ORIGINAL AND ECLECTIC.

ARTICLE XX.

Remarks upon the Vertex Presentations of the Foetus. By C. C. Howard, M. D., of Lowndesboro', Ala.

Professor L. A. Dugas:

Dear Sir—The principal object of this communication, is to offer some theory, to account for the great frequency of the first position, Vertex Presentation of Baudelocque; and so little do I find on the subject, in the obstetrical works examined, that I indulge the hope that this paper will be read with some interest, if derived alone from considerations of its novelty.

Before proceeding, however, I desire to say, that the two most prominent opinions as to the cause of the relative frequency of the head presentation, I regard as very improbable. The first, as well, perhaps, as the most generally received opinion, to which reference is made, is that the child is dependent from the cord, and the attachment of the navel string being nearer to the pelvis than to the head of the child, the head therefore hangs downwards.

In the latter stage of pregnancy, the child certainly cannot present the cranial or caudal extremity, as if by chance; yet, in the earlier stage of pregnancy, this can be done, as is implied in the opinion disagreed to. This ready possibility of the foetus to be turned in the uterus, I have no doubt, exists long after the
cord is entirely too long to suspend the foetus, if, indeed, it ever did. This, therefore, is of itself sufficient reason for me to reject the theory. The uncertainty, too, of the point of the placental attachment, presents strong objection to it; both of which objections are urged by others. But, the character and known office of the cord present other and insuperable objections to the theory. What is more improbable, than that two or three little blood-vessels would be used in this way, to accomplish so important an end, as that, the accomplishment of which is so necessary to the population of the world? What can be more important than to keep up a healthful and regular circulation between the foetus and placenta? And what would be better calculated to disturb this circulation than this very suspension?

The other somewhat prominent opinion, to which I desire to offer some objection, is that advanced by M. P. Dubois, in a memoir read at the Academy of Medicine in 1833, as I learned from M. Chailly's work on Midwifery, page 64—viz: "An instinctive determination of the foetus itself presides over the accomplishment of this law, and this instinct is proved," &c. It is an almost universally admitted fact, that the head of the foetus in utero is not only usually downwards in the last periods of gestation, but in the earlier also; and that after the sixth month, changes from one presentation to another are not frequent. If, therefore, instinctive determination of the foetus presides over this matter, does it not do so at an astonishingly early period of its existence—even before it possesses sufficient strength to make itself felt by the highly sensitive and ever watchful female, carrying it? I submit then, that this decision is made at too early a period, even for instinct. But, admitting that the foetus is instinctively led to select this position, what power has it at three or four months of intrauterine life to assume it? Even at birth, and for weeks after, it is so feeble, that if you place it in one position it cannot assume another. How, then, I ask, is it possible for the foetus, unaided by gravity, to assume that most difficult of all positions, viz., the inverted? Aye, more, to put its head to a certain point and keep it there, opposed by the force of gravity. The new born infant has strength to seize the mother's breast and
extract nourishment from it; the young of the opossum may graft themselves on the tits of their mother; but the foetus perform the manœuvres mentioned above: never, never.

Is it not much more reasonable, than either of the theories remarked on, to suppose that the embryo approaching a state of floating in the liquor amnii (as it does), and being easily agitated in that liquor by the movements of the mother (as it is), under the force of gravity finds a resting place, and the cranial extremity being heavier than the caudal, thereby much oftener obtains and occupies the region of the os uteri? Says Moreau, page 96, (speaking of the first theory,) "This mechanical explanation would be satisfactory and readily admitted. 1st, if the insertion of the umbilical cord were the same at all periods of intra-uterine life; 2d, if this cord were shorter than one half the body of the foetus; 3d, if the placenta were always inserted into the fundus of the uterus; so that the foetus would be constantly fixed to the fundus of the organ, and suspended in the amniotic fluid by a very short cord." These objections to the theory founded on suspension by the cord, are the valid objections, which, however, in that last proposed are removed, by attributing all necessary suspension to the liquor amnii. The difference between the two theories being in the means of a partial suspension, it may be well to notice two or three objections made by M. Dubois to the first theory, as they will apply with equal force to the last. 1st, the fact, "that he has repeatedly plunged, horizontally, into a quantity of water, a dead foetus, and has uniformly observed that all the parts of the foetus descend with equal rapidity," has no bearing on the question; for neither gravity, nor the instinctive effort of the foetus, nor any other ordinary influence would cause the head to remove from the fundus to the os, since the longitudinal diameter of the foetus, is much greater than the transverse diameter of the uterus, at full term. Besides, this objection is entirely yielded in a fact involved in this 2d objection: "Before the full term, head presentations are less frequent, although, at this period, the head is really heavier than the pelvic extremity." But why are head presentations less frequent in premature labours? They ought not to be, if instinct presides over this matter; for the law of instinct is a very certain and safe law. So is the law of gravity; and if this
last governs in this case, we ought to expect that head presentations would be less frequent in premature labours; because, the cavity containing the foetus is much more spheroidal in the first six months of gestation, than in the last three. 3d. He still further adds, says M. Chailly, (for I have never seen the memoir;) that "in women who, during their pregnancy, retain the horizontal posture, the foetus as commonly presents the head." Though there would be some decided difference in the position of the uterus, whether the woman be in the horizontal or vertical posture during pregnancy, yet, "in* both cases the os uteri, in consequence of the inclination forward of the pelvis, is" (usually) "the most depending part of the organ." Again: it must, I think, be a very small number of women who, in any strict sense, during their pregnancy retain the horizontal posture. 4th. In foetuses who have a tumour on the pelvic extremity, the tumour being even heavier than the head, this latter still presents. Like the cases just referred to, these last are very rare, and it is not shown by any means, that, in a number of such cases, the proportion may not be changed: the position may be determined before the tumour destroys the usual relation of weight. Lastly, those accidental causes which sometimes carry the head to the fundus, might, under reversed circumstances, carry the breach there.

Concerning the objection, "that in animals, where the inferior portion of the organ does not correspond to the cervix, but to the fundus, head are more common than footling presentations," I cannot speak advisedly. But even if it is so, without any modifying influence, I do not see in that any sufficient reason for rejecting a theory, in so many respects plausible, when applied to a very different animal. Notwithstanding so much more space has been occupied with the question of the cause of head presentation than was intended, yet, before entering on the subject proper, I desire also to refer to the cause of the presentation of the vertex—not that I have anything whatever new to say on that subject; but, because it may enable the reader more readily to comprehend the theory about to be proposed.

In consequence of the relative size of the foetus and the cavity it occupies, it is compelled to fold itself up in some way, in order

* Moreau.
to find accommodation, ordinarily. The most natural, indeed the only natural way of doing so, is by flexion, thus: the thighs are bent on the abdomen; the legs on the thighs; the arms on the breast; the chin approaching the breast, and the entire spine more or less flexed. Now, as labor begins and progresses, so, the relative size of the foetus and the cavity in which it is contained is diminished, and before the uterine contractions are sufficiently great, to thrust any part of the foetus into the superior strait, the chin has been compelled to approach the breast, so that the vertex becomes the cranial pole. But the vertex not only becomes a pole, for the uterus, compelling the diameters of the foetus to correspond with its own diameters, the vertex farther becomes the presenting part.

Allow me now to say, that the foetus can have little to do, by instinct or otherwise, with its birth or position in the uterus, but acts as it is acted on. Farther, that it is not from mere chance that we have one position oftener than another, or that it is so merely because it is so, i.e., that there is a natural law consisting of the abstract will of the Creator; but, that the manner in which a foetus presents in parturition is determined by its relation to the cavity it occupies, and this relation is such, that those presentations and positions which are most favorable to the mother and the child, always have been, and will be, infinitely the most frequent. We are not to understand, then, that the 1st position of vertex presentation is to be expected, merely because it has been the most frequent heretofore; and we are not to practice the art of midwifery ten or twenty years, before we acquire confidence in this expectation; but, in my humble opinion, we are to expect this position in consequence of appreciable influences, generally existing. What those influences are, it is the object of this paper to define.

That the reader may see the points aimed at, it may be well to say at once, that the theory about to be proposed rests upon a certain relative configuration believed to exist between the foetus and the cavity in which it is contained; which cavity has for its walls, not only the uterus, but the abdominal walls also. Added, to this configuration is the force of gravity.

Let us now turn our attention to the shape of a foetus folded up in utero, as it is compelled to be to find accommodation. Is
it not evidently erroneous to assume, that the lateral and antero-posterior diameters of this body are of equal length? With the chin approaching the breast, and the spine flexed—admitting that the anterior face of this foetus is curved at all—is it not certain that the line describing that curve, is not equal to the line describing the certain curve of the posterior face? If these questions are answered in the affirmative, (and they ought to be,) it is seen at once, that this folded up foetus does not form an oval, as is sometimes said. But, for lack of a better term, we consent to call it an ovoid, flattened on three sides, viz., anteriorly and laterally. It would be fortunate for me had I the advantage of plates; but it must suffice to refer to any of the plates in our midwifery works, shewing the 1st position of a foetus in utero, head presentation. When the chin is removed from the breast, the foetus more nearly approaches an oval; but before, the posterior line from pole to pole, never should be mistaken for the anterior line from the same point to the same point. The above description is certainly very imperfect, and very far from being satisfactory to the writer; but the light in which the shape of the foetus in utero is to be viewed, is so far indicated, that the indulgent reader will obtain the general idea, and form, in his own mind, a distinct notion of said shape, which he is now requested to do; and I ask, can he doubt that said foetus can be better accommodated, when looking towards a vertical line intersecting the right sacro-iliac symphisis, than in any other position?

But let us proceed to state some facts in relation to the brim of the pelvis, and the abdomen.

The anatomical peculiarities of the brim bearing on the question, viz., the promontory of the sacrum posteriorly, the length of the antero-posterior diameter of the superior strait, as compared with its other diameters, and with the occipito-frontal diameter of the foetal head, are generally and sufficiently distinctly set forth in obstetrical works, as determining the position in which the head in labour is compelled; but I submit that these peculiarities have little less influence during the latter periods of gestation; and as by this arrangement the head is better accommodated during labour, so it is better accommodated during the last weeks of gestation. It is not to be understood, then,
that we very frequently have the 3d position, vertex presentation, and the antero-posterior diameter of the foetal head, being so much greater than the antero-posterior diameter of the superior strait, the position is necessarily changed to the 1st, or any other; but, on the contrary, before, and for weeks before the labour begins, the relation of the head to the brim is such that the position is, must, and will be the 1st. I do not think, however, there would be any great difference in the accommodation given to the head, in the 1st, 2d, 4th, or 5th positions, but the peculiarities of the pelvis would only affect these as compared with the 3d and 6th. It is, therefore, not to the peculiarities of the pelvis that I would attribute the greater frequency of the 1st position, as compared with the 2d, 4th, and 5th, but to the abdomen.

The anatomical facts in relation to the abdomen, to which attention is called, are—1st. That its posterior wall is composed of materials which cannot be made to give way to an enlarging uterus, but will compel said uterus to seek room in some other direction: Farther, that this wall, so far from presenting a concavity to accommodate a convexity, such as is offered in the back of a foetus when folded up in utero, rather presents a convexity, to turn off the back of said foetus.

2d. That the anterior and lateral walls are just the reverse, in the above respects, of the posterior wall: they are yielding; and the muscles, which contribute so much to make up these walls, being attached to the bony frame above, below, and laterally, yield in just such a way as to form a concavity to receive the convexity of the foetus's back.

3d. The right hypochondriac region is occupied by the largest solid viscus in the body, viz., the liver; and in the latter stages of pregnancy, when the uterus would intrude upon its location, the attempt is so far resisted that the right side of said uterus is compressed—its diameter here diminished, and the body of the contained foetus is thus naturally thrown around to the left side: so that, when a foetus lies with the head bearing the relation to the brim in the first position, its body is in the abdomen corresponding to the same position.

