitlessly made repeated observations upon calves and lambs taken at every stage of intra-uterine life, and was still unable to find any part of the placenta of these animals which contained the glycogenic matter. Spite of these early failures, I afterwards had recourse to the placenta of rabbits, guinea-pigs, etc.

I found that there is in the placenta of these animals a white substance formed by epithelial cells or agglomerate glandules. I moreover determined that these cells, like those of the liver in the adult animal, were filled with glycogenic substance. This mass of glycogenic cells appeared to me to be chiefly situated between the maternal and the foetal portion of the placenta, and subsequently to its development seemed to be atrophied in proportion as the foetus approached the period of its birth. I have since ascertained that the placenta of rabbits and guinea-pigs is formed of two portions having distinct functions: the one, vascular and persistent till birth; the other, glandular, preparing the glycogenic matter, and having a more limited duration.

Meanwhile the negative observations made upon so large a number of the ruminants arrested my attention: negative experiments which were to me just as indubitable as those from which I obtained the above positive results.

In repeating my researches, I succeeded in establishing a remarkable arrangement which no one before had distinctly pointed out: that while the vascular portion of the placenta represented by its various cotyledons accompanied the allantois and was distributed on its external surface, the glandular portion was distinct from it and was developed on the internal surface of the amnion. Whence it results, that if in the rodentia, and the other animals with a simple placenta, we find the vascular

and glandular portions of the placenta mixed together, in the ruminantia, on the other hand, we find the vascular and glandular portions of this organ developed separately on distinct membranes, and capable, in consequence, of being examined separately in their respective evolution. Thanks to this anatomical arrangement, we are able to prove clearly that the vascular portion of the placenta persists and grows until birth, while we see that the glycogenic portion attached to the amnion increases during the earlier periods of gestation, and attains its greatest development towards the third or fourth* month of intra-uterine life, then disappears by degrees, passing through the various forms of atrophy and degeneration; so that at the birth of the mammifera there no longer exist any traces of that temporary hepatic portion of the placenta. But we ought moreover to add, to arrive at the true character of these organs, that during the whole period while the hepatic placenta of the amnion is growing and performing its function, we observe that the fœtal liver does not yet possess its appropriate structure or functions, and that it is precisely at the moment when the liver is developed, and its cells, having acquired their characteristic form, commence secreting glycogenic matter, that the hepatic organization of the amnios has a tendency to disappear.

The hepatic patches of the amnios appear in ruminants from the earliest periods of embryonic life. They are developed by degrees on the internal face of the amnios, covering over the umbilical cord to a point where a well-defined line separates the fœtal integuments from the amnion. Afterwards these patches, which, particularly along that portion of the membrane which invests the cord, assume the appearance of villousities, extend themselves along the other portions of the amnios, in proportion as the blood-vessels which accompany them are developed. They gradually increase in volume; formed at first of a transparent matter, they become at a later period opaque, especially towards their edges, which are elevated a little, and sometimes cause them to resemble in their appearance patches of lichen. At other times they have quite varied appearances, flattened or filiform, and are sometimes blended one with another so as to become confluent. At their complete development, the patches attain a thickness which sometimes amounts to three or four millimetres; those which are filiform often present a considerable length, and are sometimes enlarged at their extremity in the form of a club. At a later period these hepatic patches of the amnion cease to be developed; at certain points they

* I am only able to give here these limits approximately, by reason of the impossibility of ascertaining the age of those calves which are procured at the shambles.

become yellowish, of a fatty appearance; in other places they fall off and float in the amniotic fluid, and leave on the membrane a kind of cicatrices which afterwards completely disappear.

We can determine with great ease the presence of glycogenic matter in the hepatic patches of the amnion at all periods of their development. From the time when they first appear, it is easy to recognize this matter under the microscope with the aid of iodine. Until the patches are completely developed we can obtain from them their glycogenic matter in large quantities and study its characters. To obtain it easily, the process will consist of digesting the amniotic membrane in boiling water, which will enable the patches to be easily detached for the purpose of rubbing them in a mortar, and extracting the matter from them by boiling, exactly as we proceed for the extraction of glycogenic matter from the liver.

As regards its characters, we may say that the glycogenic matter of the amniotic patches exhibits the most perfect identity with the glycogenic matter of the liver. It is dissolved in the water, giving it a milky appearance; it may be precipitated by alcohol and crystalized by acetic acid. Iodine communicates to it an intense vinous red color, which disappears when heated and reappears on cooling. This coloring of the glycogenic matter of the amniotic patches by iodine takes place not only when the matter has been extracted from the cells by boiling, but it may be observed also in the cells of the organ themselves, as we shall see presently. Like the glycogenic matter of the liver, that of the amniotic patches also changes into dextrine or fermentible sugar (*glucose*) with great ease under the influence of diastatic ferments, both animal and vegetable, and by the action of boiling with strong acids.

While we study the structure and the histological development of the hepatic patches of the fœtus, we can follow out very distinctly the formation of the glycogenic cellules, as well as the development of their contents.

The amniotic membrane in the calf seems to be at first destitute of any well-marked epithelium, and we find its tissue chiefly constituted of fibres of elastic tissue with nuclei contained in a network of cells of a fusiform aspect. At the very moment of the appearance of the patches we discern under the microscope on the internal face of the amnios, and continuously along that part of the membrane which invests the umbilical cord, a kind of spots formed of epithelial cells; then in the centre of each spot we observe groups of glandular cells in very small number at first, and soon we are able to observe the patch at its very origin and composed of only one or two glandular cells. We very easily distinguish the glandular or glycogenic from the

epithelial cells which accompany them, both by their appearance and by their reaction with iodine. In fact, when we add to an amniotic papilla or patch on the stage of the microscope a little tincture of iodine acidulated with acetic acid, we soon see the glycogenic cells assume a vinous red tint, while the epithelial cells continue colorless or become faintly yellow. By degrees during development the groups of glycogenic cells increase and assume the form of papillæ, especially on that part of the membrane which invests the cord. Examined under the microscope, these papillæ are formed of glycogenic cells covered with an epithelium; as soon as we add the acidulated tincture of iodine, we see the glycogenic cells of the papillæ colored of a vinous red, especially at their base, which is very definitely separated from the surrounding tissue. The hepatic patches are composed of the same elements as the papillæ; it is always difficult to ascertain whether as regards their agglomeration they ought to be considered as consolidated papillæ, or as having another mode of growth. All we are able to say is, that we can see them spread in the direction of their circumference, which exhibits well-developed glycogenic cells, while in the centre these cells sometimes appear to be at a less advanced stage of development.

When we bruise the patches or the papillæ and mechanically separate their histological elements, we obtain isolated cells provided with a nucleus and sometimes a nucleolus, and containing a granular substance: the granular substance is colored wine-red by the acidulated tincture of iodine; the nucleus, the volume of which seems to me to vary under the influence of reagents, never assumes the same color by the action of iodine. The cells of the hepatic patches of the amnion offer a great resemblance in form and reaction to the cells of the liver in a state of function. In fact, we can isolate both the cells of the amniotic patches and those of the liver by allowing a small portion of the tissue of these organs to macerate for some time in a concentrated alcoholic solution of caustic potash; we then see that the contents of the two orders of cells continue insoluble in this reagent, and fall to the bottom of the liquor in the form of a white deposit which offers under the microscope both the original form of the preserved cells and an amorphous granular matter. When, again, under the microscope, we saturate the excess of potash with crystallizable acetic acid, and afterwards add tincture of iodine, we see the wine-red color appear, and that with greater intensity than when we operate on the fresh cells.

When the hepatic patches of the amnios begin to grow yellow, to fall off, to be absorbed, or to degenerate into fatty matter, we perceive changes in their microscopic structure; the glandular cells in general lose their nucleus at the same time with their

glycogenic matter, so that upon treating a fragment of these altered patches with the acidulated tincture of iodine, we see a mixture of cells, some of which assume the wine-red color, while others remain uncolored. It is evident, moreover, that the cells which remain uncolored are destitute of nuclei and of granular contents. We sometimes perceive a transition between these two extremes, that is to say, we see cells in which the nucleus and the granular matter have nearly disappeared, and in which the wine-red color is scarcely perceptible.

A little later, when the patches of the amnios form mere cicatrices, we find only the flat cells, destitute of nuclei, and in which it is impossible to detect the slightest trace of glycogenic matter. These cells at a later period finish by disappearing themselves. When the patches, instead of falling off and disappearing, degenerate into fatty matter, we observe under the microscope the presence of a fatty matter at the same time that we see mixed with it very fine octahedral crystals, which exhibit all the characteristics of oxalate of lime, so far as to be insoluble in water or in acetic acid. It is needless to add that there is, besides, a complete absence of glycogenic matter in these degenerated hepatic patches.

If now we institute an examination of the structural development of the liver, parallel with that which has just been made of the evolution of the glycogenic patches of the amnios, we shall be struck with the constant inverse relation which we observe between the development of the cells of the liver and of those in the amniotic patches.

At the early periods of embryonic life,* when the amniotic patches are well filled with glycogenic matter, it appears that the liver of the fœtus, still very soft, is made up of rudimentary cells rounded or fusiform, soluble in the alcoholic solution of potash, deriving no color from iodine, and showing none of the characteristics of glycogenic cells. At this period the tissue of the liver does not give the least trace of glycogenic matter.

At the end of their period of growth, when the glycogenic cells of the amniotic patches begin to disappear or degenerate, we find in the liver of the fœtus cells which have acquired their definitive forms as liver-cells including one or more nuclei with granular contents, not dissolving in an alcoholic solution of potash and deriving the wine-red color from the contact of iodine, after we have saturated the alkali with acetic acid. It is at this period that we begin to be able to separate from the liver of the fœtus, which has become more firm, glycogenic matter exactly similar to that which the adult liver produces.

* At the commencement of embryonic life in the embryo calf while from two to three centimetres long, I have not as yet perceived the amniotic patches. Perhaps hereafter glycogenic cells will be discovered in the umbilical vesicle.

Later still, when the patches have entirely disappeared or degenerated into fatty matter, and the foetus is near the period of its birth, we find that the tissue of the liver, now grown as solid as in the adult animal, is now made up of elements which have assumed their definitive form: all the cells of the liver are then filled with glycogenic matter, and at that period we can separate from the liver of the foetus glycogenic matter, as abundantly as in the best nourished adult animal.

To resume: From all the facts which are contained in this paper, I think we can make the following inferences:

1. There exists in the placenta of the *mammifera** a function which till now had remained unknown, and which appears to take the place of the glycogenic function of the liver during the earlier periods of embryonic life. This function is located in a glandular or epithelial structure of the placenta, which in certain animals is mixed with the vascular portion of that organ, but which in ruminants appears separate, so as to form on the amnion patches of an epithelial appearance, which every one has doubtless seen, but the significance of which has been hitherto unknown.

2. This temporary hepatic organization of the placenta, by permitting us to study directly in an insulated anatomical element the production of glycogenic matter, confirms and completes by a new example that which I have long ago maintained, that the formation of the glycogenic amylaceous matter is a common faculty of the animal and vegetable kingdom. The observations contained in this paper furnish us with further novel analogies, as we see glycogenic amylaceous matter form around the embryo animal, just as in plants it accumulates in their seeds around the embryo vegetable.

3. The glycogenic function in animals commences, then, from the origin of foetal life, and before the organ in which that function is located in adult life is developed. But then it is located in a temporary organ belonging to the foetal appendages.

4. All which has been said in this paper relates exclusively to the *glycogenic* function of the liver; but it will be really a question, to examine whether the *biliary* function which the liver possesses in the adult is equally accomplished by the placental hepatic organ which we have described. The question should be put in these terms, viz.: Whether the same glandular cells are charged with two functions which thence must be consolidated and united; or whether, on the contrary, the liver

* In birds, (the chicken,) I have ascertained that before the development of the glycogenic cells of the liver there exist glycogenic cells which are developed in the walls of the vitelline sac; but not having as yet completely followed out their evolutions, I will treat this subject in another communication, confining myself at present to speaking of the *mammifera*.

ought not to be considered rather as a complex organ in which are found mixtures of anatomical elements distinct from one another, and destined, the one for the formation of the amylaceous matter, the other for the biliary secretion? This question, which as yet has not been solved, in spite of the numerous histological labors of which the liver has been the object, appears to me capable of being illustrated and even decided by physiological researches, made, on the one hand, on the embryonic developments of the function, and, on the the other, upon the inferior animals. I have entered upon some researches on this subject, of which I will give an account to the Academy as soon as they shall be terminated.

II.—*On Glycogenic Bodies in the Umbilical Membrane of Birds.* by M. SERRES. The important communication of M. Bernard on the glycogenic function of the placenta has dissipated some doubts which, in studying the embryogeny of birds, had arisen in my mind concerning the use of certain small glandular bodies which have been observed on the surface of the umbilical membrane of the chicken in the process of formation.

We know that in the course of the second or third day of incubation there is developed on the opaque portion of the *area germinativa* in the hen's egg, a membrane composed of capillary vessels so numerous that its whole surface is entirely covered with them.

These vessels originate about the twentieth hour of incubation with the appearance of minute cells, which become the *puncta sanguinea* of Wolff. Without communication with one another at first, these cells become covered about the twenty-fourth hour with capillary vessels extremely delicate; they soon form *vascular islets*, isolated at first, but forming unions with each other from the thirtieth to the fortieth hour, so as to form the most beautiful capillary plexus which can be witnessed in the animal organism. These are facts already known.

But (which is hitherto unobserved) there are little glandular bodies interposed between the *vascular islets*, and scattered over the whole surface of the umbilical membrane. They may be seen under the microscope from the twenty-fourth to the thirtieth hour of incubation; their pale color renders them easy to distinguish from the vascular islets, which are of a reddish hue; at the thirty-fifth hour they become of a bright yellow color, and the volume which they have acquired permits us to distinguish them more easily.