The 1st position, then, is not only the most easy and natural in parturition, but also in the latter stages of gestation.
A probability in favor of this view of the subject, is derived from a fact already mentioned, viz., premature labours are less apt to be of this position, than labours at full term. The probability is inferred in this way. In the latter stages of pregnancy, the abdomen is more nearly filled than in the first stages, and the peculiarities which have been pointed out are more fully developed.

Again: turn the foetus round, so that its back will nearly or quite correspond with the back of the mother, (and this you will do if you turn the face to the pubes or acetabulum,) and what is the inevitable tendency of the contractions of the abdominal muscles, so common in pregnant women? Certainly, in the event they fail to throw the foetus round, they are to produce extension, instead of flexion. If they produce extension, the effect must be unpleasant to the mother, and perhaps to the foetus also; and this, necessarily, if the foetus occupies less room in flexion than extension; and if the former remark, 'that the foetus is compelled to fold itself up in utero in order to find accommodation ordinarily,' be true. To be more explicit: When a foetus lies with its back corresponding to the maternal back, the abdominal muscles expend their force upon its cranial and caudal extremities; and the maternal back successfully resisting these contractions, the foetus is either thrown round or extended. If extended, uneasiness is produced at least on the part of the mother; this uneasiness farther induces contractions, which, ordinarily, compel the foetus to pass round, and accommodate itself to the cavity containing it. The exceptions are numerous, it is true—about as many as present the head other than in the 1st position; and though to state the causes of those exceptions might throw more light on the subject, yet, we will only say that they are mainly to be found in the absence of a healthful and favorable relation in size of foetus and cavity.

The second great cause determining the position of a foetus in utero, both in the last stages of gestation and in parturition, is to be found, I submit, in the force of gravity. About the period to which reference has been so often made, though the woman, to maintain the centre of gravity, may throw the shoulders and body back, (which last increases the convexity of the posterior wall already referred to,) yet the posterior wall of the
abdomen will not be carried so far back, as that the foetus will occupy a vertical position—will be balanced on its head. So far from this, it will be removed several degrees from a perpendicular. For the reasons apparent, if not already given, this leaning will be anteriorly or laterally, and the evident tendency is to the first; but counteracting agencies, as I have tried to shew, and as experience, I think, shows, make it left-laterally, usually. Now, as by a well-known law of gravity, the heavier seeks the lowest point, and as the posterior side of the foetal ovoid is the heavier side, so it should take the most dependent position.

The foetus, then, lying in utero, in a position corresponding to the 1st position of Baudelocque, has its diameters to correspond more nearly with the diameters of said cavity; and the tendency to deviate from this position is, usually, prevented by the action of the abdominal muscles, aided by the force of gravity.

Since writing out these remarks originally, I have found the following, in the beautiful and able Treatise on Midwifery, by F. J. Moreau, in which I am not very sure that he did not intend to put this whole matter in a nut-shell. Treating of "attitude and position of the foetus in utero," page 95, he says—"Generally, this position is as follows: the head rests on the os uteri; the hips and feet are toward the fundus of the organ; the back is turned toward the anterior paries of the abdomen of the mother; and the belly toward the lumbar vertebrae of the latter." Then, in the last paragraph on that page, he says, "this position may be explained by the laws of gravity, and in a purely mechanical manner. Thus, the back of the embryo, by virtue of its convexity and weight, should lodge in the most concave part of the uterus, which is the anterior surface; it can adapt itself more accurately to this surface, which coincides with the anterior soft wall of the abdomen, whilst the lumbar column and a portion of the viscera of the mother, which project posteriorly, accommodate themselves more easily to the unequal surface of the anterior region of the foetus."

So far as I am aware, nevertheless, these remarks attribute to the abdominal parietes an importance not, hitherto, fully recognized. Much that has been said may be ideal. I do not doubt, however, in the least, that the facts mentioned have their influ-
ence, and that influence is great. But without deciding on the value of these remarks, further than is indicated by writing them out, and forwarding them to you, they are placed at your disposal, with the assurance, that if in your estimation they will not be read with interest or profit, you will do me a favor to suppress them.

Although the practical bearing of the above theory has not been referred to, and though it is regarded as being great—since this article has been extended much beyond my design, I only desire to mention, in this connection, the question of position or decubitus in parturition.

In the theory, is contained the best reason, in my judgment, yet offered, for the left side. In comparison, the questions of convenience to the accoucheur, and modesty of the female, fall into insignificance. Still, they do not much affect an objection to that position, in the last throes of labour, which I have long had, and which is, there seems to be less force and agreement in the expulsive efforts in that position. But, without contraindication, I shall, unhesitatingly, hereafter give preference to the left side, at least until the head engages in the superior strait.

May 22d, 1854.

ARTICLE XXI.

Additional Remarks upon a case of Hepatic Abscess. By H. R. Casey, M.D., of Columbia county, Georgia.

Dear Doctor—I wrote you some time since, giving you an account of a Case of Hepatic Abscess that had occurred in my practice; which letter you placed before the medical public, through the columns of your valuable Journal. (Vide S. M. Journal, Sept. No. 1853.)

If the circumstances giving origin to that letter, were of sufficient importance to justify me in presenting them to your notice, I think I am authorized in giving you this letter, which may be taken as a sequel to the former.

In the conclusion of my former communication to you, I stated that the different orifices which gave vent to the suppuration, were at the time of my writing, all cicatrized, and the woman in the enjoyment of fine health.
For some months thereafter I had frequent opportunities of meeting with her, and the facial expression, the lively air, the embonpoint, in fact, every appearance was the heraldry of health. She remarked to me, that she felt better and stronger than she had done for years.

Some short time subsequent to this, she became enceinte, a situation considered most interesting and desirable by "ladies who love their lords."

But the consummation of her hopes was not to be realized.

About the third month of Utero-gestation, she was taken down with pains and a flowing, and I was summoned to see her. I immediately put into requisition the means usually resorted to on such occasions to prevent the threatened catastrophe.

There being a decided proclivity in the uterus to throw off its contents about this period of its gravidity, (she having aborted two or three times,) I did not hope, nor did I expect to succeed, particularly as the uterine contractions were very forcible, and the hemorrhage great. Things continuing thus and rather aggravating for two or three days, and being satisfied that the maternal and fetal connection was severed, I administered the vinum ergot which soon had its effect in the expulsion of a dead foetus.

She was put to bed, and had no untoward symptoms. She had a good "getting up," was doing well in every respect and began in three weeks from the accident to attend to her domestic duties.

Her circumstances in life being such as disqualified her from having menials to go at her bidding, and come at her beck, she unfortunately sought the wash tub too early, and at night after a hard days washing she was taken down, and the following morning I was sent for.

I found her quite ill, with great febrile excitement, pulse 130, skin hot and dry, complained of no particular pain, but a general malaise. I gave her forthwith 10 gtt. verat. viride, and directed its repetition in 7 drop doses until the pulse was brought down to 90, when I directed 15 grs. sulph. quinine to be given. At bed time to take 12 grs. blue mass, followed in the morning with sulph. magnesia, and after its operation, she was again to resort to the great febrifuge.
Called to see her on the third day, found her somewhat improved, but still with considerable fever, pulse 98, strong, tongue coated, with pain in right side. Fearing the suppurative termination, and dreading to go through the scenes that were enacted on a former occasion, I boldly set to work by the aid of all those means that would be conducive to that end. To ward off the dire tendency, I put her upon mercurial alteratives, had croton oil freely rubbed over the right hypochondrium, and restricted her as to dietetics. I continued this treatment for several days, using saline purgation on alternate days, but found at length I would have to give up the contest. The inflammation would not yield to my treatment, and suppuration became evident. Again the hectic, like the bright red coral of the ocean, began to play upon the cheek.

A change in her symptoms called for a change in my Therapeutics. The depressing treatment was laid aside, and an invigorating one, with a generous diet, substituted.

Directing my efforts to the pointing of the abscess, externally, I was in a few days called on to use my lancet. Finding a fluctuating tumour opposite the side of the affected viscus, I punctured the most prominent point, and gave vent at once to about two gills of matter. The puncture was directed to be kept open, and warm althea poultices applied.

From this time to the closure of the wound in health, I suppose some half a pint more of matter escaped.

She recovered from this attack sooner and with less constitutional disturbance than in her former trial, owing in a great measure to the fact that in the present instance, the abscess was smaller, judging from the relative amount of matter excreted.

There was nothing peculiar in the treatment or subsequent management of the case. The obvious indications being to build up the system, such remedies were ordered the known tendency of which was to increase the stamina.

She was put upon mineral tonics, and iodine and an appropriate dietetic treatment instituted.

I am happy to have it in my power to say that my patient is again in the enjoyment of fine health.

In the course of my remarks in the former letter to you, I
stated that a suppurative termination of this Phlegmasia in this country was of rare occurrence, and that its existence was even denied by some.

This unfortunate yet fortunate lady stands to-day a living witness of the susceptibility of the liver to suppuration, and to re-suppuration.

I will make one other remark in connection with this case.

This lady, previous to her first attack of hepatic abscess, had been for several years a victim to Hæmorrhoids, from which at times she suffered greatly. During the treatment of her case she found that her piles had disappeared and from that time she has never felt them.

Now, in her case, in all probability, the piles were caused by her liver disease checking the return of the blood of the hæmorrhoidal veins. Her liver being torpid, congested and enlarged, acted as a mechanical impediment to the passage of the blood of the portal circulation; and hence the induction of piles. This, however, is but a speculation, which I think, however, warranted from the facts of the case.

May 25th, 1854.

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ARTICLE XXII.

A Case of Ulceration of the Cervix and Os Uteri, successfully treated. By A. F. Attaway, M. D., of Madison county, Georgia.

The following is a case brought to me on the 2d day of May last, supposed by the friends of the patient to be one of prolapsus uteri, for which they wished me to operate. By interrogating the patient a few minutes, I was convinced of their delusion, and induced to believe that I had a case of ulceration to contend with.

The woman was thirty years old; had given birth to three children, the youngest of which was six years of age. Six weeks after the birth of this child she got her feet wet, in consequence of which, she thinks, she had a sudden and violent attack of sickness. The best medical aid was procured that could be had, and, after an illness of several weeks, her health
was only partially restored. Since then, she has been subject, more or less, all the time, to pain in the loins; a sense of heat and forcing in the pelvic region; general debility, and unnatural vaginal discharges. She had been under the care of various physicians for the last five years, without realizing any permanent benefit.

By the touch, per vaginam, I found the uterus considerably swollen, and slightly prolapsed; the cervix and os soft and patulous, presenting an irregular surface, in consequence of cicatrization, and very sensitive to pressure, the least of which caused lancinating pains in the pelvis, and more especially in the region of the ovaries. Upon introducing the speculum, an extensive ulceration of the cervix and os uteri was perceptible, and that part of the organ which was not covered with deep ulcers, (for there was more than one, varying in size from that of a pea, to a shilling, or larger,) was of a deep red or livid color, presenting such an aspect of malignancy, that I was disposed to doubt the efficacy of any treatment I might prescribe. I, however, treated the case as follows:

Patient confined to recumbency as much as possible, without too much inconvenience—the speculum introduced, and the organ well cleansed with a camel-hair brush, and a piece of fine sponge affixed to the end of a stick of whalebone, dipped in the suds of Castile soap—then, with the best quality of nitrate of silver, I cauterized the ulcerated surface thoroughly. This was done once every three days, for three weeks; after this, only once a week, for nine weeks—at the lapse of which there was no ulcer to be seen, and the organ looked of a natural and healthy color.

In addition to the above, I washed the vagina and cervix uteri, once a day, with cold water, impregnated with Castile soap, followed by a solution of the sulphate of zinc, 10 or 12 grs. to the ounce of water. Her diet was nutritious, and easy of digestion; her bowels were attended to; and occasionally chalybeates were used. Such was the treatment which resulted in a permanent cure. The woman is now in vigorous health. Nine months have elapsed without any symptoms of the disease returning.
ARTICLE XXIII.

The Leaves of the Magnolia Tripetala as a Dressing for Blisters. By Jno. Stainback Wilson, M. D., of Airmount, Alabama.

As this Journal is eminently practical, it is hoped that a brief notice of the remarkable species of Magnolia which heads this article, will not be unacceptable.

The M. Tripetala is known by the common names of "umbrella tree" and "wild cucumber," the latter being, we think, the most common in Alabama, where it grows abundantly; although it seems that this name is most generally applied to the M. Acuminata.

The species of Magnolia under consideration, is one of the most remarkable productions of the United States, and will not fail to attract the attention of the most unobservant, by the wonderful size of its leaves and the beauty of its flowers: "the former are eighteen or twenty inches long, by seven or eight in breadth," and even larger than this: while the latter are of corresponding magnumdite, being "seven or eight inches in diameter." We are informed that "this species of the Magnolia extends from the northern parts of New York to the southern limits of the United States," but we have never seen it in any part of the State of Georgia. Still, as it is no doubt a common production of the rich lime lands of the South Western and Western states, its "medical properties and uses" should be known to the physicians of that region; and it should not suffer unmerited neglect like too many of our useful indigenous remedies. The M. Tripetala (the bark) is highly esteemed by the common people as a tonic, and some of them even consider it an infallible specific in dropsey; this of course cannot be conceded, but the estimation in which it is held is, at least, an evidence that it is not by any means destitute of medicinal virtues. And in addition to this, we have the higher evidence of our Dispensatory that it has been found useful in chronic rheumatism, and intermittent and remittent fevers. We have had no experience with it in the treatment of the above diseases; but we have used the leaves as a dressing for blistered surfaces with
satisfactory results, and the main object of this article is to commend this application to the attention of the profession.

We will simply, in conclusion, mention why we think this dressing should be considered worthy of notice: 1st. The leaves of the Magnolia are not officinal, and it may therefore, be presumed, that their uses are unknown. 2nd. We think that they are equally as good as the collard or cabbage leaves, so much used; while they are often more readily obtainable, much larger, and less offensive in smell.

Before using, we scald them, but think it possible that they would answer every purpose, if applied in the natural state.

**Experiments with Phosphorus, and remarks upon its Dose and Action, when given in the form of Alcoholic Tincture or Solution.** By Wm. M. Boling, M. D., of Montgomery, Alabama.

More especially since I have used in my practice the Veratrum Viride, the discovery of an article equally certain, prompt, powerful and reliable in its action as a cardiac sedative, but without some objections that have seemed to me to pertain to the operation of this remedy, I have thought a great desideratum. By an article presented to the Alabama State Medical Association, at its meeting in Selma, in December, 1852, with impressions that I had previously received in regard to it, my attention was directed to the Gelseminum Sempervirens, or Yellow Jessamine. I entered upon my experiments with this article, somewhat sanguine of success, but disappointment was the result. After a number of experiments with it, in which certain effects said to result from its use, such as dimness of vision, trembling, &c., were produced in a marked degree, I could not discover that it at all reduced the frequency of the pulse. My experiments with it were upon healthy subjects.

In a well written and exceedingly plausible paper, which was published in the New Orleans Medical and Surgical Journal for January, 1854, on Pneumonia, by Doctor S. Ames, of Montgomery, Phosphorus is mentioned as a remedy of great value in this disease, and its curative influence, if I mistake not, ascribed to a cardiac sedative operation. I have been induced to make some experiments also with this article; the results of which I will give.

Two preparations of the article are mentioned by Doctor Ames, to wit: a saturated Tincture in Anhydrous Alcohol,
which he supposed to be equal in strength to the Ethereal Tincture, or to contain four grains to the ounce; and a diluted tincture, made by adding nine parts of alcohol to one part of the saturated tincture. As a dose of the saturated tincture, from half a drop to two drops are spoken of, but it is stated that the medicine "cannot be continued in the smallest quantity just mentioned for any great length of time, without inducing considerable disturbance of the stomach, as shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." It is also remarked that "its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome, or even dangerous, after several repetitions at intervals of three or four hours;" and that "this quality was developed in one instance by repeating it in a dose of two drops of the strong alcoholic solution three times at intervals of twenty-four hours." In regard to its sedative action, Dr. Ames remarks, that "its sedative or contra-stimulant, is its medicinal or therapeutic action," and in a note, makes the following statement: "Two young gentlemen, my personal as well as professional friends, have been recently engaged in some experiments to test the effects of phosphorus on persons in health; they themselves being the subjects of the experiments. These gentlemen (Doctor Pollard and Doctor Oliver) found that a single dose of two drops of the saturated alcoholic solution invariably reduced the force and frequency of the pulse. The changes in frequency ranged in the number of pulsations to the minute, between eight and twelve beats. A change was perceptible in about twenty minutes, which reached its maximum in from an hour to an hour and a half."

Doctor Ames, however, for reasons given in his paper, prefers the weaker solution, and his method of administration is to add, sixteen drops of it to four ounces of water, of which he gives a teaspoonful every "third or fourth hour—usually every fourth." As there are thirty-two teaspoonfuls in four fluid ounces, the quantity of the diluted tincture thus given at a dose, would be half a drop. This dose Doctor Ames prefers "after many trials with larger ones."

I have been thus particular in referring to the peculiar effects said by Doctor Ames to result from the use of these two preparations of phosphorus, but more especially to the dose and method of administration, that comparison may be conveniently instituted with the dose and manner of administration followed by myself, and any fault or deficiency in the latter, detected and pointed out. I feel inclined to pursue the subject further, if I have failed in any way to conduct my experiments satisfactorily.
The pulse—as most persons know, and as any one may satisfy himself of—will be found to vary in many persons, if not in every one, somewhat in frequency at different times in the twenty-four hours; and often, even when felt at short intervals, will be found so to vary from trivial or inappreciable causes. To prevent any fallacy from this circumstance, it will be noticed, that in my experiments on some days, the frequency of the pulse at different hours is given, though the phosphorus was omitted. A comparison may thus be made between the pulse on the days that the phosphorus was, and on the days that it was not, given.

My first subject was Sam, a healthy mulatto boy, seven years old. On the 25th of February, having kept him in the recumbent posture sometime, his pulse being 102, at 1 o'clock, P. M. I gave him seven drops of the saturated tincture. At 2 o'clock, P. M., pulse variable, from 90 to 108. 3 P. M. (subject asleep,) pulse 94. The dose of seven drops were repeated. 4 P. M., (subject asleep,) pulse 104.

Feb'y. 26th—1 o'clock, P. M., pulse 100; 1½ o'clock, pulse 114; 2 o'clock, pulse 100. No phosphorus was given to-day. The subsequent experiments with Sam were with the diluted tincture. The experiments already given were conducted with the subject in the recumbent posture. In the following, he was generally, when a dose was to be given or the pulse counted, called in from play.

March 10th—11 A. M., pulse 100; half a drop of the diluted tincture given. 12 M., pulse 103; half a drop given. 1 P. M. pulse 100 to 106; one drop given. 2 P. M., pulse 104; two drops given. 3 P. M., pulse 108; two drops given. 4½ P. M. pulse 92; two drops given. 5 P. M., pulse 88, and five minutes later, 96.

March 11th—Half after twelve, pulse 96. 1 P. M., pulse 104. 2 P. M., pulse 100. 3 P. M., pulse 104. 5 P. M., pulse 92. No phosphorus was given to-day.

March 12th—10 A. M., pulse 98; 50 drops given. 11 A. M., pulse 102. 1 P. M., pulse 96; 100 drops given. 2 P. M., pulse 104. 5 P. M., pulse 100; 200 drops given. 5½ P. M., pulse 100; 200 drops given—making in all 550 drops in seven and a half hours. 6½ P. M., pulse 107. 7½ P. M., pulse 94.

March 13th—8 A. M., pulse 92; 500 drops given. 9½ A. M., pulse 92.

March 17th—4 P. M., pulse 104; nine hundred and ten (910) drops, being exactly one ounce, given at a dose. Here a child seven years old took, at a single dose, one thousand eight hundred and twenty (1820) of Doctor Ames' doses for the adult. Half after four P. M., pulse 116. 5 P. M., pulse 120. 7 P. M., pulse 104, and the subject "feels very well."
The two hours immediately succeeding each of the last two doses, Sam spent riding in my buggy with me, and attended to my horse at the different stopping places; and though I noticed him carefully, no appreciable effect was manifested—the variation in the pulse alone excepted—otherwise than that he seemed a little merry, which was fairly, I think it will be admitted, attributable to the alcoholic part of the dose—about half an ounce in one instance, and an ounce in the other. It would scarcely seem necessary, while the large quantity of alcohol would render it improper, to repeat, or to carry the joke any further with Sam.

My second subject was a negro man, about 25 years old, under treatment by means of the bandage and the recumbent posture, for a small ulcer on the foot. Otherwise, he was healthy. He lay in my office during the experiments, which were all conducted with the saturated tincture.

Feb. 27th—1 P. M., pulse 78; ten drops given. 4 P. M., pulse 72.
Feb. 28th—12 M., pulse 78; ten drops given. 1 P. M., pulse 80. 3 P. M., pulse 72. 4 P. M., pulse 78.
March 1st—11 A. M., pulse 74. 2 P. M., pulse 74. 3 P. M., pulse 68. 4 P. M., pulse 80. No phosphorus was given to-day.
March 2d—12 M., pulse 72; 20 drops given. 1 ½ P. M., pulse 71. 2 P. M., pulse 67.
March 3d—12 M., pulse 74; 25 drops given. 2 P. M., pulse 74; repeat the dose. 3 P. M., pulse 67. 3 ½ P. M., pulse 72.
March 4th—11 A. M., pulse 80. 12½, pulse 72. 3 P. M., pulse 68. No phosphorus was givin to-day.
March 5th—12 M., pulse 74; 40 drops given. 1 P. M., pulse 74. 3 P. M., pulse 66.
March 6th—9 A. M., pulse 70; 60 drops given. 10 A. M., pulse 71. 1 P. M., pulse 74; 70 drops given. 2 P. M., pulse 72. 3 P. M., pulse 80. 4 P. M., pulse 80.
March 7th—12 M., pulse 78. 12½ pulse 64; 80 drops given. 2½ P. M., pulse 76.
March 8th—Half after twelve o'clock, pulse 74; 100 drops given. 1½ P. M., pulse 76. 2½ P.M., pulse 74. 3 P.M., pulse 74; 10 drops given. 4½ P. M., pulse 84; 21 drops given. 6½ P. M., pulse 84.
March 9th—8 A. M., pulse 74; 2 drops given. 9 A. M., pulse 74; 30 drops given. 10 A. M., pulse 76; 40 drops given. 10½ A. M., pulse 76. 2 P. M., pulse 70. 3 P. M., pulse 78; 50 drops given. 3½ P. M., pulse 75; 50 drops given. 4 P. M., pulse 84. 5½ P. M., pulse 84; 100 drops given—making in all 272
drops of the saturated tincture administered during the day. 6½ P. M., pulse 82.

March 10th—7½ A. M., pulse 76. 8½ A. M., pulse 70. 10 A. M., pulse 80. 11 A. M., pulse 80. 12 M., pulse 82. 1½ P. M., pulse 76. 2 P. M., pulse 68. 3 P. M., pulse 74. 4½ P. M., pulse 80. 6 P. M., pulse 82. No phosphorus was administered to-day.

March 11th—No phosphorus was given, but the pulse being counted at intervals, was found to vary about as it did yester-

day.

March 12th—10 A. M., pulse 76; 100 drops given. 11 A. M., pulse 82. 1 P. M., pulse 74. 2 P. M., pulse 82.