It is at this period, so important in the development of the embryo chick, that I have caused them to be represented in the *Archives of the Museum*. In the embryo which was used in designing that figure, their number reached to five hundred.

They were disseminated not only over the *area opaca* of the umbilical membrane, but also over nearly the whole space of the *area pellucida*, in which they were more distinct from the fact that at that period the *area vasculosa* of the germinal membrane, still clouded, is not yet traversed by the capillary vessels which afterwards form in it. From the thirtieth to the sixtieth hour their volume continues to increase, but the fulness of the arteries and veins partly conceal them.

As I have already said, the nature of these little bodies as well as their use was entirely unknown to me; but on hearing the clear and precise demonstration which M. Bernard has given of the glycogenic cells or glands of the placenta, I have no longer any doubts but that these bodies were their analogues in the class of birds—a class in which the placenta is represented by the umbilical membrane in part and partly by the allantois.

If the analogy of these bodies be established, shall we not be justified in saying that there exists in birds a diffused hepatic organ or a transitory liver analogous to that of which M. Bernard has demonstrated the existence in the placenta of the ruminantia?

In submitting these observations to our colleague, I will take occasion to remark that those which he has presented on the retarded action of the ordinary liver in ruminants are perfectly established in birds. Although in that class the liver appears as a double diverticulum of the intestinal canal at the end of the third day, nevertheless the corresponding vascular system of the portal vein is not developed till much later. From the slow formation of the structure of that organ, it is not till towards the eleventh or twelfth day of the formation of the chick that it is in a condition to enter upon its functions. Now it is just at this epoch that the umbilical membrane and its system of blood-vessels is replaced by the allantois, on the surface of which we do not see the glycogenic glands.

In relation to general embryogeny, one of the consequences of the discovery of M. Bernard is to establish, as he has done, that in the course of embryonic life there exist two glycogenic organs: the one transitory, residing in the placenta; the other, permanent, which is the hepatic organ. He proves in this manner the continuity of the glycogenic process in the blood through the entire duration of intra-uterine life.

Applied to the normal development of the embryo, this view is very just; but in the abnormal condition, when a degraded embryo without an hepatic organ and with a placenta sometimes so rudimentary that it scarcely equals the hundredth part of an ordinary placenta, how does the glycogenic function *then* become established? We know that in these abnormal beings,

which through their frequency constitute the greater portion of monstrosities by defect—we know, we say, that they are all destitute of liver, of heart, and of head, and that their placenta is extremely reduced in its dimensions. In this condition their existence would be incomprehensible if Nature did not supply that placental imperfection. She supplies it, then, by transforming the tegumentary covering of the acephalous monster into vast pouches filled with a sero albuminous liquid, and the walls of which are covered with a network of capillary, venous, and arterial vessels—vessels communicating by special trunks with the general vascular system of the body. Beyond this arrangement, so favorable for supplying the imperfection of placental respiration, the interior of these pouches is lined by a membrane of a serous nature, beneath which are found certain round yellow bodies, sometimes forming little patches by their union: are not these bodies glycogenic glands? In an acephalous monster, plates of which I have caused to be made in the work which appeared serially in Vol. XXV., of the Memoirs of the Academy, the subscapular pouches each contain more than eighty of these bodies, the posterior scapular pouches contain each from thirty to forty, and the axillary sinuses have from fifteen to twenty; the dorsal and inguinal pouches likewise possess them, but in smaller numbers.

It is needless to add that before the communication which we have just heard, I was entirely ignorant of the nature and use of these bodies. Such are the observations which I desire to submit to the attention of our colleague, M. Bernard.

[*Note by the Translator.*—The communications of MM. Bernard and Serres reminded us of the ancient nomenclature, in which the placenta was called the Liver of the womb, *Jecur Uterinum*. Thinking that we might find something pertinent to the present subject, we turn to our old books, and were interested at discovering that the term was used not merely with reference to a fancied resemblance in form, texture, etc., between the placenta and the liver, but with the belief that the placenta actually performed functions during foetal life which were afterwards assumed by the liver.

We have before us a copy of the "Anatomy of Human Bodies, etc., by Isbrand de Diemerbröck, Professor of Physic and Anatomy in Utrecht. Translated from the last and most correct and full edition of the same, by William Salmon, Professor of Physic. London, 1689." The original work of Diemerbröck was published in Latin about the middle of the seventeenth century.

In commencing the history of intra-uterine life, he gives the first place to the placenta, speaking of it under that name which

he says was first assigned to it by Fallopius, but adding, "Others, from its resemblance in use, color, and substance, call it the *Uterine Liver*." After giving a very satisfactory account of its form, texture, growth, and connection with the umbilical vessels, he enters upon the uses of the organ, which portion of his subject he opens by the following quotation from the illustrious Harvey: "*The placenta concocts the nutritive juice coming from the mother for the nourishment of the birth.*" Wherein this concoction consists, according to the theories of humoralism and vital spirits prevalent in those days, it would not profit us to inquire; but the description he gives of it is precisely the same as he assigns to the liver in extra-uterine life, so that "the uterine liver performs that function alone, which in men born the liver and spleen perform together. . . . Those bowels, therefore," [the liver and spleen,] "not being able" [to perform these functions,] "by reason of their tender constitution, provided nature has substituted in their place, for the time, a uterine liver, which supplies the office of both from the time that the blood begins to flow from the birth through the umbilical arteries into the uterine liver till the delivery;" adding, afterwards, that another reason for this "*concoction*" taking place in a different organ is, that "the *fermentaceous liquor* that is to be mixed with it [the blood] ought to be less acrimonious, and, by the same consequence, ought not to be prepared and concocted in the liver and spleen as in man born, but only in the uterine placenta, to the end it may be more mild and temperate when it enters the birth."

The function, then in the performance of which the *placenta* or *uterine liver* anticipates during foetal life the functions of the liver is the *concoction* of a *fermentaceous liquor*, which for some reason or other, Diemerbröck does not know why, (nor, for that matter, does Bernard or Serres,) must be constantly mixed with the blood in order to keep up its nutritious properties.

Of course the *discovery* of M. Bernard is not the less original or valuable for having been anticipated some two hundred years by an *opinion* of Diemerbröck. Discoveries in the present day are grounded upon industrious research, multiplied experiments and observations, and rigid induction—whereas the opinion we have exhumed is based upon nothing but the vaguest conjectures suggested by slight physical resemblances between the liver and the placenta. But if these old anatomists only conjectured where the moderns demonstrated, it must be admitted that their guesses were sometimes wonderfully happy; and for such a brilliantly successful guess as this, the name of Diemerbröck, now a fact fading from the memories of medical philosophers, shall be heard once more before it sinks in the limbo of things forgotten; his *fermentaceous liquor* shall be heralded side

by side with the *glycogenic matter* of modern physiology, and the placenta shall again be known as the "LIVER OF THE WOMB."]

Dropsy and Albuminous Urine: A Clinical Lecture, delivered, at the Baltimore Infirmary, June, 1859. By Professor CHARLES FRICK, M. D., &c.

DURING the past few weeks, gentlemen, there have been several cases of disease admitted to the wards, having certain features in common, but which nevertheless depend on different pathological conditions: these are dropsy and albuminous urine. And as the material is sufficiently abundant, I propose to speak to you of the relation they bear to each other, and of the various alterations with which they are associated.

In the first place, I must tell you that dropsy is an effusion of serum from the bloodvessels, into the cellular tissue, or one of the serous cavities, or both; and that it is produced by two causes. The one, a mechanical impediment somewhere in the course of the vessel, whereby the blood is prevented from returning to the heart, which thus becomes engorged to such a degree, that the serum soaks out, to relieve the distension; and the other, an alteration in the composition of the blood, which has become more watery, and readily escapes into the surrounding textures, more particularly as the coats of the vessel have undergone some alteration, in consequence of their being improperly nourished. Now, these two causes combined—that is, mechanical obstruction and thinning of the blood—when they exist together, are almost certain to occasion dropsy, although either may do it separately. And, as we shall see presently, when any pathological condition exists in which these two are present to a decided degree, then are we most apt to have a large amount of dropsy. But you are also aware that fluid is poured out as the result of acute inflammation. You have remarked occasionally, that common plegmonous inflammation gives rise to pitting from cedema of the surrounding cellular tissue; and you have had ample opportunity to observe the frequency with which a liquid effusion attends an attack of pleurisy. Now, in these cases, neither of the conditions I have just alluded to, are necessarily present; the blood is not altered, nor is there any mechanical impediment to its return. But one circumstance, however, is particularly remarkable. The fluid does not consist of serum alone, as in the first instance, but of liquor sanguinis; that is, serum, with fibrin in addition; the latter undergoing coagulation, and producing the fibrinous

bands and adhesions, so commonly a result of acute inflammation, and which no doubt serve, in many instances, to repair in a measure the injury inflicted by the disease. I say that the effusion in one instance is serum, and the other, liquor sanguinis. This is not invariably true, but is so commonly correct, that practically we may assume it always occurs.

The effusion, then, in these two cases, is entirely different; and without attempting to enter too deeply into the explanation of this difference, I will merely say, that if the accident in the first case is attributable to mechanical causes principally, independent of the bloodvessels; in the latter, the bloodvessels alone are concerned. In ordinary dropsy, the serum escapes, because the blood is thin, and is prevented from returning to the heart; while in an inflammatory effusion, liquor sanguinis is poured out, because the capillary vessels of the part are the seat of a morbid accumulation, from some cause inherent in the part itself. And I beg you to bear these two distinctions clearly in view, for otherwise you will scarcely have a just appreciation of the two different pathological conditions—that is, dropsy on the one hand, and inflammatory effusion on the other. It is true, you cannot always say which of the two is present in a case under consideration, yet, nevertheless the principle is correct, and generally the one can be recognized from the other with tolerable certainty.

But to return to dropsy. If you will bear in mind that a serous effusion, whether in a serous cavity, or in the cellular tissue, is due to mechanical obstruction and impoverished blood, you will realize that when the dropsy is local, the cause must be local; and if general, the cause must be general. Thus, aneurism of the popliteal artery produces œdema of the leg below the knee; femoral aneurism of the whole leg; and an enlarged uterus, by the pressure it occasions upon the iliac veins, will oftentimes be attended by dropsy of both lower extremities. These are instances of simple mechanical pressure. Again, if the obstruction be seated somewhere in the heart, so that the lower cava is interfered with, the whole of the lower part of the body is involved; while a thoracic aneurism, pressing on the vena innominata, has more than once given rise to dropsical swelling of the upper portion of the body alone; and you can understand also, that if the portal vein, which you know carries the blood of the intestinal canal through the liver, be obstructed in its course, the resulting effusion must make its appearance in the peritoneal cavity. These, too, are examples principally of mechanical pressure. As an illustration of dropsy occurring in persons whose blood is more watery than in health, you will observe that women whose constitution is weak, and whose tissues and muscles are flabby, are prone to have their legs

swollen after standing or walking, and that this puffy condition is always aggravated towards the close of the day. But, as I said just now, dropsy, to any extent, most often depends upon the two conditions combined.

Now, the explanation of the dropsy resulting from kidney disease is not so obvious as others. You are aware that a large portion of the kidney is made up of small tubes, communicating with bloodvessels, and these in turn with the malpighian bodies, which are so largely distributed through the cortical portion of these organs. And you can readily understand, that if structural alteration has taken place in these tubes, so that the circulation through the gland is obstructed, they must become more or less congested, and serum will be poured out, which, mixing with the urine, passes off by the bladder, and its albumen may then be coagulated by heat and nitric acid. And the same result will take place from congestion occurring from other causes. This serous drain, then, in a very short time, has a decided effect in deteriorating the blood, rendering it thinner and more watery; and as the obstruction to the blood in the kidneys, from the proximity of the renal arteries to the aorta, would be very soon felt in the circulation generally, we would have presented the two conditions most likely to occasion dropsy, namely, deteriorated blood, and interference with its transit through the vessels. But there is this peculiarity about renal dropsy. It usually makes its first appearance in the looser portions of the cellular tissue, as the eyelids, the scrotum, &c., or in those places where it meets the least resistance, and which are most distensible. And now you will understand why it is that general dropsy must be due to some general cause, interfering with the circulation; why cardiac dropsy should be first evidenced in the lower extremities; that from diseased liver in the peritoneal cavity, and renal dropsy in the looser portions of the areolar tissues. So much, then, for dropsy.

I have just said, that when the circulation through the kidneys is interfered with, serum escapes with the urine, and its albumen may be detected by heat and nitric acid. And if, in addition to this obstruction, the blood is deteriorated, either from the drain of albumen constantly going on, or from causes seated elsewhere than the kidneys, the escape of serum is more decided, and its albumen more abundant. You will readily appreciate, then, that albuminous urine does not always denote disease of the kidneys. It is often occasioned by heart disease, and may be produced by pressure on the renal veins; and I need scarcely remind you, that if the urine contains pus, the albuminous liquor in which the globules float, is coagulable by heat. When, therefore, albuminous urine is present, you cannot necessarily diagnose renal disease; for this is to be made

out by other phenomena, and by the assistance of the microscope, of which I shall have more to say to you another time.

Now let me endeavor to show you, in a few words, the application of the principles I have thus far laid down to several cases of disease which we have had under care within the past few weeks.

CASE I.—The first is the man who entered the Infirmary April 28, and left on May 20, named Lynch. I found him much emaciated, and very pallid. His legs and scrotum very œdematous, and there was a considerable amount of fluid in his abdominal cavity. He had very decided dyspnœa, and coarse râles could be detected over both lungs posteriorly. His pulse was weak, intermittent, and irregular, and beat one hundred and eighteen times in the minute. His urine very high colored, of a sp. gravity of 1.023, and amounted to thirteen ounces in the twenty-four hours. Both heat and nitric acid threw down a copious precipitate, but under the microscope nothing was perceptible but a few amorphous crystals of uric acid. On examining his heart, I detected a loud, rough murmur, taking the place of the first sound, and most evident towards the apex of the heart. He told us that he had had rheumatism some years since; that for some time he had labored under difficulty of breathing, particularly in mounting ascents; and that the dropsy, which first made its appearance in the lower extremities, had commenced about four months previously. Now what was the alteration in this case? Manifestly an insufficiency of the mitral valve, allowing regurgitation. And I recognised it in this way. The heart, in contracting, forces the blood through the aortic and pulmonary orifices, and at the same time, the auriculo-ventricular valves should close, so as to prevent the blood passing from the ventricles into the auricles. And synchronous with this, the first sound occurs. A murmur dependent on valvular alteration occurring with the first sound, must therefore be due either to stricture of the aorta or pulmonary artery, on the one hand; or to an insufficiency in the mitral or tricuspid valves on the other. As it was more evident towards the apex of the heart, I concluded it was the latter; and there was no regurgitation in the veins of the neck; and as disease of the left side is more common than the right, I assumed it to be in the mitral valve. Now, in what way are the dyspnœa, the dropsy, and the albuminous urine dependent on this condition? In the first place, if the blood regurgitated into the left auricle, this would be first distended, then the pulmonary veins, then the pulmonary artery, and in turn, the right ventricle, the right auricle, and the cavas. The distention in the pulmonary vessels would give rise to the dyspnœa, and in time the obstruction of the cavas, particularly the lower, from its having gravity

to overcome, would become more distended, and allow the escape of serum into the cellular tissue of the legs first, and subsequently into the abdominal cavity. The same cause would produce congestion of the kidneys through the renal veins, and the serum thus poured out would pass off by the kidneys. That the albuminous urine did not depend on structural alteration of the kidneys, I was satisfied; partly because there was no evidence of such a condition revealed by the microscope, and partly because the whole phenomena could be explained by the cardiac affection. The indications for treatment were very plain; to lessen the amount of fluid in the circulation generally, either by the skin, the kidneys, or the bowels, and to support the strength. Accordingly, he was ordered squill, assafoetida, and the oil of juniper. Under this treatment, his urine was increased to twenty-three ounces, but with no marked benefit. He was then ordered sal rochelle, which occasioned two or three loose serous discharges daily, under which his dyspnœa subsided, and his dropsy sensibly diminished. He left us, however, in the course of a few weeks, his condition more comfortable, but not materially improved.

CASE II.—The second case is at present, under treatment, and is of more interest. Lewis Snyder, aged 45, in St. Vincent's Ward, entered the house June 7th. He is a pallid man, and says he was not sick until six weeks before his entrance. Has never had rheumatism, but admits for some time past he has had difficulty in mounting ascents. He had no dropsy, his dyspnœa was extreme, and his pulse small, contracted, irregular, and one hundred and twenty-eight. His urine had been very copious, but latterly has undergone a marked diminution, and was at entrance seventeen ounces, high colored, specific gravity 1.022, and highly albuminous. Dullness over his heart much increased, and the pulsations very vigorous, although irregular. The apex is felt in its normal position, but there is also distinct pulsation at the ensiform cartilage. Above the sternum there is no unusual pulsation, nor can any blowing sound be heard any where in the cardiac region. There was nothing unusual in his lungs, except râles throughout, mixed also with dry sounds. A microscopic examination of the urine revealed nothing of importance.

Now what is the diagnosis in this case? The most marked symptom was the difficulty of breathing, produced evidently by swelling in the membrane of the bronchial tubes, and the secretion poured out in them as a consequence. This was scarcely catarrh; it did not commence as such attacks do, and moreover, was associated with a very irregular action of the heart, and a kind of dyspnœa, which I have more than once pointed out as belonging rather to the heart than the lungs. That the right

ventricle is hypertrophied in this case, is proved by the increased dullness and pulsation at the ensiform cartilage, while at the same time its apex is found in the normal position. For although the heart may be displaced to the right side by various causes, so that the apex may be felt at the ensiform cartilage, yet in such cases, there will be no pulsation in the normal position. The right ventricle, then, is enlarged, but will this explain the difficulty? I think not. Regurgitation of the tricuspid or mitral valves, would give rise to a decided blowing sound, which there is not; while contractions of the tricuspid would be more likely to occasion dropsy, and pulsation of the jugular veins. But assuming that there is contraction of the mitral valve, the solution is easy. In the first place, the blood passes from the auricle to the ventricle, partly by the contraction of the first, and partly by the dilatation of the latter. Nor is it impelled with sufficient force to give rise to a murmur, while the healthy semilunar valves, flapping back into their place, are quite sufficient to occasion a clear second sound. If, then, the valve be contracted, the usual amount of blood is not received in the left ventricle, nor sent by the aorta to the various arteries. The pulse, therefore, was small and contracted. In the second place, the auricle must have been unnaturally distended, and thus an excess of blood was accumulated in the pulmonary vessels and the lungs. This explains the dropsy. This accumulation would be felt more or less throughout all the organs, the kidneys among the rest, and the albumen found in the urine was but a result of these organs relieving themselves of the superabundant serum thus forced upon them. So much, then, for the diagnosis.

The indication for treatment was manifestly to relieve the engorged vessels by some drain, which would at least afford temporary relief, and at the same time to support the general strength. Accordingly, he was ordered

Assafoetidæ, gr. j.

Scillæ pulv., gr. iij.

Ol. Juniperis, gtt. $\frac{1}{2}$, every 4 hours.

I did not prescribe digitalis, veratrum viride, or any similar agent, although the pulsations were both too frequent and too forcible, because I considered these were but evidences of the degree to which the heart was obliged to labor to overcome the impediment; and a sedative of this character might lower the frequency and moderate the impulse, but it would unquestionably render the heart more irregular, and increase the dyspnœa. In two days after the commencement of the treatment, our patient was able to lie down, and his cough had much diminished. In a week all difficulty of breathing had disappeared, the pulse

became fuller, more infrequent, and perfectly regular. The abnormal impulse of the heart had altogether subsided, and his strength had decidedly improved. I would recall to your recollection the fact, that in twenty-four hours the quantity of urine had increased to thirty-five ounces; the next day to fifty-six, and has continued between fifty and sixty ounces up to this time. The albumen, too, has notably diminished, and latterly I have ordered him sulph. iron and quinine, for the purpose of improving his strength.

You will remark that, in both of these cases I have endeavored to drain off the fluid from the kidneys, in preference to the bowels, and it is for this reason: When liquid discharges from the bowels are produced, it is always a serous fluid, containing various other matters; and as this serum is derived from the blood, such a drain, continued for any length of time, must necessarily weaken the patient to a very great degree, and thus tend to keep up the very causes which originally produced the disturbance. The kidneys, on the contrary, drain off principally the water, and unless the specific gravity of the urine thus voided is high, it can scarcely be said that any material waste is going on. Do not understand me to imply that waste, and very great waste, may not be produced by diuretic remedies. On the contrary, when the agents are properly selected, as, for instance, the alkalis in rheumatism, very decided emaciation may be produced by their use in a short time. You have more than once seen rheumatic patients in these wards, waste most rapidly under the use of large quantities of bicarb. potass., and have remarked at the same time, that the urine, instead of containing six hundred or seven hundred grains of solids, had increased to one thousand two hundred, and even one thousand eight hundred in twenty-four hours; and this for days together. Indeed, herein lies, according to the views I hold, as I have more than once told you, the efficacy of these salines in rheumatism. Not, as has been assumed, in neutralizing some assumed element, but in producing general waste, and of course wasting the rheumatic elements, whatever they may be, at the same time. But what I desire to explain to you now is this: When the object is to produce an alterative effect, as in rheumatism and analogous diseases, and the kidneys are the means selected to eliminate the morbid substances, the solids are of every importance, the fluids in which they are dissolved of little or none. Thus, eight hundred grains of solids in such cases is a decided improvement over four hundred; but whether the eight hundred grains is dissolved into twenty or forty ounces of water, is of secondary importance. On the contrary, when it is desired to obtain a diuretic action, as in heart disease or dropsy, the fluid is all important, the solids less. Here, then, a material

point is gained, if the urine is increased from twenty to forty ounces; but different from the other case, it is much preferable that the solids should remain at four hundred grains, rather than be increased to eight hundred, for there is no object in producing waste of tissue. For this reason, colchicum, the salines, &c., are preferable in the first instance, but squill, juniper, and similar agents in the latter. And in the patient to whom I have just referred, although his urine was increased from seventeen ounces to sixty, yet the specific gravity fell from one thousand and twenty-two to one thousand and nine; so that the waste of solids was not materially increased, and he therefore, under good diet, has improved in strength.

Still more interesting than the two cases I have spoken of to-day, are three patients laboring under dropsy and albuminous urine, dependent on kidney disease; but these I must reserve for another occasion.—[*American Med. Monthly*.

Treatment of Asthma by Stimulants.—*Theory of the Modus Operandi of Stimulants.*—*Illustrated by Coffee.*—*Curative Influence of Violent Emotion.*—*Its Action analogous to that of Stimulants: Acts also as a "Nervous Derivative."*—*Cases.* By HYDE SALTER, M. D., F. R. S., Assistant Physician to Charing Cross Hospital.

ONE of the commonest and best-reputed remedies of asthma, one that is almost sure to have been tried in any case that may come under our observation, and one that in many cases is more efficacious than any other is strong coffee. To the question, "Have you tried strong coffee?" the asthmatic is pretty sure to answer "Yes;" and he is also pretty sure to add that it gives him relief.

About the *modus operandi* of this remedy, I was long puzzled; I could not make it out; and it is only lately that I think I have stumbled upon it. The *rationale* of its efficacy is, I think, to be found on the one hand, in the physiological effects of coffee—the particular nervous condition that it produces—and, on the other, in a feature in the clinical history of asthma which I have long observed, and of which I think the efficacy of coffee is highly corroborative.

This fact is, that *sleep favors asthma*—that spasm of the bronchial tubes is more prone to occur during the insensibility and lethargy of sleep, than during the waking hours, when the senses and the will are active. I have already referred to this in my observations on the "Clinical History of Asthma,"* in explaining

* *Medico-Chirurgical Review*, July, 1858 and 1859.

why the paroxysm invariably, (or almost invariably) chooses the hours of mid-sleep for its onset. Let me just refer to this subject again; for it is both interesting and important, as it explains a curious and very constant phenomenon in asthma—the hour, namely, of the attack—is highly illustrative of its pathology, and furnishes the key to some of its treatment.

I think, then, that sleep favors the developement of asthma in two ways:

1st. By producing insensibility to respiratory arrears.

2d. By exalting reflex action.

The way in which sleep favors the development of asthma, by producing insensibility to respiratory arrears, and exalting reflex nervous action, I have already sufficiently explained in the papers on the Clinical History of Asthma, to which I have referred.

There can be no doubt that sleep *does* exalt reflex nervous action. It is a fact, so abundantly inculcated by the history of disease, as hardly to require illustration or proof. The phenomena of epilepsy, cramp, lead tremors and other examples of deranged muscular action, all teach it. It is just as sleep comes on, just as the will is laid to rest, or during sleep, that these different forms of involuntary muscular contraction must commonly occur. Any one, to convince himself of it, has only to fall asleep sitting on the edge of a chair, in such a position that it shall press on his sciatic nerves. As long as he is awake, his legs will be motionless; but the moment he falls asleep they will start up with a plunge and suddenly wake him. As soon as he is awake they are quiet and still again, with no disposition to start, till he again falls asleep, and that moment they start again and wake him; and so he may go on as long as he likes. He changes his position, sits back in his chair, and they start no more. I need not explain what so clearly explains itself. I heard, some years ago, of a case of what might be called chronic traumatic tetanus, in which the source of irritation—the excito motory stimulant—was extensive disease of the hip-joint. The moment the patient fell asleep he was seized with opisthotonos, which, of course, immediately woke him. On awakening, the tetanus vanished; on again falling asleep, it reappeared; and this alternation of falling asleep and waking continued for weeks, if not for months, the patient getting no continuous rest till he was quite worn out. As long as he was broad awake, the tetanus never appeared.* Hosts of similar facts, illustrative of the same truth, might be cited.

Anything that exalts, reflex nervous action, increases, of

* I was further informed, respecting this case, that, after everything else had failed, sleep was procured, with an immunity from the tetanic spasms, by putting the patient into the mesmeric state. In this way he got rest, and greatly improved; but what was the ultimate issue of the case, I do not know.

course, the potency of reflex stimuli. Now, I have elsewhere endeavored to show that the phenomena of asthma are, in almost every case, those of excito-motory action, and that the exciting causes of asthma are, in the great majority of instances, such as act by a reflex circuit. They would, therefore, on the asthmatic's falling asleep, immediately acquire a potency they did not before possess, just as the pressure on the sciatic nerve did, in the illustration I have given. Thus it is we see that the asthmatic may gorge himself with unwholesomes, and yet, as long as he keeps himself awake, suffer no consequential asthma; the irritant is there, the undigested food is in the stomach, but as long as he is awake, as long as the will is dominant, it is inadequate to the production of reflex phenomena. But let him fall asleep, and in an hour or two the paroxysm will be established.