March 13th—8 A. M., pulse 72; 2 drops given. 9½ A. M., pulse 80. 10½ A. M., pulse 84; 2 drops given. 1½ P. M., pulse 78; 2 drops given. 2½ P. M., pulse 80; 2 drops given. 3½ P. M., pulse 78; 2 drops given. 4½ P. M., pulse 80.

March 14th—9 A. M., pulse 74; 150 drops given. 9½ A. M. pulse 74. 12½ pulse 84. 1½ P. M., pulse 80.

March 15th—9 A. M., pulse 72; 200 drops of the saturated tincture given. 10 A. M., pulse 72. 11 A. M., pulse 74. The subject was questioned and examined particularly. There was no nausea or vomiting; no burning sensation, or feeling of op-

pression in the stomach or epigastric region; in fact, he says he feels "very well." 1 P. M., pulse 76. A short time before he took the two hundred drops he ate a hearty breakfast, and at 2 P. M., with decided relish, a substantial dinner of bacon, cabbage, potatoes and corn bread. 4 P. M., pulse 80.

How much further the dose might be augmented with safety and without appreciable effect, I am at present unprepared to say; but reasons I think will appear as we proceed, that will render it not very improbable that the quantity of alcohol rather than any suppositious quantity of phosphorus, the pre-

parations, as prescribed and given may contain, should form the only necessary limitation of the dose. To be ready for the press, I must here close my experiments so far as they are to be used in the present paper, though I shall probably continue them hereafter.

In my experiments, two methods of administration were principally pursued. In one, the tincture was dropped into a glass with water and given to the subject, sometimes before the entire disengagement of the whitish vapor that rises as the solution is mixed with water; and often a part of the vapor would escape with the first expiration after the dose was swal-

lowed. In the other method, the solution was dropped into a vial with water; the vial was then corked and shaken, and the dose poured into a glass when given. These are the two plans
pursued it would seem by those who have used the article here as a curative agent in pneumonia. The latter seems to be the one adopted in the prescription given by Dr. Ames in his paper. The preparation that I at first used in my experiments I obtained from the Apothecary from whom I am in the habit principally of procuring medicines for my own use, and who prepared it at my request; but though I had no reason to suppose that the article thus procured was not of good quality, I subsequently supplied myself with both the diluted and saturated tinctures from the Apothecary who prepared the tinctures used by Doctor Ames.

As to the effect of the article upon the pulse, others may judge from the experiments recorded as well as myself. To me as a sedative it appeared to be entirely null; and this I apprehend will be the ultimate verdict of the profession. From my previous reading I had been led to expect a stimulant action from the larger doses, but no appreciable effect of the kind was observed that was not fairly attributable to another and more obvious cause than the phosphorus, to wit, the alcohol. This I think, as the subject is further developed, will not seem at all strange. In not one single respect was any effect that I could discover produced by the phosphorus. In no instance was there either disturbance of the stomach, nausea, vomiting, a burning sensation or a feeling of oppression at the epigastrium, though from the quantity of alcohol alone in the last two doses taken by Sam, something of the kind might not unreasonably have been expected. In no instance was there any alteration in the appearance of the tongue, diminution of appetite, or appreciable alteration or disturbance of the system in any way. My subjects ate with a good appetite, drank, slept, and in every respect deported themselves—while all their functions apparently were performed—as if nothing had been given: a temporary alcoholic excitement in the case of Sam after the last two doses alone excepted. When my subjects were asked how they felt after taking a dose, large or small, the answer generally given was, "very well," or "first rate."

It is stated in effect by Doctor Ames, that when the dose of phosphorus is enlarged beyond a certain point, the sedative influence is counteracted or prevented by a stimulant action, the result of the "local inflammation it excites" in the stomach and bowels. Now, as nothing of this preventive or counteracting character occurred in my cases—the sedative action of the article therefore not being in the slightest degree interfered with, seeing that in the experiments mentioned by Dr. Ames the pulse was invariably reduced in frequency from eight to twelve beats in the minute in the course of from an hour to an
hour and a half by a single dose of two drops of the saturated tincture—to what an extreme state of sedation should my subject have been depressed who took two hundred (200) drops of the same at a single dose, and again one hundred drops at one dose, and two doses of fifty drops each, besides several smaller doses, making in the aggregate two hundred and seventy-two (272) drops, all in nine hours and a half; while the degree of inflammation of the stomach and bowels that should have occurred, but did not, is awful, even to think of.

Further, in regard to the stimulant action of these tinctures. It will be observed, that after the administration of several of the doses, a slight increase in the frequency of the pulse took place. Still, the variation does not seem greater, omitting the doses of the 13th and 17th of March taken by Sam (in which so obvious a cause for the excitement was present, as in one instance an ounce, and in the other about half an ounce of alcohol,) than on other occasions where the smaller doses were given, or when it was omitted entirely. If, however, it should be supposed by any that a stimulant action from phosphorus was manifested on the occasion referred to, the absence of all symptoms of the local inflammation of the stomach and bowels, to which Doctor Ames regards the stimulant action as secondary and consequential, would show that it could not in my experiments be fairly attributed to any such cause. It would seem not improbable then, admitting that any stimulant effect really was produced by the phosphorus, that the received explanation of authors of a diffusive operation may be the correct one. It will not be doubted, however, that phosphorus may be so administered as to produce a local irritant action upon the stomach, consequent upon which there would be a febrile reaction.

Besides the experiments given, I have also taken a good deal of the article; indeed, had taken it sometime before I commenced its administration to Sam. I took it at first for a while with the occasional omission of a dose on account of absence from my office on professional business; but in one instance I took for eight successive days as much as five drops of the saturated tincture three times a day without the omission of a single dose, and in no respect did I ever experience any appreciable effect from its use. Though in every instance I was careful to drop out the full number intended, of five drops, I did not regard it as a matter of any importance when a few drops, say a half a dozen or so, more than that number happened to escape, but took the dose as if but the intended number was contained in it.

In connection with this experimental practice upon myself,
I will again call attention to the views of Dr. Ames in regard to the effects of the tinctures of phosphorus and their dose. Thus he says, speaking of the saturated tincture, that it "cannot be continued in the smallest quantity just mentioned,—half a drop—" for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." Though he admits that in the quantity of two drops, "a single dose, or perhaps a few doses may be given with impunity," he would evidently regard any lengthened use of it in such a dose, as a very grave and serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses of two drops each, at intervals of twenty-four hours. It is most desirable that we should yet be able to discover and explain the cause of these discrepancies. While Dr. Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—being just ten times the quantity—a long time, and for eight days without omission of a single dose, without effect. While under his observation, from a cumulative action, dangerous effects resulted from three doses of two drops each, administered at intervals of twenty-four hours, being in all six drops taken in the course of three days, yet I have taken for eight successive days three doses of five drops each, or fifteen drops per day, without effect. Indeed, unless I should discover something in its action, which has never as yet been manifested in any of my experiments, from my own experience with the article, and with all the lights at present before me, I should not hesitate, were it not for the mere trouble of the thing, to continue it in the same manner for years.

In the healthy subject at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet not only did the subjects of my experiments take it in doses as mentioned, so immeasurably greater than the doses with which such effects are said by Doctor Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus, while they sometimes took it in the middle of the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions I myself having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my vial, taken the dose, and proceeded with my meal without disrelish or any subsequent manifest effect.
A word or two of explanation in reference to the dose recommended by Doctor Ames, that I may not be charged with rashness or temerity on account of the doses given to my subjects, may not be out of place. It will be seen that these took at different times doses of various sizes, from the favorite one of Doctor Ames—half a drop of the diluted tincture—up to two hundred drops of the saturated tincture.

It is supposed by Doctor Ames that the saturated alcoholic tincture contains four grains to the ounce. The diluted tincture would in that case contain four grains to ten ounces, or there would be one grain in two and a half fluid ounces. Doctor Ames' dose then of half a drop, supposing the drop to be equal to the minim, would be the one-twenty-four-hundreth ($\frac{1}{24000}$) part of a grain, there being twelve hundred minims in two and a half fluid ounces. But the drop is not equal to the minim. While there are but sixty minims in the fluidrachm—Professor Wood tells us that it takes one hundred and thirty-eight drops of alcohol to make a fluidrachm—it would seem scarcely probable that the small quantity of phosphorus held in the diluted tincture would cause any material variation in the size of the drop. In sundry trials made by myself in which several vials were used, I found that the number of drops of the diluted tincture necessary to make a fluidrachm varied with the shape of the vial and the thickness of the lip, from 100 to 125. Let us take, however, the smallest number, though the drops in that case will be found above the average size. Of these there would be just two thousand in the two and a half ounces of the diluted tincture, and each drop therefore would contain the one-two-thousandth ($\frac{1}{2000}$) part of a grain, consequently the half drop dose would contain the one-four-thousandth ($\frac{1}{4000}$) part of a grain; or there are four thousand of Dr. Ames' doses in a single grain. The strength of the saturated tincture is ten times that of the diluted tincture, consequently my subject who swallowed two hundred drops of the former at a single dose, took what was equal to four thousand (4000) of Doctor Ames' doses of the diluted tincture, and this without any effect.

The above estimates are all based upon the supposition of the correctness of the assumption of Doctor Ames, that the saturated alcoholic tincture contains in solution 4 grs. of phosphorus to the ounce, as the ethereal tincture it is said does. We are not told by Doctor Ames whether he has any authority for this opinion, nor does he speak of any experiments performed by himself to test the solubility of phosphorus in alcohol. It would appear to be then a mere supposition, that an ounce of the saturated tincture contains four grains. Without some experimental tests we cannot feel certain that it may not contain more, or
that it may not contain less, than the quantity mentioned. No author that I have consulted says anything as to the exact quantity of phosphorus that any given quantity of alcohol will dissolve. Many of our very highest authorities on the Materia Medica do not even speak of an alcoholic solution; and those who refer to it at all, at least all that I have consulted, speak of phosphorus as very insoluble in alcohol as compared with its solubility in ether.

In the absence then of any definite statements on the subject, I was led to institute myself a series of experiments with the view of ascertaining with greater certainty the solubility of phosphorus in alcohol. In a vial containing one ounce of anhydrous alcohol, I placed four grains of phosphorus, in another two grains, and in another one grain. At the end of fourteen days—the time usually considered necessary for the preparation of tinctures by maceration, the time directed by a majority of the pharmacopoeias—of the four grains, about one-fourth or less was dissolved; of the two grains, about one-half or less, and of the single grain, there still remained a portion undissolved. On testing these several tinctures by dropping them in water, they all gave off the white vapors as freely as any of the tinctures supplied to me by the apothecaries. It is fair I think to say then, that the saturated alcoholic tincture, instead of containing in solution four grains to the ounce, contains in reality but about one grain to the ounce. It may possibly be a little more; it would seem as likely to be less. The favorite preparation then of Dr. Ames—the diluted tincture—would contain one grain to ten ounces. Allowing, as heretofore, one hundred drops to be equal to sixty minims, or a fluidrachm—and this is a low estimate—we have in the ten ounces eight thousand (8000) drops. Eight thousand drops then of the diluted tincture will contain one grain of phosphorus, and one drop will contain the one-eight-thousandth (\(\frac{1}{8000}\)) part of a grain. The favorite dose of this tincture with Dr. Ames is half a drop, and the quantity of phosphorus consequently contained in it is the one-sixteen-thousandth (\(\frac{1}{16000}\)) part of a grain.

But it is very probable, even, that not all, indeed that but a small proportion of the almost inconceivably minute quantity of phosphorus contained in the dose is received by the patient. In using the saturated tincture or solution at the moment of contact between the drop and the water, a quantity of vapor is given off in the form either of phosphorus as such set free, or as one of its acids formed in consequence of chemical decomposition and combination. Possibly this may be the cause of the apparent nullity of effect in my experiments; and if so
we are necessarily forced to the supposition of an equal nullity, from the same cause, in the hands of others, who use the alcoholic solution suspended in water; and this is the method pursued by Dr. Ames. Now it does not seem at all improbable, on the contrary extremely probable, that the action or chemical change alluded to, attended with a proportionate loss, also takes place when the diluted tincture is combined with the water, though the disengaged vapor, on account of the minute quantity of phosphorus contained, may not be appreciable to the sight. The alcoholic solution or tincture, it is more than probable then, is not the best preparation of phosphorus, by which to secure with any certainty, its full and peculiar operation, whatever this may be, upon the system. Lobstein, who seems to have made the subject of phosphorus something of a hobby, prefers its administration in the form of ethereal tincture, and tells us that such a preparation “is not decomposed by cold water,” as seems to be the case with the alcoholic tincture.