And not only will *sound sleep* determine, by this exaltation of reflex susceptibility, the production of asthma by its exciting causes, but a small dose of the same condition—sleepiness, drowsiness—will favor the supervention of asthma in a proportionate degree. Not only is drowsiness a premonitory sign of an attack, but a powerful predisposer to it; and the asthmatic knows that he yields to it at his peril. I have often noticed in asthmatics, that the sleepiness that is so apt to come on after dinner, will be accompanied by a slight asthmatic oppression and wheezing: as the drowsiness deepens, so does the asthma, and in this way it may settle down into an attack; but if the patient rouses himself, or if anything occurs to engross his attention so as to wake him up, broad awake, the asthma quickly vanishes. It is in this way, I think, that is to be explained the fact, that asthmatics can dine out late and unwholesomely with impunity; while, if they dine at the same time and in the same way at home, asthma is sure to come on. At home they want that excitement which at a dinner party, keeps the animal functions in a state of exaltation and the mind vividly awake, and effectually banishes the least approach to drowsiness. Of the fact, there is not the slightest doubt. I know an asthmatic, who can with impunity, dine out at seven o'clock, as dinner-eaters of the nineteenth century are apt to dine—shirk nothing from soup to coffee—walk home at ten o'clock, a distance perhaps of four miles, with the wind of a deer-stalker—go straight to bed, and get up the next morning scathless; but if he were to dine at home at six, or even at five o'clock, he would be wheezing at nine, and by four the next morning, downright asthmatic.

I believe a certain amount of the curative influence of fright, or other strong mental emotion, is to be explained in the same way.

"But why," it may be asked, "all this round-about digression? What has all this to do with the curative influence of coffee?"

I believe it is simply its explanation. For, what are the physiological effects of coffee? They consist in the production of a state of mental activity and vivacity, of acuteness of perception and energy of volition, well known to those who have experienced it, and to a certain extent very pleasurable, and which is the very reverse of that abeyance of will and perception, which, in drowsiness or sleep, so favors the development of asthma. In sleep, will and sense are suspended; after taking strong coffee, they are not only active, but exalted. It produces rapidity of thought, vivacity of spirits, clearness of apprehension, increases tenfold the working powers, and altogether intensifies mental processes. Not only is there no disposition to sleep, but sleep is impossible; the thoughts hurry one another through the mind; the bodily movements are energetic and rapid; and if the effects of the drug are pushed far, a very unpleasant condition is produced, something like that of delirium tremens, *minus* its hallucinations. Now, if the suspension of the will, or its depression, favors the production of excito-motory phenomena, and thus favors the development of asthma, is it unreasonable to suppose that its exaltation should prevent or cure it? It *must* do so—if not positively, at least negatively, by removing the predisposing condition. And bearing in mind this marked physiological effect of coffee—that this exaltation of the animal nervous functions is exactly what it produces—it certainly does seem to me reasonable to suppose that this is its *modus operandi*. And if of coffee, then of strong tea, and alcohol, and ammonia, and ether, and other stimulants of undoubted value in asthma.

To show that this is the *rationale* of the cure of asthma by stimulants, I do not think it is necessary to show that it is only when the asthmatic is drowsy, or has been sleeping, that they do good. If anything that rouses the asthmatic to a state of wakefulness, will put a stop to asthma, that was creeping on him while he was sleeping or sleepy, *à fortiori* anything that carries him beyond a state of mere wakefulness—that gives him an active, not a mere passive wakefulness, will be still more efficacious, and will be adequate to the checking of an attack, that, in spite of his being broad awake, was gaining on him.

The very frequency with which coffee gives relief, makes it hardly worth while for me to narrate the history of any cases. I should think, from my own experience, that coffee relieves asthma in two-thirds of the cases in which it is tried. The relief is very unequal, often merely temporary, and sometimes very slight; sometimes it is complete and permanent. It is often taken in the morning; and patients will tell you that, previous to taking their coffee, they are not fit for anything, can hardly move about; but that taking it is immediately followed by freedom of breathing, and an ability to enter at once on their daily occupation.

There are two or three practical hints with regard to the administration of coffee, that are worth bearing in mind.

1. It cannot be given too strong. Unless sufficiently strong to produce its characteristic physiological effects, it does no good, but rather harm; moreover, if given very strong, it need not be given in much bulk, and quantity is a disadvantage—its effect is less rapid, and it oppressively distends the stomach.

2. I think it is best given without sugar or milk—pure *café noir*.

3. It should be given on an empty stomach; if given on a full stomach, it often does great harm, by putting a stop to the process of digestion; indeed, so much is this the case, that I consider coffee accompanying a meal, especially late in the day, so peculiarly apt to induce asthma, that it deserves to be classed among its special provocatives. I have mentioned elsewhere the case of an individual who never dared to take the usual after-dinner cup of coffee—it would make the simplest dinner disagree with him. But the same asthmatic found in strong coffee, on an empty stomach, one of the most valuable remedies.*

4. For some reason or other, I don't know why, it seems to act better if given hot—very hot.

I adverted just now to the influence of mental emotion on asthma, and stated my belief, that the *modus operandi* was, like that of coffee or other stimulants, by producing an exaltation of sense and will—an intense activity of the intellectual part of nervous action—and proportionately lessening the tendency to excitation; and this it does to a much greater degree than stimulant remedies, and its effects are, therefore, proportionately more sudden and complete. It was indeed, the curative influence of violent emotion, and the observation that it and coffee-taking, alike banish that condition in which asthma is most prone to come on, that first suggested to my mind the theory of the action of stimu-

* Since writing the above, I have received the following account from an asthmatic gentleman, singularly confirmatory of my own observations:

"I used to think," writes my informant, "strong coffee the best of all remedies. I remember one instance especially, only a pattern of many others, but more striking when told. With bent back, high shoulders, and elbows fixed on the chair arms, I had been laboring for breath all the afternoon. About five o'clock I had two breakfast cups of strong coffee. The hard breathing disappeared rapidly and completely. My sisters were dancing in the next room, and in less than an hour I was dancing with them, quite free from asthma.

"Of late, coffee has often had an opposite effect upon me. The after-dinner cup of coffee, to which I have been for several years habituated, now produces a sensation of stuffing of the chest, and incapacity of moving about. I believe this is because it stops digestion; and the reason I did not suffer for some years, I take to be, that my originally most excellent and enduring stomach, could stand it so long and no longer. Coffee, on an empty stomach, I still deem a most valuable remedy. I do not share the prejudice against putting milk and sugar into coffee that is used as a medicine, provided that it remain *café noir* and be not made *café au lait*."

lants on asthma, that I have just endeavored to propound. I think, too, that mental emotion acts, if I may so express it, as a nervous derivative. There are many phenomena, both in health and disease, that seem to show that only a certain amount of nervous activity can be in operation at a certain time; and that, if a nervous action of one kind comes into operation, another that had been previously going on is immediately depressed or arrested. Such is the explanation of the well known experiment of the two dogs, one of which was taken hunting immediately after a meal, while the other was allowed to sleep. In the one that was taken hunting, digestion, on its return, was found hardly commenced; in the other it was completely over, and the stomach empty. In the sleeping dog, the whole vital dynamics, not being otherwise employed, were appropriated by the function of digestion; while in the hunted dog, they were entirely taken up by its energetic locomotion, and drafted away, as it were, from that nervous superintendence of digestion without which the function cannot be carried on.* The power of strong emotion, or hard study, in retarding digestion is an analogous fact. Just in the same way, I think, the extraordinary activity and exaltation of thought and perception, that characterize the state of mind that the taking of coffee, ether, and other stimulants produces, acts as a nervous derivative in asthma, and diverts from the nervous system of the lungs, that morbid activity which engenders the spasm of the bronchial tubes.

The cure of asthma by violent emotion, is more sudden and complete than by any other remedy whatever; indeed, I know few things more striking and curious in the whole history of therapeutics. The remedy that stands next in speed and efficacy—tobacco pushed to collapse—takes time, a few minutes at least; but the cure of asthma by sudden alarm, takes *no* time; it is instantaneous, the intensest paroxysm ceases on the instant. This is a fact so little known, as far as I can see, and yet so practically important and theoretically interesting, that I think it will not be unprofitable if I endeavor to impress it more deeply by the narration of some cases of its occurrence.

CASE I.—A gentleman suffering an unusually severe attack, so bad that he had been unable to speak or move all day, was suddenly alarmed by the illness of a relative; he ran down two flights of stairs and up again, and administered the restoratives he had procured, and then observed, to his astonishment, that his asthma was gone. This gentleman tells me, that on many other occasions, different forms of mental emotion have cured his asthma.

CASE II.—C. R., a confirmed asthmatic, states that, when he

* See Dr. John Reid's experiments in Todd's *Cyclopædia of Anatomy*, vol. iii., p. 899; also those of Bernard and of Bishoff, in Müller's *Archiv*. 1843.

was suffering from an unusually severe attack, a fire occurred just opposite his house. Previous to the occurrence of the fire, he was in bed, breathing with the greatest difficulty, and unable to move. When the excitement of the fire was over, he found that he had been standing in his night-shirt, looking with the rest, out of the window, and that he had quite forgotten all about his asthma. On another occasion, when he was suffering from an attack, some sudden anxiety arose about two of the members of his family being out late; the alarm from which he suffered, relieved his asthma, but not so suddenly as in the case of the fire. On another occasion, a sister of his was seized with sudden illness that seemed to threaten suffocation; he was suffering severely from asthma at the time, and was in bed; he jumped out of bed in great alarm, and found then that his asthma was perfectly cured. He was sufficiently well to run for a doctor, and continued well throughout the day.

CASE III.—Not long ago I was informed by a patient at the hospital, who had suffered greatly for many years, that, however severe an attack might be, venereal excitement would almost invariably cure it. He told me also, that, when a youth, he had been guilty of the practice of onanism, and that the unnatural excitement thereby produced, had just the same curative effect on his asthma. Indeed, he pleaded this effect of it as a sort of excuse for the practice; and assured me, that when his breath was very bad at night, he used to resort to it for the purpose of curing it.

I have known two or three cases in which sexual excitement has had just the same effect.

CASE IV.—The following account of the curative influence of mental excitement I have received from a medical friend, who has suffered from asthma all his life: "On one occasion I was sitting with fixed elbows on a sofa, breathing hard; a lady came into the room whom I had known very well, and whom I had not seen for several years. I got up to receive her, and sat down again on a music-stool; with no especial purchase, therefore, for the respiratory muscles, and yet with comparative ease of breathing. This ease lasted for about an hour, and then the difficulty of breathing came on again. I attribute the temporary amendment to the diversion of nervous energy. Just the same thing has happened to me more than once. On another occasion I was suffering a good deal at a farm house. I got on horseback with some difficulty, and an anxious hope that the horse would go quietly, to fetch myself an emetic from a town three miles off. The horse ran away with me. I pulled in, at first weakly and almost despairingly, but the need of exertion brought the power; after a run of about a mile, I succeeded in pulling up, and was delighted to find my asthma gone. Another time I was breath-

ing very hard, and a friend engaged me in an argument. At first I could only get out a sentence in successive gasps; but gradually, as I got excited, the hard breathing went off, and I could talk fluently."*

From the foregoing observations, then, I think we may conclude—

That, since the abeyance of the will favors, in proportion to the degree of the abeyance, that development of asthma, and since the effect of strong coffee is to dispel such suspension or depression of volition, and restore the will to its wonted, (or even an unwonted) activity, it is by thus exalting the will, and so disfavoring the development of excito-motory action, that this remedy relieves asthma.

That the same interpretation applies to the relief of asthma by all other stimulants whatever.

That thus strong coffee and mental excitement, although apparently so different, belong to the same category of remedies for asthma.—*Edinburgh Medical Journal, and American Med. Monthly.*

Oxalate of Lime in Urinary Sediments.

M. Gallois read a memoir on this subject before the Académie des Sciences, the purport of which is contained in the following résumé :

Oxalate of Lime is a substance that can be readily recognized in the urine of the healthy human being, at all ages. It appears in considerable proportion under the influence of certain kinds of food, and probably of certain medicines. It is very commonly met with in the urine of the sick, but excretion does not constitute, in itself, disease. Oxaluria is thus not a morbid entity, but only a symptom common to affections very diverse in character. Nevertheless it is proper to say that oxaluria has been observed very often in spermatorrhœa, and in certain diseases of the nervous system, especially in dyspepsia.

There is a substance which very frequently accompanies oxalate of lime in urinary sediments, as well as in gravel and calculus, and this is crystalized uric acid. The very common co-existence of these two, seems to explain the formation of the oxalate in the system.

The connexion supposed to exist between oxaluria and diabetes, cannot be admitted:

The oxalic acid (and consequently the oxalate of lime,) ap-

* For additional cases of the cure of asthma by mental emotion, I must refer the reader to a paper on Pathology of Asthma, in the *British and Foreign Medico-Chirurgical Review*, for July, 1858.

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pears to be derived from the uric acid, and should be considered as a higher degree of oxidation of this body, or of the elements that form it, so that wherever uric acid or its elements are found in the body, oxalic acid can be produced under the influence of a higher oxidation, which is effected in the blood.

Oxaluria requires ordinarily no other treatment, than that appropriate to the physiological or morbid condition with which it is connected. Hence, the contradictory character of the medication heretofore proposed for its treatment: to abstain from food and medicine containing oxalic acid, to use small doses of nitro-chlorhydric acid in a bitter tonic infusion, or nitrate of silver, (in the sandy form of oxaluria,) or colchicum in some cases, or phosphate of lime, &c., &c.

Gallois is satisfied that *alkaline mineral waters* are the most *efficacious* in hindering the excretion of oxalate of lime, especially where there is a coincident deposition of uric acid, a condition which seems to him most frequent.—*Gaz. des Hôpitaux*, and *Ibid.*

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*Means of Eliminating Grains of Powder from the Skin.* By  
Prof. BUSCH, of Bonn.

PROF. BUSCH has taken the idea of this treatment from the method employed by M. Hebra, for freckles. This method consists in provoking, by applications of a strong solution of corrosive sublimate, and eczematous inflammation, after the cure of which the spots on the skin disappear. As the pigment in freckles is not in the epidermis, but in the *rete mucosum*, (rete Malpighii,) M. Busch concludes that in the development of the eczema, the epidermis not only is raised, but that the most superficial layer of the dermis is acted upon; and on this account, he has been led to propose this means for eliminating grains of powder from under the skin, when it is a recent case, and they are too numerous to be removed by an instrument.