I have heard it remarked by individuals that certain physicians* (naming them) must be in the habit of using very strong medicine, for that when they dropped it in water “it fairly smoked.” Had the dropping been conducted in the dark, the conflagration from which the smoke was given off, might also have been seen. It would not, therefore, however it is probable, have been believed, that the strength of the medicine was all smoke—vapor—but rather that “’twas spirit Pandemonium,” at the very least.

Lobstein—who from the case to be referred to, there is grounds to suspect, must have been decidedly something of a Charlatan—once, from his own account, took advantage of one of the singularities of phosphorus, to operate to an extent beyond the influence of mere smoke, upon the imaginations of the persons surrounding a patient that he had been called to see. A woman fainted. He gave her a few drops of phosporic ether. “In two minutes after,” he tells us, “a flame was observed to proceed from her mouth, which caused all present to cry out with astonishment. I observed, humorously,” he goes on, “that she had a devil in her. Nevertheless, I gave her a second dose, and after a short time she opened her eyes and exclaimed ‘ah, I feel very well!’ The bystanders were so much surprised that they really believed that the “devil had

* With several of the physicians of Montgomery, who are no doubt indebted principally to Doctor Ames for directing their attention to it, phosphorus, in the form spoken of in the text, has been for some three or four years past, quite a favorite remedy in pneumonia, and one to which a remarkable degree of success claimed, in the treatment of this disease, is, in a considerable degree, ascribed by them.
flown out of her mouth,"—exorcised, as a matter of course, by the medicine given.

I have the permission of my friend Doctor Baldwin to say, that he has performed a series of experiments somewhat similar to mine,—has even given larger doses—and with like success.

In conclusion, I would remark, that though prior to the experiments here recorded, I had taken and given, experimentally, a good deal of the alcoholic solutions of phosphorus, I have never, in any instance, given them in a case of acute or dangerous disease. It would seem to have been truly said, that "there are more false facts than false theories in medicine."


Observations on the Human Eye by means of the Speculum Oculi.

[The first and second numbers of the Deutsche Klinik (Berlin) for January, 1854, contain an able article by Dr. Oscar Sæmann, on the Speculum Oculi of Helmholtz, and its application to the diagnosis of diseases of the organ of vision. The first part of this paper is chiefly devoted to a description of the mechanism of the instrument, the manner of its application, &c. The second part is that which we present to the readers of the Monthly, under the above title. H. N. B.]

In order that we may be able to distinguish with our eye the pathological alterations existing in any organ, it is necessary that we should be familiar with the appearances which that organ presents in its normal condition. It becomes necessary, therefore, that I here give a brief sketch of what we perceive in the healthy eye by means of the speculum oculi.

If we give the reflector such a position that it sends the rays of the taper passing through the convex glass into the eye to be examined—by which the dark spot, which corresponds to that point of the speculum not covered with foil, must fall directly upon the pupil—and look through the transparent portion of the reflector, we see the pupil clearly illuminated. The degree of its lucidity is, under otherwise equal circumstances and with a sufficiently dilated pupil, different in different eyes, and depends upon the greater or less capacity of the background of the eye to transmit and reflect light. A part of the rays of light, for example, is reflected by the retina and its vessels; another part passes through these to the choroidea. Of this transmitted light, again, one portion is reflected by the vessels of the choroidea, whilst another is absorbed by the pigment of this membrane, and the remaining portion penetrates the more transparent parts of the same, to be at last reflected
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by the sclerotica. The stronger the pigment of the choroidea is developed, the more the light is absorbed; the less it can penetrate, so much the more dimly will appear the illumination of the pupil: the less the pigment is developed, the more the light is reflected; so much the clearer will be the pupil. The color of the light presents all shades, from whitish-yellow to yellowish, from yellowish-red to the finest rose, which latter color I observed in an albino. Van Trigt directs attention to the fact, that the abundance of the choroid pigment is in direct ratio with that of other tissues, especially the hair; and that consequently in blonde individuals the pupil appears much clearer than in individuals with brown or dark hair. If we now apply to the instrument the concave lens corresponding to the state of refraction of both eyes, we perceive, in the back-ground of the eye, vessels of larger or smaller size, which sometimes run isolatedly, sometimes so that artery and vein lie together. The background of the eye itself appears in a reddish light, which passes to a dark brown when the pigment is very abundant, but shines of a clear rose color when the choroid pigment is less. The particular parts of the retina also do not appear equally colored; the color is clearest round about the optic nerve, and grows gradually darker towards each side. If we direct the eye somewhat inwards, it is not difficult, after some practice, to discover the Papilla nervi optici, which exhibits a truly splendid appearance. It presents itself generally as a circular, more rarely as an elliptical, clear white disk, which, shining like the full moon in a blood-red sky, is more or less nettly bounded by the surrounding parts, and at its periphery is girdled by a dark, often not wholly closed ring of various breadth. At times, here and there, single dark spots may be distinguished upon the shining disk, caused by little inequalities of the papilla. Somewhat inward from its middle point issue the Arteria and Vena centralis, the first of which is marked by its clearer red color and smaller circumference, sometimes single, sometimes forming a coil. For the most part these vessels extend upwards and downwards over the papilla, and divide near its periphery into two or more branches; but they do not always take so regular a course, but wind about in all directions, sending out their twigs over this disk. The artery, as well as the principal trunk of the vein, exhibit at the summit of their curve a light streak, which proceeds from the reflected light, and is not observed in the neighboring venous branches, because their walls are too little arched, more level. The falculiform line of shade lying inwards from the papilla, which Helmholtz has always, and Van Trigt never, seen, I have also at times, but not always, been able to discern.
If we cause the eye of the individual examined to look directly at the image of the taper present in the reflector, we have before us the point of direct vision, the *macula lutea*. Helmholtz says it rises less abruptly from the surrounding parts, has a darker gray-yellow color, and shows no vessels. According to our observations, however, it is distinguished in nothing from the rest of the background of the eye, neither by a different color or a greater want of vessels.

If we examine the normal eye with convex glasses, through which the eye of the observer is adapted also for the anterior parts of the same, *cornea, iris, lens, corpus vitreum*, we see the magnified pupil shine with uniform clearness.

After this short description of that which we perceive by means of our instrument in the normal eye, I proceed to the observations which we have made upon the diseased eye.

**Diseases of the Lens.**—The most frequent diseases of the lens are opacities, *cataractae*. When the opacity is far advanced, and its color a clear gray, there is no difficulty in the diagnosis: but the cognizance of opacities in their first commencement is extremely difficult, and the difficulty is increased from the fact that in elderly persons generally the pupil does not appear of a pure black. But, by the aid of the speculum it is possible to detect even the slightest opacities of the lens and its capsule, since the latter are very accurately defined when the background of the eye is illuminated, and we examine through a convex lens of 3—6" focal distance.

We have observed commencing opacities of the lens, which were wholly inaccessible to observation according to the ordinary methods of examination, in 24 individuals—eight of whom had a perfectly normal vision, and were examined only *experimenti causa*, the rest suffering from disturbed or extinguished vision, which had its origin, however, in other diseases of the eye.

The most frequent opacity was that of a nucleiform cataract (Kernstaar,) and was absent in only three of these 24 individuals. Upon application of a convex lens, No. 3, it was seen in all gradations of size, from the smallest point to a deep black disk of nearly 1" in diameter, and was always conformable, no single dark atoms being distinguished as composing the opacity. Its periphery was mostly circular, in two cases elliptic, with the greater diameter running obliquely; also dentate, stellate. It occurred in 20 cases bilaterally, and had but rarely reached the same stage of development in both eyes. This deeply dark disk was sometimes surrounded by gray, irregularly formed specks, which, in one young lady, represented a
second circular concentric disk in both eyes: in other cases by a large number of dark points of various size, but which never equalled the nucleiform cataract, and were sometimes very irregularly placed, sometimes appearing, more or less clearly, as a concentric layer, sometimes as a vincular, or as a stellar stratum. We saw these dark points, also, in those three individuals in whom the nucleiform cataract was wanting. In seven cases an opacity was seen at the outer border of the lens, which twice was encircled by a ring accurately defined and circular within, once by two concentric rings, separated from one another by a clear line, and consisting of thickly crowded points, while four times dentations were sent out towards the middle of the lens, by which the existing nucleiform cataract was rendered dentate or stellate.

The examination of more perfectly developed opacities of the lens, which may be already diagnosticated by the mere sight, shows that such cataracts always have a greater circumference than one would suppose from an exterior view. Here the speculum can accomplish something in the determination of the time at which the cataract will attain sufficient maturity for operation, since we have, in the visible progress of the opacity, a sure guide independent of the self-delusions of patients. In the examination of such strongly developed opacities of the lens, at times a deep black figure appears to us, which consists of three radii proceeding from the pole of the lens, growing smaller towards the equator; their direction corresponds exactly to the meridian lines, and there is no doubt that we have to do in such cases with the cleaving of these, often observed in cataractous lenses. Since the meridians of the anterior and posterior hemispheres of the lens have an opposite course, so we can easily determine from the course of these dark radii in which hemisphere the cleaving has taken place.

For the exact history of the development of cataracts, the speculum will be of the greatest use. Already it is proved with greater certainty that most opacities of the lens, taking their origin in the nucleus, extend in a centrifugal direction. This centrifugal extension is at times met by a second opacity, beginning at the border of the lens, and progressing in a centrifugal direction; but the latter is exceedingly rare. The opacities of the lens are composed of single dark atoms, which increase in number, become thickly crowded together, and at last conglomerate to a conformable mass. This conglomerate often proceeds with a certain regularity, and two special forms of it may be observed, namely, either these dark atoms unite to form concentric rings, or radiate lines. Only the opacity of
the nucleus of the lens has appeared to us, hitherto, always conformable; probably the cause of this lies in the denser stratification of the fibres which exist in the nucleus, and the want of abundant uniting tissue. If the speculum enables us on the one hand to recognize the least beginnings of the cataract formation, so also, on the other hand, it often teaches us that there is no opacity of the lens existing, when, from the mere sight, we should conclude that such was the fact. The pupil, especially in older persons, is not a pure black; it appears gray-yellow, whitish-yellow. Several cases have occurred to me, in which physicians had declared to eye-patients that they had a cataract, and might be eventually cured by operation, when the speculum proved that there was absolutely no opacity of the lens existing, and that the disturbance of vision was dependant upon a very different lesion of the eye.

Diseases of the Corpus Vitreum.—The circumstance that the vitreous body is almost completely withdrawn from observation, without illumination of the background of the eye, had made the diagnosis of its diseased conditions impossible. The disturbances of vision dependant upon this body, on account of the want of all externally perceptible alterations, were referred to lesions of the retina, and placed in the Augean stable of amblyopia and amaurosis. The speculum now teaches us, that diseases of the vitreous body, especially obscurities of the same, occur no more rarely than in the lens. These obscurities are of a twofold character:—either the vitreous body loses its natural transparency, the capacity to transmit rays of light to the retina, in which case, by the application of a convex lens, No. 6, the background of the eye appears wholly or in a greater part very dimly illuminated, and the retina-objects to be found behind the obscured spots, as well as the vessels and papilla of the optic nerve, are either not at all or only very indeterminately made visible by the concave lens; or black corpuscles are found floating in this body.

The first, from the analogy of opacities of the lens, we must consider as true obscurations of the vitreous body; they appear to us like cloudy opacities, which have a diffused extent, and almost always involved the greater part of the vitreous body. In sixteen individuals, six of whom were completely blind, and the rest all suffering from important disturbances of vision, we diagnosticated this diffuse obscurcation, since the retina-objects could not be perceived by any concave lens, and the background of the eye appeared dim, as if washed away. A confounding with opacities of the lens could not occur here, since cataracts, which so materially obstruct the vision, could scarcely
escape an exterior view, to say nothing of the examination by the speculum. Besides, in five cases there were simultaneously slight opacities of the lens, which, however, appeared as small points, and could by no means explain the above symptoms. The conclusion may be drawn, that obscurations of the vitreous body of relatively slight intensity produce serious injury to vision, whilst cataractous lenses, which show exteriorly a tolerably intensive gray color, often allow the patient the reading of coarser prints.

Still more frequently than these obscurations, we observe those dark floating corpuscles in the vitreous body, which are set in lively motion by the slightest oscillations of the globe of the eye, and cross before the clear, shining pupil in the most different directions. These corpuscles present the most manifold forms; sometimes they resemble small coiled serpents, sometimes polyhedral cells, sometimes long, irregularly formed coagula, sometimes they appear as innumerable floating points. We observed, also, the most various forms in the same eye, and, indeed, sometimes in such great number, that the vitreous body, after a movement of the globe, appeared like dirty swamp water. When the globe returned to rest, these corpuscles sank to the bottom from the force of gravitation. We made this observation upon 26 individuals; four of them had normal sight; the most, however, complained to us of their own accord that they saw dark bodies floating in the air, and the description which they gave of their form frequently agreed very exactly with that observed through the speculum. They were often very short-sighted, so that they required acute concave spectacles. The vessels of the retina and choroidea were mostly clearly visible, nevertheless, we were obliged to use strong concave glasses, which corresponded well with the nearness of vision.

Concerning the nature of these corpuscles, nothing definite can be said; pathological anatomy and microscopy must furnish an explanation of them. We have not, hitherto, detected in them a spontaneous motion, so that we cannot at least consider them living entozoa. They may, indeed, often be blood or exudation coagula; and the circumstance that in two cases we found a tolerably extensive extravasation of blood upon the retina, favors this view in some measure. From their exterior form, we might often also be led to consider them as cells; but it would be remarkable that such cells should remain so many months in the same stage of development. Perhaps they are many times residua of the lamellæ of the vitreous body, which, according to Bowman, exist in early life, and later are broken up.
The frequent, manifestly swift movement of these corpuscles in the substance of the vitreous body, which, according to Kolliker's latest investigations, consists, in adults, of a more or less consistent mucus, permits us, nevertheless, to decide upon the fluidification of this body with some certainty, which accords also with the short-sightedness of such patients so frequently observed by us. Fluidification of the vitreous body makes its coefficients of refraction smaller; but this is not sufficient to explain the frequent high degree of nearness of vision, if we do not admit that through this means the diameters of incurvation of the refracting media are altered. In one man, who complained that upon motion of the eye he saw small, clear, shining corpuscles floating about in the air, I observed in the vitreous body little glittering points, visible sometimes here, sometimes there, which disappeared upon rest of the globe. Could they have been crystals of cholesterine?

Diseases of the Retina and Choroidea.—The background of the eye appears to us, as we have already described above, as a field of vision shining with a reddish light, intercrossed by larger superficial vessels of the retina sending out single smaller branches, and by a deeper lying convolute of smaller vessels of the choroidea, upon which the Papilla nervi optici is accurately delineated through its intense brightness and the central vessels so clearly visible upon it. The truly surprising clearness with which all this is seen, did not allow Helmholtz to doubt that vascular distentions, varicosities, exudations before the retina, in its substance, and between the retina and choroidea, would be easily recognized. The observations hitherto made public have dispelled every doubt; and we also have not rarely observed pathological processes of the retina and choroidea. If we have not been able hitherto to recognize all visible abnormalities in their true essence, if we even overlook much which is abnormal, nevertheless, we can already assert that a large number of amauroses, which have heretofore been considered as neuroses, depend upon visible textural alterations of the retina and choroidea.

Most frequently we have observed distention of the vessels of the retina, by which their main trunks appeared enlarged, sent out many branchlets not formerly visible, and the whole background of the eye appeared of an unusually intense red color. It is true, that the size of the vessels, the number of their branchlets, the color of the background, are very different in different men, and, therefore, error is very easy; but the correctness of the diagnosis is favored on the one hand by the subjective symptoms, the pain in the eye and frontal region,
the feeling of unusual fulness in the eye; on the other hand, by
the frequent favorable result of treatment by the abstraction of
blood.

The true inflammation of the retina with exudation seems
also not to be rare. The spots covered with exudation appear,
sometimes, whitish, reflecting the light strongly; sometimes
reddish; sometimes they have a more greenish tint, and when
of greater extent, are surrounded by an irregular dark border;
sometimes, especially upon the papilla, they appear as dark
specks. The exudation-mass, for the most part, encloses the
vessels of the retina in such a manner that they become com-
pletely invisible, or at least appear only very dimly as white-
red, not well defined marks. Only twice have I seen tolerably
clearly the vessels of the retina running over the exudation-
mass. In one case, already alluded to above, in which the ves-
sels of the retina were surrounded by a reddish exudation, we
observed simultaneously in the vitreous body those coagula-like
corpuscles, which probably had become loosened from the floor
of the retina, and floated free in the vitreous body.

Injuries of the retina and its vessels from external violence
very rarely occur, and are then mostly connected with such
important lesions of the whole bulb, that examination with the
speculum can furnish no results; it may also happen that a
cataract needle in unskilful hands, or the lens dislocated by it,
injure the retina. We have not observed cases of this kind.
Van Trigt has wounded the internal eye of cats, dogs, and
other canines, with needles, and in this manner made highly
interesting observations upon the results of these injuries and
the cicatrization of the wounds. On the other hand, we have
observed a case of spontaneous rupture of the central vessel,
which I may be allowed to report in detail.

A man, 55 years of age, plethoric, was suddenly seized with
giddiness, and sank into unconsciousness. As he aroused from
this condition after an hour, he was blind in his right eye, which
before was perfectly normal. The physician called first, made
a venesection, and otherwise conducted the treatment lege
artis. Upon the fifth day after this mishap, he came to my
office to consult me on account of his eye. The pupil of the
right eye was more strongly dilated than that of the normal
left eye; both had a yellowish grey mucus, otherwise nothing
abnormal exteriorly. The examination with the speculum
gave the following result:—the papilla nervi optici was ac-
curately bounded on the outer side, nevertheless even here
was more dimly illuminated than usual; at the inner and in-
ferior side it was covered with a red blood-coagulum. From
the upper border descended a vessel, which, irregularly den-
tated, terminated free; all the other vessels of the papilla were covered with the coagulum. Otherwise all is normal. There could be no doubt that here a laceration of the central vessel had taken place at the point where it issues from the porus nervi optici. Ten days later, the coagulum, now of a less dark red, was still visible only at the inner border; all the rest of the papilla was of a greenish color, with occasional clear spots interspersed; out of its midst was now seen passing downwards a rose-colored, not well defined line, which glimmered but dimly, and at its superior border vessels were distinguished which represented two arches. In the vitreous body floated single dark specks. The power of vision continued totally extinguished.

Morbid dilatation of the vascular walls we have never observed except in veins, which then appeared like dark, undulatory or spiral cords running over the field of vision. We never found them upon the papilla.

In a young man, who saw objects perfectly clearly only in a bright sunlight, but recognized them very imperfectly with a cloudy heaven, at evening, and by lamplight, we found the retina covered round about the papilla, here and there, with black specks; the papilla itself likewise contained several dark specks, but appearing less black, and less accurately circumscribed. Otherwise nothing abnormal. The supposition that here a pigment-transudation had taken place, sufficiently explains the perfectly normal perceptibility of the patient existing only in an intense light.

In conclusion, I will make mention of one other case, in which the speculum showed us, with tolerable certainty, that a lesion of the optic nerve itself was the cause of the blindness.

F. S., 11 years of age, with brown hair, brown-yellow iris, and very dilated pupils, was born amaurotic in the left eye; the right eye normal. The left ocular globe is a little smaller than the right, and sits deeper in the orbit. The papilla nervi optici appears as an unusually small elliptic disk whose lesser horizontal diameter is about half as large as the great perpendicular diameter; the arteria and vena centralis give off as usual two branches, but very fine, and besides the branchlet passing inwards, a second is also here visible running outwards; the macula lutea and the rest of the background of the eye present nothing abnormal. By means of a concave lens, No. 3, we discovered a central punctiform opacity of the lens. The right papilla is circular and quite large. While here, on the one hand, the circumstance that the amaurosis of the left eye was congenital, and on the other the obstructed development of the bulb warranted us in concluding upon an analogous con-
dition of the optic nerve, so also the examination with the speculum confirmed this diagnosis.

Sanguineous distention of the choroidal vessels is a very frequent phenomenon, which is characterized by abnormal size and intensive dark color of the choroidal veins. We have also observed, in many cases, genuine choroiditis with secretion of the exudation-mass. When the exudation is deposited in small circumscribed spots, the background of the eye appears uneven, and reflects the light from those spots stronger than from others; if the exudation has a greater extent, the retina becomes mostly separated, as it were, at the point of entrance of the optic nerve, and projects like a large eminence, whilst it crowds the corpus vitreum out of its position; such an eye has then lost the normal relation of internal parts, and the vessels coursing upon the hill-like tumor may be seen by mere illumination from the mirror without the application of a concave lens. Since such extensive exudations are mostly serous, of a fluid nature, so the retina, together with the vessels, may be seen floating after a movement of the globe. Ruptures of the retina from exudations crowding upon it from behind, in consequence of which it floats about freely in the exudation and disorganized vitreous body, we have not observed. In one case of choroiditis, which had had its seat more in the region of the ora serrata, we found the following. If we looked through a convex glass, No. 6, in the direction of the axis of the eye, the pupil being nearly normally illuminated, a dark body appeared at the lower border of the papilla, which was bounded above by three convexities, and floated hither and thither upon motion of the bulb, without leaving the border of the iris; simultaneously there appeared several other dark dentations, also black coagula-like corpuscles floating free. If we looked inward, the eye being directed inferiorly, we saw behind this dark body a yellowish green exudation-mass projecting inwards deep into the eye, which formed several strata lying behind one another, representing mountains as viewed from a distance. Through a concave lens we beheld, in the very depth of the eye, other well defined black specks; but the vessels were seen only very imperfectly with the eye directed strongly upwards. The exudation, in this case, penetrated the vitreous body.

If we now review the truly surprising results which the speculum oculi has furnished us in so short a time, we can not only concur in the expectation cherished by the highly esteemed inventor, that all the alterations of the transparent media, the retina, and choroidea, found in the corpse, will also be recognised in the living eye; but also assert, with safety, that it will be possible for us now to pursue with exactitude the progressive
development of the pathological processes of these structures, as well as their recession, whether effected by nature or not.

The advantages which will be derived by the practising physician, irrespective of the cure of these diseases, we have not hitherto considered; but one thing is certain, that an exact knowledge of the diseased objects must be the basis of a rational therapia: without this knowledge we grope in the dark. We shall not be able to cure all diseases, even in this manner; but the physician gains a great advantage thereby, when, from his knowledge of their incurability, he holds himself aloof from fruitless, perhaps injurious, attempts to cure.—[Amer. Med. Monthly.

On the Medical Treatment of Cataract. By Dr. Garcia Lopez.