In a case treated in this manner, M. Busch made use of a solution of corrosive sublimate, of the strength of five grammes (℥iv.) to a quart of water, the application of which, on account of the recent injury, was not continued more than an hour the first day; but the succeeding days, as the patient felt only a slight burning sensation, these applications were made successively during many hours, until the fifth day, when the whole injured part was covered with a considerable eczematous eruption. The applications were then suppressed, and the next day a part of the vesicles simply dried, while others produced their crusts. When one of these crusts was elevated, on its under surface,

grains of powder could be seen, and below these, an epidermis of new formation. As this new epidermis was very tender, M. Busch waited for twenty-four hours, then elevated with a spatula, all the crusts and scales of the epidermis, and at the same time a great number of grains of powder. In this manner, almost all the grains of powder which were situated between the new and old epidermis were detached, and were easily eliminated, with the exception of a small number, only appreciable by a very close examination, and which were situated deeper than the rest. Whether this method can be employed in old cases, experiment only can prove. M. Busch adds, that all agents strongly irritant can be employed for inducing this inflammation, in order to produce the elimination of the foreign body; but the sublimate solution is preferable, because the degree of irritation can be exactly graduated by it, and after the eczema it produces has disappeared, the skin preserves its normal color.—[*Gaz. des Hôpitaux*, from *Virchow's Archives*, and *Ibid.*

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#### *Treatment Preventive of the Sequelæ of Measles and Scarlatina.*

MANY precautions are adopted by physicians to prevent the unfortunate sequelæ of these diseases, and the confinement to the sick-chamber for several weeks after convalescence ranks among them. To avoid this, M. Scoutetten, of Mentz, has devised the following method, which we find in the *Gazette Hebdomadaire*, for April 1, 1859:

As soon as convalescence commences, that is to say, when the skin is no longer red with the eruption, he rubs over the whole body slightly warmed, oil of sweet almonds or olive oil, and puts the patient in bed again, for two hours. The next day he gives him a tepid bath for an hour, then places him in bed, and if the skin is very dry, a new friction with the oil is made. These two frictions and one bath are usually enough to remove all danger. Still, in severe cases, it is well, to avoid any risk, to repeat the means indicated from time to time, until the skin regains its suppleness. These precautions taken, convalescents may be permitted to go out without fear of bad results.

In order to justify this method and explain its importance, it is necessary to remember the state of the skin in infants affected with measles or scarlet fever. At the commencement of the disease, the dermis is red and swollen; during convalescence the tissues return to their normal condition, but the epidermis, which has been distended, not being elastic, becomes detached, and falls off in the shape of fine powder when the attack of



measles has been light, or is removed in large scales when it has been severe, and especially when the eruption has been that of scarlatina. The skin beneath is dry and harsh; perspiration and transpiration are badly performed, and the functions of this important organ are impeded or suspended. When the skin acts badly, the kidneys and the mucous membrane of the air-passages or of the digestive apparatus, undertake to supply its place; thence arises a thick, sedimentous, and sometimes albuminous urine, severe diarrhoeas, which terminate in emaciation and death; obstinate coughs, sore throats, croup, pneumonias, pleurisies with effusion; finally, serous infiltrations into the areolar tissue of the limbs, or accumulations of liquid in the abdomen, and in other cavities where serous membranes exist. These severe symptoms occur after an exposure to cold of the skin, which inflammation has rendered more sensitive, and the functions of which are interfered with by an inert epidermis, which obstructs its pores. The object of the treatment proposed by M. Scoutetten, is to oppose the causes of these symptoms.—*Amer. Med. Monthly.*

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*On the Treatment of Chronic, Organic Diseases of the Heart.* By  
Professor LEBERT.

THE treatment of chronic organic diseases of the heart, offers one of the most difficult tasks to the practising physician; it is nevertheless certain that a proper and thorough treatment of these diseases, may do a great deal toward alleviating the condition of the patients, and towards prolonging their lives. Prof. Lebert advises especially, great care with blood-letting, purgatives, and all debilitating measures in the treatment of organic diseases of the heart. In valvular disease, he employs venesection but very rarely. If in the course of the disease, an acute inflammation, in the form of pericarditis or endocarditis, occurs, it is often useful to extract blood to the amount of six ounces, by means of cups or leeches; if, after this, an energetic treatment is still necessary, such advantage will be obtained from the application of a large blister, and the endermatic use of half a grain of morphia daily. The author's observations on the use of digitalis we may omit, as generally known. Most allied to digitalis, in regard to its therapeutic effect, is aconite. Although it acts less heroically and more slowly than digitalis, and does not decidedly diminish the frequency of the pulse, aconite is nevertheless a remedy which is capable of lessening considerably the dyspnoea, palpitation, and the various subjective symptoms of the patient, even the tumultuous excitement

of the heart. A very important point to be considered in the treatment of advanced diseases of the heart, is the general cachexia and debility of the patient gradually developed. Lebert has examined for several years, the muscular structure of diseased hearts, in order to see how far the gradual decrease of the functional capacity of the same depends upon changes in the muscular fibre itself. From these investigations it results that very frequently a small degree of fatty degeneration of the primitive cylinders of the muscles of the heart exists, even in cases where the color and consistency of it does not indicate the fact. The gradual increase of general debility and of the local weakness of the heart, the progress of anæmia and hydræmia, lead to the question, what effect tonics, especially iron, would have in organic disease of the heart.

The better the patients are nourished (with the avoidance of strongly stimulating food,) the longer they resist the evil influence of the disease; the patient should, therefore, not be restricted to a vegetable diet, but a moderate amount of animal food should be allowed with it. Of beverages, tea, coffee, alcoholic liquors, and wine in large quantities ought to be avoided, whereas, infusion of cocoa, or decoction of roasted acorns, are very appropriate, particularly for breakfast. Light beer, or small quantities of old wine mixed with water, may be allowed at the table. Besides an amalystic diet, the use of iron is indicated, particularly in the later anæmic and cachectic period of the disease. Lebert recommends especially, iron reduced by hydrogen, (gr. ij. at each meal,) or twenty to twenty-five drops of the *tinctura ferri pomati*, if necessary, in connection with equal parts of tincture of aconite. The tartrate of iron and potassa (three to five grains three times daily,) is a very useful preparation; and as the effect of iron is perceptible only then, when its use is persisted in for a long time, it is necessary, occasionally, to make a change in the preparation employed. The author has not derived much advantage from the application of blisters, setons, and moxas.—[*Wien. Medizin. Wochenschrift, Mediz. Neuigk.*, and *Ibid.*

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*On the Use of Tannin in Large Doses in Albuminous Anasarca.*  
By Dr. GARNIER.

PURE tannin, tannic or gallic acid, was at first prescribed in hæmorrhages and diarrhœa, in pretty small doses, as it was considered a poison. Later, many practitioners recommended its use in nocturnal sweats, in diabetes, and in atonic diseases. Dr. Garnier, of Paris, was induced to try tannin in the albuminous

anasarca, consecutive to scarlatina, a disease which so obstinately resists ordinary measures. From the observations he has lately published in the *Archives Générales de Médecine*, it results that under the influence of large doses of this agent, the general infiltration of the cellular tissue, and the presence of albumen in the urine, have simultaneously disappeared. Dr. Garnier gives the following conclusions at the end of his treatise:

1. Tannin, in the dose of fifteen to thirty grains through the day, cures anasarca or œdema which has been passively developed, and co-exists with albuminous urine. 2. Its curative action manifests itself by copious urine, which gradually assumes its physiological characters, by perspiration, loose stools, etc. 3. These symptoms appear on the second day of the administration of the tannin. 4. Given in solution of about three to eight grains, tannin does not derange the digestive organs. 5. The action of tannin seems to be primitively directed upon the fluids of the economy, the albuminous principles of which it coagulates and plastifies; its action upon the solids seems to be consecutive, tonic, and astringent.

The facts reported by Dr. Garnier, are quite interesting; it is, however, important to remark, that tannin has been given only in cases of transitory albuminuria, following congestion of the kidneys, and that albuminous or granular nephritis, is not at all in the question. This distinction should be made, in order to appreciate therapeutic methods properly.—[*Archives Générales*, and *Ibid.*

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### *Virchow's Cellular Pathology.*

THE object of Dr. Virchow's work on Cellular Pathology is to establish a new foundation for the creation of a Philosophical Medicine. Dr. Virchow desires to put an end to all the one-sided theories now in vogue—to the theories of the humorist and the solidist; to the crisis, exudation and blastema, iatro-chemical and iatro-mechanical theories. In the place of all these partial stories, he proposes a knowledge of the fine organic processes of cell-life, as the only foundation of a rational doctrine of disease. The cell-theory, and its relation to practical medicine, however, is not altogether a novelty; but according to Virchow, it has as yet, only reached a certain stage of progress. What has been heretofore done, stands as a mere forerunner to his cellular pathology—just as ideas stand to deeds, and imagination to investigation. The foundation upon which he fixes his deductions, are actual histological facts, which he places before his disciples in the shape of microscopical preparations and



drawings. It will be said by some, that there is nothing new in the founding of pathology on histology. And on this score reference may be made to the last edition of Rokitansky's work, which is filled with numerous microscopic representations of diseased structures. It may be indeed asserted that Rokitansky even laid the axe to the root of his earlier, and somewhat humoral, style of pathology. He has rejected the doctrines of Crases. In his third edition, Rokitansky says: "The origin and increase of cells and nuclei take place, either immediately out of a blastema, as free cells and nuclei, or in mother-cells and mother-nuclei, endogenously;" and he adds, "the extra-cellular origin of these elements out of a free blastema, cannot be doubted, in my opinion, at the present moment." Virchow, on the other hand, utterly rejects the theory of the origin of free cells in a structureless blastema; just as elsewhere, men reject the theory of equivocal generation. He asserts, that only out of ready-formed structures arise cells; and that out of old cells there is a continual growth and development of new cells, *omnis cellula ab cellula*. He substitutes a cell-growth theory for the blastema theory. He does not consider exudation as a peculiar product of the capillary vessels, containing plastic and other constituents; but he regards it as an ordinary transudation mixed with the product formed in the diseased structures outside the vessels, among which products may be mentioned fibrin, pus, mucus, etc. Hence, then, according to Virchow, the inflammatory process does not take place in the capillaries, but in the elements of the tissues themselves—in the cells which through an increased activity attract the blood to them, and become thickened and swollen. In this manner, by cell-growth and by attraction of the juices, likewise arise all other new formations; the homogeneous, as well as the heterologous and malignant, so also new areola, or bony tissues, pus, tubercle, sarcoma, and cancer, the disease of plants, etc. The history of heterologous forms is this: in a particular place, or at a particular time, or in a particular degree, there arises a growth which is not proper to the part. Some other structure normal in some parts of the body, is substituted at the part for the structure normal to it, forming what Virchow calls a pathological substitution of tissues. The malignancy of heterologous growths consists in this: that the tissue affected is, by its vascularity, disposed to form a fluid, which is injurious to the structures around. The dyscrasiæ, also, can only arise through the propagation, by means of the blood, of the products of certain existing local affections. For example, the so-called phlogistic or fibrinous crasis depends originally upon a local production of fibrin in an inflamed tissue—fibrin being always a local product, and never arising in the blood. Many other blood-diseases also depend upon diseased states of

the spleen and lymphatic glands—those being glands in which white and red blood corpuscles arise. The disease which authors call pyæmia, consists partly of thrombosis and emboli, partly of ichonhæmia, of leucocythemia; the substance which forms the obstruction in phlebitis not being pus, but a purulent-like detritus. Pus arises only out of tissues. Life and disease, in fact, cannot be explained by physical or chemical actions, but are the developments and processes of organized elements.—*Schmidt's Jahrb.—Med. Times and Gazette, and Amer. Med. Monthly.*

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*Contributions to the History of Nervous Diseases of Syphilitic Origin.* By Dr. GJOR.

THE description of these diseases is based upon the accurate observations of thirty cases. Fourteen of the patients were less than thirty-five years of age, eleven were from thirty-five to forty years, and only one individual was more than forty-five years old.

In the great majority of the cases, the invasion of the disease was preceded by distinct prodromic symptoms; they consisted of pain in the lumbar region or in the extremities, or of an obstinate headache, with nocturnal exacerbations.

Most frequently the characteristic signs were of a paralytic nature; the symptoms which accompanied the commencement of the paralysis, were in general not very serious, and rather fugacious; fifteen times attacks in the form of apoplexy occurred, but were of but little intensity; five times only, these attacks gave rise to a complete loss of consciousness, and consecutively to weight in the head; twice the loss of consciousness was associated with convulsions; in two cases the apoplectiform attack occurred twice. Of these fifteen cases, there were only four in which the paralysis supervened suddenly, and without the health having been seriously impaired; in the eleven other cases, it developed itself imperceptibly.

In half of the cases hemiplegia was noticed; eight times, paraplegia; twice, facial hemiplegia; in three cases, paralysis confined to one extremity; in two, a general weakness of the movements of the four extremities; in nine, anæsthesia; and in two, hyperæsthesia. The paralysis of the extremities, was accompanied, in several cases, by paralysis of the sphincters, and in four cases by amblyopia, with dilatation of the pupil.

The interval which separated the first symptoms of constitutional syphilis from the paralytic attacks, was scarcely obvious in two patients; several months to a year, in eleven; one to five years, in eight patients. In the rest of the cases, the appearance of paralytic symptoms was retarded still longer.

It seemed that the patient who had suffered from several relapses of the symptoms of constitutional syphilis, were not any more exposed to the nervous diseases, than those in whom these symptoms had been observed only once; the number of cases of the first category is, in fact, only ten in the statistics of Dr. Gjør; he observes, however, that the number of his cases is not sufficient to deduce from them unobjectionable conclusions.

The treatment of the nervous diseases of syphilitic origin, has not given up to the present time, very satisfactory results. Of the thirty patients of Dr. Gjør, only five were cured; in twelve, a more or less decided amelioration was obtained; in six cases, no change took place, and seven times the disease had a fatal termination.