The majority of surgeons consider the cure of cataract without an operation a chimera. And yet it is impossible to deny that cataract is sometimes spontaneously cured. Is it, then, impossible that therapeutics may some day realize what nature occasionally accomplishes by herself? This question is often asked by unprofessional persons. Medical men have uniformly denied the possibility of the cure of complete lenticular cataract by medical treatment, while impudent and shameless charlatans have industriously propagated the opposite opinion among all classes of society. In the clinical reports of Dr. Beauvais* are recorded five homeopathic cures of cataract, one of which, treated by Dr. Caspari, was evidently a case of corneal opacity depending on trichiasis. The offending cilia were removed surgically, the patient took one drop of cannabis daily, and recovered entirely.

In the work of Tavignot the reader will find an account of the experiments of Professor Pugliatti, of Messina, on the curability of capsular or capsulo-lenticular cataracts without an operation. This surgeon supposes that a strong solution of ammonia, applied at the internal orbital angle, penetrates the tissues of the eye, and exerts a curative effect upon the opaque crystalline system. He combines with this treatment the administration of five grains of iodide of potassium daily. He reports several cases which were benefitted, and one or two which were supposed to be cured by this means. In every case the treatment lasted many months. In the majority of cases no amelioration was perceived. In the Spanish Journal El Porvenir Medico, for December, 1853, we find four cases in support of the efficacy of the treatment of Pugliatti, reported by

* Annales d'Oculistique, tom. ii., p. 218.
the author, whose name we have placed at the head of this article, Dr. Garcia Lopez:

1. A man of fifty years; nearly complete capsular cataract had existed for three months: vesication with ammonia, without any internal treatment, procured resolution in two months.

2. A man of thirty; soft, striated, lenticular cataract, two years old; capsule healthy. After six weeks of treatment the patient demanded an operation. Dr. Lopez found the lens diffusent and lutescent.

3. A woman of forty; hard, incomplete capsulo-lenticular cataract in both eyes, developed within a year. She could only distinguish very large objects. After seven months of treatment the patient recovered her vision, and could sew and follow her usual avocations; the opacity had disappeared except in a limited central point.

4. A woman of fifty; capsulo-lenticular cataract had existed for three years on the right side; a similar opacity was forming in the left eye, complicated with amblyopia. In six months, after the constant employment of ammoniacal vesication and iodide of potassium, a decided amelioration took place in the left eye; the right was unaffected.

Dr. Lopez reports these cases without any desire to attach undue weight to them, and can hardly be supposed to be prejudiced in behalf of a method which he did not originate.

There is another remedy to which much consequence is attached, to which Dr. Lopez does not allude, we refer to the iodine ointment allowed to dissolve on the conjunctiva. The editor of the Paris Gazette hebdomadaire, (Dec. 1853,) reports a case of double capsulo-lenticular cataract cured by this method.

In a succeeding article we have given a full account of the mode of treatment of cataract depending on capsulitis. The reports of the Medico-Chirurgical Society of Richmond, contained in the present number of the Journal, contain the histories of two cases of traumatic cataract, and one of supposed congenital cataract, in which the advantages of a free use of mercury in opacities of the crystalline system connected with inflammatory action are strikingly exemplified.

In all such cases, and in immature cataracts, no harm can arise from instituting medical treatment during a period which would be otherwise lost in awaiting the opportune moment for an operation. It must be confessed, however, that Lebert's researches into the structure of cataract demonstrate that in the great majority of cases medical treatment must be unavailing. Rognetta was of the opinion that an absolute denial of the efficacy of remedies in cataract was unwise; we concur in this sentiment, and trust that farther researches will be made in this interesting and obscure subject.—[Virginia Med. and Surg. Journal.]

In the Bulletin de l'Académie Impériale de Médecine, for January, 1854, we find a report by M. Bouchardat, on the researches of a young and learned chemist, M. Mouriès in regard to the effects of phosphate of lime in the nutrition of animals, and the influence which the judicious employment of this salt is capable of exercising upon the mortality of children in large cities.

It has been a comparatively short period since physiologists began to appreciate properly the importance of inorganic principles in the phenomena of life. The farther we penetrate into this complex problem, the greater is the importance attributed to bodies, the presence of which in the human organism was regarded as quite accidental.

Very dissimilar organic compounds may be substituted for each other in our diet without any disorder in the general harmony, but the inorganic principles can only be replaced by substances very closely analogous to them. Albumen, fibrin, and casein, and other more complex aliments, though differing in origin and composition, may fulfil the same physiological end, but it is different with inorganic principles. Lecanu has shown that iron is indispensable for the proper constitution of blood-globules; chloride of sodium is of primary importance also as a constituent of the liquor sanguinis, and it is only as an exception that we find, in certain graminivora, this salt partially replaced by the phosphate of soda or of potash. Liebig has shown that the chloride of potassium of the muscles cannot be replaced by chloride of sodium. Each inorganic constituent of the organism has, therefore, its definite and limited sphere of action, to which it is exclusively adapted.

Among the indispensable inorganic salts, the phosphate of lime holds an important rank. M. Mouriès has devoted himself to the elucidation of its peculiar action. He deduces from his experiments the following conclusions:

1. Phosphate of lime plays a more important part in nutrition than has heretofore been believed. Independently of its necessity as a constituent of bone, this salt maintains that irritability without which there is no assimilation, and consequently no nutrition. Its insufficiency, therefore, produces death with all the symptoms of inanition, while its insufficiency in a less degree, produces a series of lymphatic diseases.

2. The food consumed in cities is deficient in this respect. Nurses' milk has, consequently, the same defect. The infant as well as the foetus suffers from the deprivation of this element.
so indispensable to its development and life. Hence one of the causes of the increase in the number of still-born children, and of the mortality of infancy.

3. The addition of this salt, in combination with animal matter, to alimentary substances, obviates one cause of disease and death.

The following are the principal facts on which M. Mouriès relies to establish these conclusions:

The blood of animals contains a constant proportion of earthy phosphates, which is independent of their ingesta. The pigeon ingests phosphate of lime slightly in excess, in the grain and calcareous gravels which it picks up; the horse swallows an excess, in its fodder; the dog procures a still greater excess from the bones on which he is fed; and yet the blood of the pigeon contains in 1000 grammes 1.20 of phosphate of lime; the horse 0.5; the dog 0.4. This result is not accidental; all birds whose blood has been analyzed have 1.5 to 1.2 of phosphate of lime, while the proportion in the blood of the carnivora and herbivora varies from 0.9 to 0.4. The proportion thus regulated by nature, is modified by age and sex. The bull, cow, and calf, have the same food, yet their blood contains respectively 0.5, 0.9, 0.8 of phosphate of lime.

The requisite proportion of alkaline phosphates varies, therefore, in different animals. A pigeon weighing one pound died at the end of ten months, during which period he was fed daily on one ounce of wheat, with common water for a drink, by which rather more than a grain of phosphate of lime was ingested daily: on the other hand, a woman weighing 100 pounds enjoyed perfect health upon a diet which furnished her daily with 90 grains of phosphate of lime. Thus health in the one case, and death in the other, with relatively equal quantities of this salt.

We shall recur to this example to show how complex are the conditions of these experiments, and what reserve is necessary in drawing conclusions from them.

M. Mouriès asserts, and the fact has already been noted by Chossat, that if the proportion of alkaline phosphates of the food is deficient, there ensues atony of the digestive organs, imperfect assimilation, and death. To prove that pigeons die from want of phosphate of lime, we may observe that their death is hastened if they are allowed only distilled water, while their lives may be preserved by adding earthy phosphates to their food.

M. Bouchardat observed that the grain on which MM. Mouriès and Chossat fed their pigeons contained only traces of common salt. The birds, therefore, should be expected to suf-
fer from the deprivation of this principle. M. Bouchardat accordingly made this experiment; he confined two pigeons, and fed them on dried grain. In two months the health of the female became impaired; she suffered from thirst and diarrhœa and laid no more eggs. She was set at liberty. She flew immediately to a window-sill impregnated with alkaline chlorides, and began to peck eagerly; there was a larger quantity of salts on the interior of the window-frame; the pigeon entered through the open window, and permitted herself to be re-captured, so imperious was her demand for these principles. Her health was re-established; in three days she laid another egg. It is wrong, therefore, to conclude with M. Mouriès that a deficiency of phosphates is the only cause of the symptoms he observed; in this case the absence of chlorides was the obvious cause.

M. Mouriès has established, by interesting calculations, that grain furnishes a sufficient supply of phosphate of lime for the reparation of bone, but not for other essential functions of the economy. From the curious fact that there is a constant proportion between the temperature of animals, and the amount of phosphate of lime contained in their blood, he deduces the principle that this salt keeps up animal irritability, without which nutrition is impossible. The following table must interest physiologists:

<table>
<thead>
<tr>
<th>Phosphate of Lime.</th>
<th>Temperature.</th>
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<tbody>
<tr>
<td>Mouriès. Poggiale.</td>
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<tr>
<td>Blood of the duck</td>
<td>1.50</td>
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<tr>
<td>&quot; the hen</td>
<td>1.35 1.25</td>
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<tr>
<td>&quot; the pigeon</td>
<td>1.20 1.23</td>
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<tr>
<td>&quot; man</td>
<td>0.80 0.6</td>
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<tr>
<td>&quot; the horse</td>
<td>0.40 0.5</td>
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<td>&quot; frogs</td>
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If these results are confirmed, it will appear that the ingestion of phosphate of lime is not only indispensable for the reparation of bone, but that it is connected with the function of caloriification.

In the second portion of his memoir, M. Mouriès, starting from the principle demonstrated by Chossat, verified by Bous-singault, taught by Bérand, and now admitted by all physiologists, that diet is defective which does not contain enough phosphate of lime to repair the waste which is continually going on in the economy, attempts to prove that the food commonly consumed in cities does not contain the quantity of this salt which is required by nurses and pregnant women.

He commences by calculating the quantity of phosphate of lime which ought to be ingested in the twenty-four hours, which
he estimates from analyses of the excreta at 110 grains. He then attempts to show that this quantity is not contained in the food of nurses in cities. The urine of women in the country contains 90 grains of phosphate of lime in the twenty-four hours, while the amount of this salt in the urine of women in cities varies from 20 to 90 grains. M. Mouriès has sought to confirm his hypothesis by direct proofs; he has examined the food consumed in cities, and shown that it exhibits a deficiency of one half in alkaline phosphates. He has examined the milk of nurses, and shown that in 18 healthy country women the proportion of earthy phosphates in the milk varied from 1.2 to 2.4 per cent., while in the milk of 10 Paris nurses the proportion varied from 0.5 to 0.9, and in 7 others there was only a trace of phosphate of lime.

In the third portion of his essay, M. Mouriès adduces clinical facts in illustration of the advantage of supplying this deficiency of phosphate of lime in aliments. In 13 cases, in which the proportion of phosphate of lime averaged 0.7, 75 grains of this salt with twice that quantity of albumen was daily administered in soup; in a week the proportion of earthy phosphates in the milk rose to 2.1. In 5 cases pregnant women were subjected to the same treatment; the milk, after delivery, contained 1.9 to 2.1 of phosphate of lime. Only 3 of the 18 children died.

These results though insufficient to determine such a serious question, are yet very worthy of attention. In the debate to which they gave rise, M. Gibert vehemently condemned the present tendency of chemists to interfere in medical inquiry. The question of lactation was a medical one, he said, and was only to be solved by clinical observation. M. Bouchardat, on the other hand, feared only ignorance, and was not alarmed at the application of chemistry to medicines, especially when its results were as inoffensive as those he had discussed.—[Ibid.

[From the New-Hampshire Journal of Medicine.]

**Therapeutical Value of Ox Gall.**

Mr. Editor:—In the Journal for January a treatise on the medical virtues of Ox Gall met my eye; I can indorse every idea there advanced, and more; I have been in the frequent use of the article for quite a number of years, as an injection in constipation,—taken in pills in a dried preparation, and in a fluid state to arrest spontaneous emesis. It is an anti-emetic of much power, and entirely safe. A little anecdote occurs to my mind which took place in this vicinity some twenty years since.