Dr. Gjør employed particularly the iodide of potassium, and often combined with it strychnia, or the preparations of arnica; this remedy has afforded him the most advantageous and prompt results. Mercury was employed in five cases, and was not successful in a single one. Dr. Gjør tried syphilization several times; in one case only it produced a rapid cure, in the six other cases, it was not followed by any improvement; in all syphilized patients the state of the general health was, however, much improved.

The three autopsies reported by Dr. Gjør, prove, at least, that nervous diseases of syphilitic origin, are not always owing, as was formerly supposed, to exostoses situated in the cavity of the cranium or of the spine; in one case, Dr. Gjør found softening of the brain; in the two others, no lesion of the nervous centres could be detected.—[*Schmidt's Jahrbücher*, and *N. A. Medico-Chir. Review*.]

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*On the Treatment of Prolapsus Ani in Children.* By M. GUER-SANT.

IN a small hospital in London, to which patients suffering from diseases of the rectum alone are admitted, prolapse of the anus is not treated by operation. According to Dr. Salmon, this disease can be cured, not only in children, but also in adults, by the evacuation of the bowels in the recumbent posture, as in this position the patient cannot strain much, thus, generally, preventing the descent of the intestine. After the observance of this treatment for some time, the relaxation of the parts, which permitted the prolapse, disappears, and a cure is effected. At the same time vegetable astringent injections are employed, although alum will be found useful. As a local application strychnia may also be mentioned. This was first employed



successfully by Duchassoy, who made two or three very small blisters around the margin of the anus, and dressed the raw surfaces with the ointment of strychnia. Dr. Johnson treated in 1854, two cases by the local application of the same remedy; he states that in the lighter and more recent forms of prolapse he obtained a cure by regulating the bowels, employing astringent injections, and administering tonics. In neglected cases, when the sphincter is much relaxed, strychnia may be of some use, but it is not, however, to be recommended, as it is unreliable in grave, and superfluous in light cases; it is, moreover, troublesome, and not without danger. Dr. Johnson considers cauterization after Guersant's method more valuable.

In the Metropolitan Free Hospital, Mr. Hutchinson employs the tincture of nux vomica, in very small doses, since he believes it imparts tone to the sphincters and walls of the intestines.

Sir Benj. Brodie prescribes, internally, calomel and rhubarb, and uses injections of the dilute tincture of the chloride of iron.

Mr. Ashton maintains that in children suffering from this affection, the liver is principally to be attended to; and Mr. Curling recommends the use of cod-liver oil, after sufficient purgation.

Mr. Salmon treats recent cases in children in the following way: First the patient must evacuate his bowels in no other but the recumbent posture; second, the prolapsed part is thoroughly moistened with a decoction of oak-bark and alum; third, internally, he employs calomel and rhubarb; when he orders, fourthly, tonics, and a nourishing diet.

When the usual remedies have been employed without effect, and the symptoms remain obstinate, M. Guersant advises cauterization of the skin and sphincter muscles with the hot iron. He does not cauterize the whole of the protruded mucous membrane, as was formerly done, as he considers the measure barbarous and hazardous. He proceeds, generally, in the following manner: The child is subjected to a certain diet, and shortly before the operation an injection is administered to clean out the lower bowel. The patient is placed upon his side, his thighs are flexed, and the protruded rectum reduced; an assistant drawing away one buttock, while the operator controls the other. The cautery is about the shape of that used by dentists, either curved or straight, and terminates in a small ball, which, above, runs out into a little point. It is applied at opposite points around the anus, and must penetrate the sphincter in order to be efficient. It is also necessary to draw the margins of the anus well apart on applying the cautery, and allow the small ball, after its point has penetrated the sphincter, to act upon the margins of the skin and mucous membrane. The

patient must be placed under the influence of chloroform, and the iron be used at a white heat. If the rectum protrude during the operation, it should be pushed out of the way of the instrument. Cold-water dressings are the only applications made after the operation.

M. Guersant recommends this plan of treatment as an excellent, but not infallible means. In a few rare cases the children are cured in one day; more frequently, however, the prolapse returns in a few days, and the cure is not completed before the eighth or tenth day, when cicatrization has taken place. Sometimes a second cauterization will become necessary.—[*Behrend und Hilderbrand's Journal für Kinderkrankheiten*, and *North Amer. Med. Chir. Review*.]

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*Anatomical and Physiological Researches on Dropsy consecutive to Typhoid Fever.* By DR. LEUDET.

THE different kinds of dropsy following typhoid fever have been described by a great many pathologists; in France this subject has received less attention than in other countries. MM. Louise, Chomel, Forget, Guerin, and Piedagnel describe only the œdema confined to the lower extremities. M. Martin is perhaps the only French author who has given an account of the anasarca and ascites following typhoid fever.

Partial œdema has been very well described by R. Virchow, (*Archiv für pathologische Anatomie*, tome ii. p. 401,) and by Magnus Huss, (*Statistique et traitement du typhus et de la fièvre typhoïde*, p. 205; Paris, 1855.)

Dr. Griesinger, of Tübingen, has given a very complete description of these dropsies (in *Virchow's Handbuch der Pathologie*, etc., tome ii. p. 173, 1857.)

"There exists," says he, "a local dropsy consecutive to the coagulation of the blood in the veins, the cause and signification of which are evident. We meet, as a very rare complication of typhoid fever, in its second stage, an extensive dropsy of the cellular tissue and of the serous membranes. This dropsy is more common in certain epidemics; I have observed it, during an epidemic which befell a population suffering from dearth, in about one-fourth of the cases. the œdema supervened generally at the end of the third week, more rarely at the end of the second; it commenced sometimes in the face, at other times in the inferior extremities, spread rapidly over the whole body, and was often accompanied by more or less ascites. A small number of patients presented, in addition, a marked albuminuria. In most cases it was not very decided, or had already

ceased. During this time the skin was warm and dry; a copious eruption of miliaria accompanied the beginning of the dropsical effusions, and persisted as long as they lasted; sometimes the pulse was at the same time slow and irregular. The internal organs showed no symptoms of disease. This dropsy lasted generally from six to ten days and disappeared mostly within a few days. It exercised no favorable influence upon the termination of the disease; in fact, only a small number of patients succumbed. The dropsy aggravated the prognosis only by the circumstance that it prolonged the period of convalescence."

M. Leudet had not seen a single case of this dropsy during nearly ten years of observation made in the hospitals of Paris; but having had charge, for about four years, of a division of the Hôtel-Dieu of Rouen, he had the opportunity of observing a certain number, and has reported seven cases of this kind in his memoir.

A careful comparison of his observations with those published by other authors enabled him to draw the following conclusions:—

The typhoid fever which is followed by dropsy is characterized in Rouen, as in Paris, by ulceration of the intestinal follicles. The serous infiltrations, whether confined to the lower extremities, or general, were of the nature principally described in Germany. They are rarely limited to the superior extremities or the face.

Sometimes the effusion takes place into the peritoneum; M. Leudet has observed no case of this kind; in one instance he met with a peritonitis.

The œdema is colorless and painless, and is, therefore, easily distinguished from erysipelas. It supervenes mostly without prodromic symptoms, and coincides often with some exacerbation of the fever, with sweats, and abundant eruption of sudamina, and an intense bronchitis. This latter coincident has been particularly remarkable in Rouen, but it is not constant. In the cases observed by M. Leudet, as well as in the epidemic studied by M. Griesinger, the œdema showed itself, generally, in the second or third week of the disease, and disappeared in two or three weeks.

M. Leudet has been unable to discover albuminuria or lesions of the kidneys in any of his cases. These two morbid states have been observed, however, by Rayer, Martin Solon, Finger, Frerich, Begbie, Christison, Johnson, Griesinger, Barthez and Rilliet.

The cause of this œdéma, says M. Leudet, seems to be the constitutional weakness of the inhabitants of our city, and the very nature of the typhoid fever, which is more frequently accompanied by stomatitis, pleurisy, etc. than anywhere else.



Besides, the fevers and inflammations have generally an adynamic character in Rouen. The anasarca is not a serious complication of the fever; it prolongs, however the period of convalescence. The treatment consists of tonics exclusively.

[*Archives Gènèrale*, and *North Am. Med. Chur. Review*.

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*On the Sulphate of Zinc and Sulphuric Acid as an Escharotic.*  
By Professor SIMPSON.

In a clinical lecture on carcinoma of the uterus and mamma and its treatment by caustics, we find the following very important remarks:

"If you make use of the caustic made by saturating strong sulphuric acid with a quantity of sulphate of zinc, dried and powdered, you can manage by dipping an ordinary quill pen in this mixture, as if you were going to write with it, to lay it in a number of lines across the tumor, the number of lines corresponding to the size of the growth which you wish to destroy. Very speedily the super-sulphate of zinc kills the skin in the course of the lines which you have drawn; and if you will now scratch assiduously with the filled pen along these lines, you will cut through the skin in a few seconds. Leave for a day the fissures filled with the caustic paste, and then every day or two by renewing the application of the scratching and caustic you can cut down to a greater depth. In making the first application I usually make a fissure of about a fourth or three-eighths of an inch in depth, and then destroy the tumor more deeply by successive applications. In this way five or six days may suffice for the removal of a good-sized tumor. Let me add, that in thus destroying and digging out, as it were, from its very base, a cancerous tumor of the mamma, or other external part—neither of the two caustics applied by the quill would suffice singly and individually. If you use the sulphuric acid alone you will find that the acid so chars and hardens the spot to which it is applied that you cannot next day cut or scratch deeper through it with the pen; while the super-sulphate of zinc paste keeps the parts soft and pliable. Again, if you used sulphate of zinc alone you could not cut through the skin or penetrate deeply with it. For that purpose the aid of the sulphuric acid is required, and the relatively slighter pain attendant upon this than upon most other caustics is perhaps explicable by the fact that sulphuric acid acts almost instantaneously in producing its destructive effects upon living tissues. Usually the healthy skin at the edge of the sloughing-out mass is granulating, contracting, and partially cicatrizing before the dead tumor itself

separates. Dress the exposed tissues or ulcer before and after the tumor is enucleated with black wash, chloride of zinc, sulphate of zinc, nitrate of silver, or any other appropriate surgical lotion."—[*Med. Times and Gazette*.

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*Case of Pityriasis of the Eyelashes.* By Dr. JAMES G. HELDIGE.

CASE.—A weak, scrofulous-looking child came to the Eye Dispensary, Mecklenburg Street, complaining of inflammation of the eyelids. The tarsal edges were slightly ulcerated and inflamed, and the cilia were very long, and appeared of a shining brownish color, and much thicker than natural. The itching was so intense that she was constantly rubbing the lids, which aggravated her symptoms very much. The only previous treatment had been bathing the eyes with an infusion of chamomile, which she did for four or five days, without deriving any benefit from it. On examining the lashes with a lens, Dr. Heldige detected an immense number of exceedingly small pediculi, about the size of the point of a pin, which, together with their ova, were exceedingly difficult to detach. They differed from the ordinary pediculi in shape and size, the legs being very long in proportion to the body, which was small, and of a circular form. The prescription was ointment composed of corrosive sublimate, 4 grs.; tar, ʒj; and lard, 3j; about the size of a pea to be rubbed into the tarsal edges of the lids morning and evening. This had the effect of completely removing the disease in about four days, and no relapse occurred during a period of five days longer that the case remained under observation.

This disease is not by any means of frequent occurrence. Mackenzie, in the last edition of his work, mentions only one case of it, and that not occurring in his own practice, but copied from a notice published in the '*Lancet*' by Mr. Lawrence. Arlt, of Vienna, states that he has seen the disease occasionally; and Desmarres, of Paris, in his recent work, published in 1858, does not even allude to it.

The species of pediculi peculiar to the body and head are also sometimes found in the above situation; but those described by Mr. Lawrence, and observed in the above case by myself, are, according to my own observation, peculiar to the eyelashes.

[*Dublin Hospital Gaz.*

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*Are Babies to be taught to Walk?* By—— (How to Manage a Baby, A Tract; also *The Sanatary Review*, Jan. 1859.)

"PEOPLE talk about 'teaching babies to walk;' but babies do not need teaching, for they will be sure to get up and walk

when their legs are strong enough, and it does them harm to do so before; in this, as in very many other things, babies would be all the better for being left to themselves. But this does not suit some mothers, who are in a hurry to see their children walk; such mothers cannot rest content without putting their children into leading strings, or go-carts, or leading them with the hand. All that they generally get for their pains is the sight of their children's bandy legs and crooked ankles, caused by being forced to walk before their time. Who would be a baby?

"But, though baby should not be hurried in walking, it should be allowed to keep moving all day long, while it is awake, for the limbs cannot get strong unless they are used. The best plan is, to put a piece of soft matting and a piece of carpet on the floor, and put baby down upon them to stretch, roll, and tumble about like other young creatures. If it has a ball or a rag-doll to crawl about after, it will be 'as happy as the days are long,' and will, besides, be very little trouble, and be making its limbs strong, ready to walk by-and-by. It is a great pity to accustom a baby to be nursed, for it only does it harm, and gives the mother a world of trouble in the bargain. In the summer, it is a good plan to spread the matting and carpet on the grass in the garden, and put baby down on them, to use its limbs in the pure air and light. In short, wherever it is, and whatever it does, it should keep moving all the time. The birds, the beasts, the fish, and the creeping things are scarcely ever still five minutes together in the daytime. Moving brings life and health to all things, babies among the rest."—[*Ranking's Abstract*.

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*Rules for the Dietetic Treatment of Asthma.* By DR. HYDE SALTER, Assistant Physician to Charing Cross Hospital.

1. The tendency of food to produce asthma is greatly increased by the state of sleep; therefore, nothing should be taken after such a time as digestion and absorption may be completely over in the stomach and small intestines, and even the lacteals quite empty, before bedtime.

2. This long fast before sleep involves a long period of inanition; therefore the asthmatic should break his fast early and heartily.

3. The quantity of food the asthmatic takes should be small; therefore it should be highly nutritious.

4. As a rule, the tendency of food to produce asthma is in direct proportion to its general indigestibility; therefore the asthmatic's diet should be of the simplest and plainest kind.

5. But there are some articles of diet that have a special tendency to produce asthma; therefore from these the asthmatic should exercise the strictest abstention.—[*Lancet*.



## EDITORIAL AND MISCELLANEOUS.

## DYSMENORRHOEA.

[A recent paper, published originally in the *Charleston Medical Journal and Review*, has been kindly sent us by the author, for examination. The ingenuity and utility of the new instrument therein described, as well as the acknowledged difficulty ordinarily attending the treatment of Dysmenorrhœa, induce us to spread the article in full, upon our own pages. We welcome the suggestion of a remedy, at once, so plausible, expeditious, and easy of application.

Few Physicians, who have enjoyed much general practice, can remember with any satisfaction whatever, their cases of *Dysmenorrhœa*. Occasionally here and there, as a green spot in the cheerless waste, a case may be called to mind, in which, this or that remedy, nearly always empirically applied, has effected full and satisfactory relief. but the majority have left no impression of pleasure; remedy after remedy failing, till at last, they have been either neglected or given over, in utter despair.

Much of this unsuccess has been, we agree with Dr. White, owing mainly to there being no established theory as to the nature and origin of the disease, and, while we are not prepared to admit, that, all cases of Dysmenorrhœa depend upon mechanical obstruction at the os uteri, both our observation and experience with cases, incline us to the opinion that, such is the most frequent cause of this very painful disease. Obstruction *any where* along the course of the utero-vaginal passage, seems competent to give rise to all the symptoms peculiar to Dysmenorrhœa. There is at present, a negro woman in Jackson Street Hospital, under preparatory treatment, for the operation for Vesico-Vaginal Fistula. At the time of her entrance into the hospital, the vagina was completely occluded, the union of the anterior and posterior walls, having taken place just behind the urinary fistula. Every month, this patient suffered with all the symptoms of Dysmenorrhœa, in its most agonizing form, yet we could find no indication by which we could hope safely to reach the accumulation behind. A large tumor, the distended vagina, could be felt *per anum*, but no pointing in any part of the vaginal surface. The pain was kept down with morphine during the menstrual period—and subsided almost entirely in the intervals. The collection finally pointing just behind the posterior lip of the urinary fistula, it was opened, when the patient experienced immediate and inexpressible relief, on the discharge of nearly a quart of retained menstrual fluid. The incision has been kept open with sponge tents and uterine bougies, and no symptom of Dysmenorrhœa has appeared since. Precisely the same result has ever followed our operations in *every* case of occluded vagina;

indicating plainly that *obstruction* at other places, than at the uterine orifice, is ever competent to produce all the symptoms of Dysmenorrhœa, and that, on the removal of the obstruction, the function is performed naturally and without pain. A large majority of cases of Dysmenorrhœa attended with engorgement of the neck, owe the pain and distress experienced during menstruation, we are perfectly confident, more to the resulting occlusion of the cervical canal, than to the inflamed condition of the cervix and when benefitted by local applications as cauterization and astringents, the relief is due more to the incidental enlargement of the canal, than directly to the reduction of the inflammation which had been the chief object of treatment.

It is a notorious fact, that women suffering from Dysmenorrhœa, do not bear children. This sterility is generally attributed to the disturbance of the system, consequent upon the *painful periods*. May not the mechanical occlusion be an influential cause in preventing conception? These women do sometimes, as it were, *by accident* conceive; and then, we cannot say from our observation, that they are any more liable to abort, but on the contrary, enjoy more excellent health during gestation, than they ever did before. Then again, as our observation will fully bear out the author, these women, after parturition, never again suffer from the old distressing malady; the distension consequent upon the act having entirely broken up the obstruction upon which the distress chiefly depended.

If then, as would appear from the above considerations, mechanical obstruction at the neck of the womb is a frequent, or even, an occasional cause of painful menstruation, a disease so common, and at the same time, attended with often, both bodily and mental distress, on account of the barrenness it produces, some well-arranged and convenient means of removing it, becomes a great desideratum in practice.

Dr. White's very ingenious Hysterotome is certainly a valuable addition to the armamentaria of the Surgeon and Obstetrician, for it possesses all the advantages of safety, convenience and efficiency, so much to be desired in an instrument of the kind. For his ingenuity and care in bringing it to its present perfection, he certainly deserves the thanks of our profession.]—*Editors Southern Med. and Sur. Journal.*

*A New Hysterotome.* By OCTAVIUS A. WHITE, M. D., of Charleston, South Carolina.

DYSMENORRHŒA is certainly one of the most painful and intractable disorders we can be called upon to treat. It is of very frequent occurrence, and is the fruitful source of much ill health to females. Commencing, as it has often been known to do, even with menstrual life, it may proceed unchecked throughout successive years, and yield at last, only to

time and the natural cessation of the discharge. If unrelieved, a trifling case is liable to become worse and of longer duration, until finally, the afflicted patient may be only beginning to recover from the shock of one period, when the dreaded approach of the next is near at hand. It is, therefore, not at all improbable, that many unfortunate women have fallen victims to consumption and other terrible maladies, in consequence of these periodical sufferings.

Under such circumstances, the attention of accoucheurs must ever have been directed to its relief. And such, indeed, has been the case, though it is mortifying to reflect, that until a comparatively recent date, the blindest empiricism appeared to direct the treatment. Incorrect views respecting its pathology, inevitably led the way to notable errors in practice, and the wretched applicant for relief, was too often subjected to struggle not only with the original malady itself, but also against the evil effects of injudicious means used to remove it. Thus, at one time, obstetricians erroneously attributed all the phenomena of this disease to plethora, when cups and lancets were indiscriminately applied. At another, debility was the prevailing doctrine, and tonics became the fashion. At another, spasm was obscurely hinted at as the cause, and opium was the only specific. At a still later period, we hear of nothing but its nervous and neuralgic origin; this was the day of active temporising,—main reliance being placed upon inunctions with belladonna, aconite, and the like; whilst, behold, at another, the very identity of the disease called in question, and to ovarian irritation and inflammation, imputed the source and origin of all the manifold suffering that was certainly uterine in its seat.

Such were many of the vague notions which prevailed respecting Dysmenorrhœa, anterior to the year 1823. About this period, Dr. Mackintosh of Edinburgh, first suggested the employment of mechanical means for its relief. Being led to investigate this subject by accident, he forthwith commenced a series of examinations with experiments, which resulted in convincing him that preternatural narrowing of the uterine canal, was most commonly concerned in the production of this complaint. This point being established, he proceeded to account for the alarming symptoms so frequently met with, in the following manner: "The menstrual discharge, after it is secreted in the uterus, cannot readily escape, in consequence of the small size of its orifice; distension of the organ is the result, which, by exciting the contraction of its fibres, produces uneasiness and pain in the pelvic region. When the os uteri is very small and the secretion viscid, or mixed with coagulated blood, shreds of membrane, or organized masses, then the distention becomes more considerable, and stronger contractions are excited. Sometimes the action of the abdominal muscles is called into play, and



bearing down pains are produced, resembling in every particular, the pains of labour, and continue until the expulsion takes place."

These observations, originally entertained and expressed so many years ago by Dr. Mackintosh, have since become generally adopted; and though improvements in the management of these cases are still being made, no one can fail to observe that the doctrines promulgated by that illustrious practitioner, continue to influence surgical opinions and practice even to the present day.

Dysmenorrhœa, therefore, being proved to result most commonly from mechanical causes, surgical remedies for its control have largely superceded the vaunted resources of the pharmacopœia. But though surgical appliances are oftenest available in the management of this disease, we must not lose sight of the fact, that there are also a certain proportion of cases which require constitutional measures alone for their cure. Let me not, therefore, be thought to accord to surgical interference too great supremacy, experience having long since assured me, that by far the largest number are amenable only to a careful and judicious institution of both plans of treatment combined. If, therefore, we adopt one of these modes of relief, and attempt to practice it to the exclusion of the other, we shall too often be baffled in our best directed efforts, and find the disorder liable to return, even after its apparent removal.

The method advocated by Dr. Mackintosh, consists, as will be remembered, of careful and repeated dilatations of the os and cervix uteri, with bougies of various sizes. But the same evil consequences which often eventuate during the management of ordinary urethral stricture, by dilatation, were so liable to arise during this course of treatment here, that surgeons were soon compelled to abandon this procedure and look to measures more expeditious, and against which no such objections could be alledged.

Accordingly, about twenty years after, Prof. Simpson announced to the profession, another mode of treating this obstinate complaint, by an expedient at once novel and brilliant. This was by incisions skillfully made within the cervix uteri, with appropriate instruments, and keeping the parts afterwards dilated with sponge tents until the cure was complete.

In behalf of this safe and simple operation, it is contended that it at once places the parts in somewhat the same condition as subsequent to parturition or a miscarriage. For it is a notable fact, that many cases of otherwise intractable Dysmenorrhœa have been known to abate or cease altogether after one or other of these occurrences have transpired. As to the efficacy of this mode of operating, it would be needless here to recount all the high testimony recently added in its favor. Suffice it to remark the practice has since been adopted by many of the most eminent accoucheurs and surgeons in this country and elsewhere

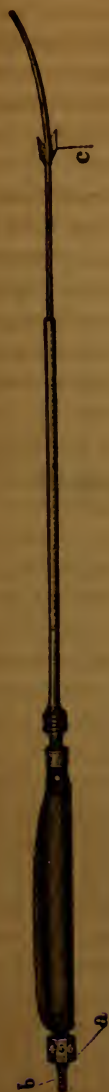
Promising such certain and expeditious relief as this procedure certainly does, the facility with which the several steps of the operation may be performed, cannot fail to interest surgeons. Already, therefore, have instruments of different construction been suggested, each having in view that important design. That invented by Prof. Simpson, has, thus far, very justly commanded the greatest favor. This, it will be remarked, however, is but a modification of the old *Lithotome Cachè*, which though a contribution of considerable value in its proper place, to the armamenta of Surgery, is certainly liable here to objections which readily suggest themselves to all who attempt its use.

Having been compelled, upon several occasions, to resort to the practice and the instrument of this eminent master, for the relief of obstructive Dysmenorrhœa, the peculiar dangers which embarrass its use, compelled me to lay it aside and employ one invented by myself, of the following description.

Annexed is an engraving, which, though considerably reduced from the original, will assist, I hope, in giving an adequate idea of this instrument. It consists, as will be seen, of a long and delicate staff or stem affixed to a handle of unusual form. The staff is about the calibre, and has somewhat the same curve as a No. 3 small catheter. It is full nine inches in length, and is so plainly notched and numbered off upon its posterior surface, from the point to the handle, that these measurements may easily be distinguished by the touch alone.

At a moderate distance from the point of the instrument, may be noticed two small blades, jutting out from the staff on either side. Each of these blades is capable of being thrust out about a quarter of an inch, or concealed by being withdrawn again within its sheath in the circumference of the staff, by means of a certain manipulation at the handle, which I shall take occasion presently to describe.

The standard depth of the womb from the os to the fundus uteri, has already been ascertained to be just two inches and a half. To render, therefore, the application of this instrument safe, and in order to bring its utility within the range of possible cases of even smaller uterine di-



mensions, a point about an inch and a half from the apex of the stem has been selected, from which the knives are made to protrude when needed.

The projection and closure of these blades is affected with great facility, by means of a female screw, (*a*) which plays upon a thread (*b*), situated at the very butt of the handle. This screw is hexagonal in shape, each side of which is made to bear a number, ranging from one to six. The instrument being closed, No. 1 corresponds exactly to the convex surface of the handle, but progressively, as the succeeding numbers are brought upwards, by turning the screw, the blades are forced out to the extent desired; No. 6, of course, denoting the maximum. The value of this little contrivance will be obvious, it being by this means alone that the operator can learn at a glance, to what degree the blades have been opened or shut, after that portion of the stem which contains them has been introduced, to perform its duty within the uterine orifice.



Fig. 2.

The second figure represents an anterior view of that section of the stem which holds the blades, the upper surface being removed, in order to display the mechanical principles upon which they act. The stem, therefore, is seen to be hollow, and the two blades, lying back to back, are held together, and fastened by means of a single pivot to a small steel wire, which is made to pass quite through the centre of the instrument, from *c* to *b*, fig. 1. When closed, the blades, of course, overlap each other, to economise space, but obedient to the screw, (*a*) they are forced to part, one on each side of the stem, in consequence of the steady resistance opposed to the inclined plane of their backs, in the shape of a wedge.

The handle made of some light material, is intentionally flattened upon its posterior surface, and convex upon its anterior. The convexity of the handle, corresponds to the curvature of the stem, and by referring, therefore, to this distinction, the operator is constantly reminded of the direction of the point of the instrument, even while hid to the handle within the vaginal passage.

To incise the neck of the womb with this instrument, the patient is made to lie upon her back, in a convenient position. The forefinger of the right hand is inserted into the vagina, and its extremity held in contact with the os tincæ, to act as a guide. The blades being closed, the instrument is next held in the left hand, and its point carefully slipped



along the palmer surface of the finger, until it is finally directed, in this manner, through the cavity of the cervix and within the os internum. It is then maintained in this position, until with the right hand, the blades are forced out to the extent desired; when, by simply withdrawing the instrument, in the same direction as introduced, the operation is fully accomplished. To complete the cure, all that remains now to be done is to prevent re-union of the cut surfaces, and this is effected by means of a sponge tent, or a permanent metallic bougie, similar to that recommended by Prof. Simpson. Whichever is selected should be worn within the parts for several days, and only changed as often as cleanliness requires.

My main object in communicating this paper, have been not merely to record my approval of Prof. Simpson's mode of treating certain cases of Dysmenorrhœa by incision, but to introduce to the profession a new surgical instrument for effecting this purpose. I trust that its credit rests not so much upon its originality and comparative cheapness, as upon its especial adaptation to the relief of *all classes of strictures* in which internal cutting would be admissible.

The instrument in my possession is one which was neatly and successfully fabricated at the factory of Mr. George Tiemann, No. 63 Chatham St., New York. The skill of this veteran instrument-maker, is already too well known to need encomium.

In conclusion, I would simply add, that this instrument needs but a single application to effect in these cases, what it requires, at least, two or more attempts with any other to perform. It offers, consequently, an easier, more expeditious, safer, and, upon the whole, less painful mode of operating than any other I know of. I therefore recommend its use to my professional brethren, hoping that it may be found of as much service in their hands as it has so far proved in my own.

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AMERICAN MEDICINE IN GREAT BRITAIN.—We are told that an intelligent gentlewoman of England, not long since, was filled with astonishment on beholding some of the inhabitants of the United States, exclaiming—"why their skins are fully as white as ours, and they wear clothes!" We are forcibly reminded of the above, (which we hope is an exaggeration, or the result of a too implicit reliance on the romancings of "Boz in America,") by a very just and most friendly article, in our valued exchange, *The Dublin Medical Press* of July 20th. The able and liberal minded Editor, is, himself, entirely free from the false impressions he is contending against, and magnanimously, in the very midst of a clamor of complaint against him, for quoting American articles into his Journal, dares to raise his voice in vindication of American Practitioners,

American writers, and the much despised American medicine generally.

Many of our readers will smile, while others will blush with indignation, when they learn the *reason* why American Medicine has not, as yet gained a more important position in Great Britain. All will, at least, experience surprise to read, that "The objection commonly made is, that *statements as to matters of fact*, are not to be relied on, when made by contributors to American Journals, in consequence of a habit of exaggeration assumed to be prevalent there." Let them disbelieve—we shall adopt the wiser course of *believing* and *proving*, and "holding on to the good wherever it may be found." We are free to admit that there is a *credulity* which may often, render its subjects liable to imposition, but on the other hand, it is well known, that there is an immuring *scepticism* far more dangerous, which ever blocks up all the avenues to improvement, and binds down its unfortunate possessors, to the narrow bounds of personal experience and immediate observation.

We cannot now enter fully into a consideration of the respective merits of British and American Medical Literature, but we can assure the Editor of the Medical Press, that his generous vindication of American Medicine, is not misplaced, and will be highly appreciated. Time only is required, to convince his confrères that they would consult the interest of their readers, by following his example in placing more reliance on, and quoting more extensively from, the reports of their American brethren.

We here present the article in full :

#### MEDICINE IN AMERICA.

Occupied by medical politics at this critical period of professional revolution, we have been obliged to postpone the consideration of many topics which should otherwise have been the subject of discussion in our columns, and amongst them one which should have been noticed. We have often been asked, and sometimes not temperately, why we quote and copy so much as we sometimes do from American journals, meaning those of the United States, and to this question we have now to reply, and perhaps after Irish fashion, we may begin by asking another : Why should we not make our brethren here in Ireland, from time to time, acquainted with the progress of professional improvement on the other side of the Atlantic? for it must be admitted, that improvement is taking place in a country so perfectly free to adopt any alteration which experience justifies. Strange to say, the objection commonly made is, that statements as to matters of fact, are not to be relied on when made by contributors to American journals, in consequence of a habit of exaggeration assumed to be prevalent there; an objection, we must say, more discreditable to those who make it than to those to whom it refers. Such an assumption is, as regards American medical literature, alto-

gether without foundation, whatever grounds may exist as to its adoption with reference to literature of a different class, emanating from this quarter, and could have arisen from prejudice only, or misconception, amounting to what may be called vulgar error. To justify the opinion we express, we have only to challenge a comparison between the medical journals of the United States and those of Europe, and to ask any candid man, whether the former exhibit less evidence of veracity than the latter; in fact, we are convinced that there is no foundation whatever, for any such offensive supposition. On the contrary, we believe that even a cursory examination of the American medical periodicals, will at once refute any such error, for we venture to say that they afford at least as much internal evidence of regard for truth, as their European contemporaries. In the United States, the journalist and contributor evidently address themselves to readers, of whose judgment and criticism they entertain respect, while nearer home we find, from the nature of many communications, writers must often consider their readers very deficient in these qualifications. We do not mean to say, that in The States there is no publication of worthless matter, or exaggeration of the importance of communications, but we venture to say that there is less of writing for mere notoriety, or, as it is called, "keeping a man's name before the public," and more of an honest conviction, that the information offered is entitled to consideration. Hence our reliance on the value of this source of professional knowledge, and our occasional use of it for the benefit of our readers; and hence, too, our regret that in consequence of defective postal and agency arrangements, we cannot avail ourselves of it as freely as we wish. That this vast country, with its variety of climate, soil, and cultivation, and its peculiarities resulting from civilization and political condition, must afford illustrations and examples highly important, both to practitioners and conductors of medical institutions cannot be denied, and therefore the advantage of appealing to facts recorded in its annals. Disease assumes characters in these regions, not to be observed in our climate, and the habits of the people have established customs and created institutions, so different from ours, that the consequences cannot be unnoticed by the journalist or neglected by the practitioner. We are at this moment here in Ireland, engaged in warm controversies, as to the nature of the qualification which should entitle a medical man to practise, and as to the conditions under which persons should be allowed to dispense medicines, both of which points seem to be more or less settled in The States, by some means which we cannot well comprehend. The American Physician, as the general practitioner there styles himself, seems to enjoy rights of practice, and liberty to compound and dispense medicines which are denied to the same class in this country, and it may be well to ascertain whether this has been followed by consequences discreditable to the medical profession, or dangerous to the public. This free trade in physic, at the same time, does not appear to lead either by law or practice, to any obstruction disabling the pure physician or surgeon from pursuing his avocations as he does in this country, and so we have here again practical proof that, however it has been done, much of what we are contending for in this country, has been accomplished elsewhere. Believing, then,



that the study of disease may be promoted by observation of its effects on black slaves in South Carolina, as well as on white free men in New England, and that the operation of peculiar laws and customs on medical affairs throughout the Union may prove instructive, we propose to make our readers from time to time, acquainted with what is passing in the western medical world. To fastidious friends at home, and considerate contemporaries abroad, we therefore appeal to enable us to accomplish this object, and we shall be grateful for any assistance afforded us, to carry our views in this respect into effect. In fact, we begin to think that the old medical world is growing older, and that a little transfusion of new blood may give new life; the journals of Europe are full as ever, but their contents remind us more of the garrulity of age than the freshness of youth. At this side of the Atlantic, men are, in fact, so busy praising themselves and endeavoring to prove that little was known until their time, that it may be well to learn how it is at the other, and thus perhaps relieve us from the monotony of books and journals which is, at present, rather oppressive.

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[Dr. L. D. Ford of Augusta, has been using chloroform in cases of Delirium Tremens for several years with the best effect. We are surprised at the *strangeness* of the remedy in England.]

DELIRIUM TREMENS.—A case of delirium tremens is recorded: the subject, a stout healthy man, in which laudanum and hyoscyamus having completely failed in moderating the violence of the symptoms, chloroform was administered; in five minutes he fell asleep, and continued sleeping for six hours, when he awoke, and the symptoms of the disease had vanished. (*Ed. of Lancet*, p. 57.)

We apprehend that the *indiscriminate* use of chloroform in delirium tremens would be highly dangerous. If a sly drinker, one who habitually gets nearly drunk, be compelled to leave off his stimulants suddenly, he will probably be attacked with delirium tremens; but in this case the use of chloroform would be hazardous. You have an impoverished vital energy, an exhausted nervine force. Small doses of brandy and constant administration of small quantities of beef-tea are here called for. But where, as in the case mentioned above, the man is not an habitual drunkard and the constitution is good, in fact, if we may use the term, when the disease is more sthenic, chloroform will be found invaluable; opium and brandy injurious.—[*Braithwaite's Retrospect*.]

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*On Hæmorrhagic Measles.* By Dr. OTTO VEIT.—The prevailing view that the hæmorrhagic or petechial form of measles is of a malignant or septic character indicating great danger, is combatted by Dr. Veit upon the strength of the experience which he has derived from the various epidemics that have occurred in Berlin, from 1847 to 1857. After quoting various authors from Huxham downwards, of whom Rilliet and Barthez alone coincide with the views advocated by Dr. Veit, he details his own observations. He met with the petechial form in 11 out of 160 cases; and although 9 of them lived in needy circumstances, they all

passed through the disease without any peculiar disturbance or ill effects. The author does not deny *in toto* the occurrence of a septic form of measles, but is unwilling to admit that the petechiæ in the cases observed by himself were due to decomposition of the blood, but that they must be regarded as a proof of the greater intensity of the *physiological* process in the same way as the catarrhal affection of the respiratory organs may be converted into croup or bronchitis. In the hæmorrhagic form the eruption, after the usual prodromata of fever and catarrh, on the second, third, or fourth day, instead of becoming paler, suddenly assumes a dark-red colour. These spots become still darker on the ensuing days, and even black; they are round or angular, but have a sharply-defined margin; they vary in size from a flea-bite to that of a pea or bean, and more. They do not disappear on pressure, but behave exactly like extravasations. These spots retain their intensity of colour for a day or two, and then pass through the various changes of colour observed in other extravasations, becoming purple, brown, and yellow before they disappear all together. The desquamation of the epidermis is more marked over these spots than elsewhere. Dr. Veit has not met with cases in each epidemic of any intensity; sex appears to exert some influence on the occurrence of the hæmorrhagic form, seven having been males and four females; while of the 160 cases of measles, 80 were males and 78 females, the sex of two children not having been noted. All the epidemics observed by the author had a benignant character, only 3 of his 160 patients having died.—[*Archiv. für Pathologische Anatomie und Physiologie*, and *Brit. and For. Med. Chir. Rev.*

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*Influence of Hunger on Poisoning.*—By the following extract from article xv. of *Virchow's Archiv. Bd. xiv.*, (Hefte 5 and 6,) it is intended to make known some of the results obtained by *Dr. Th. Köhler* of Marburg, who, among other interesting experiments, endeavored to determine the influence of hunger upon the activity of poisons—*i. e.* the difference in the rapidity of their resorption in fed and unfed animals. It appears that hunger retards resorption, and delays the occurrence of poisoning and death, so that the generally prevailing idea that an emaciated, weakened individual must sooner succumb to a dose of poison than a well-fed and strong individual, is evidently incorrect. Köhler explains that the rapidity of resorption bears the necessary relation of effect to cause to the number of respirations and the corresponding frequency of pulse. Abstinence of nutriment diminishes the absorption of oxygen pretty steadily till death, corresponding to which the quantity of excreted carbonic acid becomes less. During hunger, the blood, therefore, cannot circulate with the usual velocity, and poisonous substances that must, directly or indirectly, be incorporated with the current, are carried along less rapidly and consequently reach the central parts of the nervous system later than when the usual food has been taken. This action must necessarily also, therefore, occur later. The earlier occurrence of death in well fed animals is also thus explained. Circulation on which all processes depend being influenced by respiration, and the state of the nervous system, becomes more rapid when the muscles are violently contracted, and nourished animals distinguish them-

selves generally from the unfed by their much more forcible and violent muscular action, thus hasten their own destruction.—[*Med. and Surg. Reporter.*

*On the Treatment of Croup.* By M. MALGAIGNE.—In a letter addressed to the Academy of Medicine, *à propos* of a long discussion on the treatment of croup, M. Malgaigne complains of the erroneous manner in which his doctrines as to the employment of tracheotomy in this disease have been represented, and sums up the views he entertains thus: he regards the operation as a sad but imperative duty, justified only by the absence of all other chance for the patient, and he distinctly opposes the doctrine of M. Trousseau, who inculcates that the operation should be had recourse to as soon as false membranes make their appearance in the larynx. M. Malgaigne considers that the time for the surgeon to interfere is when the physician states himself to be unable to do anything more. [*L'Union Médicale*, and *Ibid.*

*Translation of Medical Works into the Chinese Language.*—Some of the standard medical authorities have recently been translated into the Chinese language by Dr. Hobson. They include works on the following subjects: Philosophy and General Anatomy, Surgery, Diseases of Women and Children, Medicine and Materia Medica, and on General Science. Some of these works have created great interest in them and have been republished by Chinese Mandarins, and widely circulated over China and Japan. The medical works of the Chinese show them to be totally ignorant of medicine as a science. Anatomy has never been studied, and they do not comprehend the circulation nor the functions of the viscera. It is believed that this series of treatises will spread much useful information among their practitioners, and induce attention to medical science. [*Med. and Surg. Reporter.*

*Light the only cause of Purulent Ophthalmia of Infants.*—Mr. Ballard has written an original paper upon the above subject. The generally received opinion that the disease is the result of contact with vaginal secretions is disputed,—1st, because the disease does not appear until several days after birth; 2d, extreme cases of leucorrhœa and yet no ophthalmia if the room was kept dark; 3d, no leucorrhœa and yet ophthalmia, the room being kept light.

The proofs in favor of light being the cause of the disease are, 1st. Expose an infant to bright light, and you can prognosticate the occurrence of the disease. 2d. It never prevails if the child is kept in the dark. 3d. That many cases had been cured by obscurity only.

*London Lancet.*

Prof. E. R. PEASLEE has been transferred to the Chair of Obstetrics and Diseases of Women in the New York Medical College, made vacant by the resignation of Dr. B. F. Barker, and Prof. Austin Flint, Jr., late of Buffalo, has been appointed to the Chair of Physiology and Microscopy. Dr. Flint holds the same chair in the Buffalo Medical College.

[*Med. and Surg. Reporter.*