A man of small intellect, and less cultivation, had a receipt
for preparing a strengthening plaster, and some other medicated preparations; of course was dubbed Doctor; he was a member of the Baptist church, the pastor of which was attacked with spontaneous emesis, which was not promptly arrested; some brother named Dr. Green; he had done great things in this way; a messenger was sent eight miles to inquire for his remedy; he had promised never to divulge, but he would go and see the man; arriving, he called for a hen, secludes himself, removes the liver, punctures the gall bladder, takes the bile and gives it to the patient, the vomiting is arrested, and the church enamored. They would have conferred on him M. D., had it been in their power. If my memory serves me he was canonized.

This article is here much used by the inhabitants in domestic practice, as a specific in most diseases. I recently listened to praises of a butcher touching gall. He recommended that of the hog as being far preferable to any other. I thought there might be some truth in the remark, for it must be admitted, the lords of creation resemble that animal more than any other.

In cases of convalescence, where the liver is torpid, the patient does not improve, has no appetite, how many of us have been benefitted, or at least our patients, by a recipe in a book on the liver by Dr. Saunders—consisting of soap, rhubarb, quassia, gin, &c., but gall is far preferable; use this until the liver resumes its healthy function.

I will insert a recipe, for a preparation of gall which I have used many years as a carminative in the griping and flatulence of infants, and to arrest emesis in adults.

℞. Essence of anise, 3 ij., essence of cinnamon, 5 ij., mix thoroughly; to the above add 3 iv. ox gall,—mix well by agitation.

One drop of this for an infant in some herb tea (from a drop to a teaspoonful, according to age.)

In order to preserve gall pure, I put in a two quart bottle, O. ss. strong alcohol, carry my bottle to the butcher and request him to fill it. I keep it in the cellar. It will keep good for years. Ariel Hunton.

Hyde Park, Vt., 27th Feb., 1854.

Sciatica.

Mr. Hancock read a paper on the causes and treatment of Sciatica before the Medical Society of London, Feb. 28, (Med. Times and Gaz.,) of which the following is an abstract: He alluded to the various causes assigned as producing sciatica—
viz., disorder of the stomach and bowels, inflammation and disease of the sciatic nerves, syphilis, gout, obstruction and dis-tension of the caput coli, tumors, or accumulated faeces in the course of the nerve, effusion of fluid into the sheath of the nerve, irritation and disordered state of the kidneys, and rheumatism, either acute or subacute; but that from the cases which had fallen under his observation, the prevailing opinion appeared to be that sciatica depends mostly upon rheumatism—an opinion which he considered erroneous, as having had ample opportunities of arriving at a conclusion, he felt convinced that the cause most productive of the complaint was irritation within the pelvis, either from loaded colon or cæcum, or from tumors formed within that cavity, or, as had been suggested to him by his friend and colleague, Mr. Goldsboro, by the hemorrhoidal vein, which, forming a complicated plexus over the sacral plexus of nerves, would, when congested and engorged, cause undue pressure and irritation of the nerve; that although he would not presume to assert, in opposition to the high authorities who differed from him, that the disease never depended upon rheumatism, still he maintained that it so seldom did so, as to constitute the exception to the general rule, and not the rule itself. Mr. Hancock then enumerated some of the modes of treatment, recommended such as bleeding, cupping, calomel and opium, colchicum, quinine, carbonate of iron, Indian hemp, acupuncture, actual cautery, blisters, moxas, puncturing the limb, and introducing morphia and creosote into punctures. He did not consider the disease difficult of cure; on the contrary, he had found it readily and speedily yield to the remedies he had adopted, which consisted in thoroughly purging the patient with small doses of croton-oil, combined with blue pill, henbane, and compound extract of colocynth, and removing the sensation of bruising in the course of the nerve by sulphate of quinine, in doses of three grains thrice daily. He had commonly given croton-oil, because he had found it useful and convenient, but he did not attach any specific influence to this medicine, as he had found equally good results from turpentine and castor-oil, the aloetic purgatives; and he considered that any medicine acting upon the lower intestines would be of service. He did not consider local applications at all necessary, but that they were frequently injurious, by adding to the patient’s sufferings. He narrated five cases which had fallen under his care, having been previously treated for rheumatism by the remedies appropriate for the affection, but without success. In one the sciatica had existed for nearly two years; in another, for above twelve months; and in the remainder, from four to two months. The most obstinate of
these was cured in three weeks, and the remainder in a fort-
night, the latter period being that usually required. He could
give many more cases of a similar character, but he thought
those he had described were sufficient to prove the validity of
his position, that in a vast majority of instances the disease
did not depend upon rheumatism, but upon the causes which
he had already alluded to; and he directed the attention of the
Society to an interesting fact, which he had not found noticed
elsewhere—namely, that sciatica almost always occurred on
the right, and very seldom indeed on the left, side of the body;
and when it did, he believed it was caused by impaction in the
sigmoid flexure of the colon. In the majority of cases the affec-
tion commenced with stiffness, weight, and pain in the lum-
bar region, resembling lumbago. He was inclined to attach
some importance to this fact, as affording a means of diagnosis
between those cases depending upon loaded intestine and those
resulting from tumor in the pelvis; and that where this symp-
tom is absent, it is desirable to institute a careful examination,
not only above Poupart's ligament, but in the perineum within
the tuber ischi, and also by the introduction of the finger with-
in the rectum. He had in two instances detected the exist-
ence of tumor in the pelvis by adopting these measures; and
he thought this the more desirable from the importance of
avoiding irritation as much as possible in cases of this descrip-
tion.—[N. Y. Jour. Med.

Upon Filtration of the Air in connection with Fermentation and
Putrefaction. By H. Schroder and Dr. Th. Von Dusch.

§ 1. In the year 1837, Dr. Schwann, of Berlin, communica-
ted a series of experiments, which proved that putrefaction did
not occur in a freshly boiled infusion of flesh, and that the vinous
fermentation did not take place in a freshly boiled fluid, previ-
ously susceptible to fermentation, if the air that was suffered to
enter had been first exposed to a red heat. It was proved by
these experiments, that it is not the oxygen, at least not it
alone, that causes the vinous fermentation, the putrefaction of
an infusion of flesh, or even the formation of mould and infusor-
ia, but a substance contained in the atmosphere and destroyed
by heat.

Schwann considered it probable that fermentation and putre-
faction were induced by sporules of microscopical cryptogami
and infusoria, contained in the air, which developed themselves
and increased at the expense of the fermenting or putrefying
substance, and thus continued the process. These sporules or
germs existing in the air, are, however, destroyed by heat. Similar experiments, with like results, were made by Ure and Helmholtz.

§ 2. Regaud de l'Isle has shown in his examination of the miasmatic influences of the Pontine marshes, that an interposing wood is a protection from the noxious effects,* and Becquerel says, "Une forêt interposée sur le passage d'un courant d'air humide, chargé de miasmes pestilentiels, préserve quelquefois de ses effets tout ce qui est derrière elle, tandis que la partie découverte est exposée aux maladies. Les arbres tamisent donc l'air infecté, et l'épinent en lui élevant ses miasmes."†

Finally, Lowel communicated last year a series of remarkable experiments, upon the crystallization of an oversaturated solution of sulphate of soda, and proved that such a solution, which, when in contact with air, crystallises almost instantaneously, does not crystallise if the air has been previously filtered through a layer of cotton.

§ 3. The facts above mentioned, being all that is known with regard to fermentation and putrefaction, in connection with the filtration of air, led one of us in January, 1853, to the supposition, that a freshly boiled organic substance, in contact only with air properly filtered, would be protected from fermentation or decomposition. In order to test this supposition, we performed the following experiments:

§ 4. Cotton was selected as a means of filtration, because it is known that it retains upon its surface infectious diseases, and is even capable of conveying them to a distance.

The apparatus used for filtering the air, consisted of a tube, about 1 inch in diameter and 20 inches in length, filled with raw cotton, moderately compressed; both ends were closed with waxed corks, through one of which was passed a short, open glass tube about \( \frac{1}{2} \) inch in diameter; the other end was connected by a tube of similar diameter bent at right angles, with the flask containing the meat or infusion to be experimented upon. The flask itself was connected by a tube of the same diameter, with a gasometer or air-tight vessel holding about one cubic foot of water, and provided with a discharge cock at the bottom, and another cock to intercept the communication between it and the flask; besides an opening for introducing water which could be perfectly closed. The joints all being air-tight, it is evident that as the water ran out of the gasometer, fresh air must enter through the cotton and the flask to replace it. When all was in order, the discharge cock was so regulated that the water could escape in drops only, and the air constantly passed

* Bibl. Univers. vol. xiii.
† Compt. Rend. hebd. xxxvi. 12.
through the apparatus in proportionate amount. Before being put into the glass tube the cotton was heated in a water-bath, and the contents of the flask in all the experiments were brought to ebullition, which was continued until the tubes were heated up to the part where the cotton commenced.

§ 5. The first experiment was commenced on the 9th of February, 1853. Two flasks, placed side by side, each containing meat, and the freely boiled decoction thereof, were made use of. One vessel was connected with the filtering apparatus described above, the other was left open. The meat and the decoction in the open flask, began in the second week to develop an intolerable odor of putrefaction, so that it was necessary to remove it from the laboratory.

On the 6th of March, we opened the other flask, through which during the whole time—that is during 23 days and nights—filtered air had been passed. The appearance of its contents was entirely unchanged. There was no trace of odor, but upon being heated, the pure characteristic smell of fresh unseasoned broth was developed.

§ 6. The experiment was repeated in a warmer season of the year, 20th of April.

a. We placed some meat in water, as described in §4. The treatment was the same as in §5, except that the current of air was only passed through during the day, and the vessel closed at night.

b. Besides this, was placed in an open flask, fresh meat boiled in water.

c. At the same time, a flask containing similarly prepared meat was closed with a waxed cork, through which was inserted a glass tube about 12 inches in length, and 1 line in diameter, the object being to retard the entrance of air.

d. In the fourth flask, we put meat boiled in water, and closed with a loose stopper of cotton, over which a large padding of cotton was placed, fastened to the neck of the flask by a silken thread. Upon the cooling of the flask the fresh air entering must necessarily be filtered in passing through the cotton.

In the second week, the meat in the open flask (b.) underwent stinking putrefaction, and was obliged to be removed from the laboratory.

On the 10th day, a thick growth of mould was observed in the flask with the narrow glass tube, (c.) At the expiration of 19 days, upon being opened, only a mouldy smell was perceived, not the odor of putrified meat.

The two flasks, (a. and d.) through which filtered air alone had been passed, were opened on the 14th of May, at the expi-
ration of 24 days; no mould formation or any striking change of substance was perceived; a whitish appearance was observed in some parts of the meat, which had not been noticed at least by us before.

The substance in both flasks was found, upon opening, to be without odor; upon being heated, the unchanged smell of fresh broth was developed; the taste was that of fresh unseasoned broth. Like fresh broth, it reacted slightly acid. The distillate of a part of this, was entirely neutral.

By these experiments it is therefore established, that meat, freshly boiled in water, and freshly boiled broth, remain for several weeks completely unchanged, if only such air as has been previously filtered through cotton is suffered to enter.

§7. On the 14th of May we took from Grohe’s vinegar manufactory of this place, some freshly boiled sweet malt-wort, to which some hops had been added. It smelt and looked like beer wort, tasted sweet, and reacted only slightly acid. This wort was put in the flask connecting with the filtering apparatus, and treated for 23 days, as described in §4. In the last eight days a cubic foot of air was daily drawn rapidly through, so that a visible depression was caused upon the surface of the liquid. An open flask was at the same time placed beside it and filled with freshly boiled wort. After eight days the formation of mould commenced in the open flask; the liquid became also cloudy, whilst that in contact with the filtered air was perfectly clear, and remained free from mould. On the 6th of June, 23 days after, we opened the flask; the liquid was as clear as at the beginning of the experiment, and developed upon being heated the odor of unchanged wort. The taste was sweet and unaltered, and the reaction slightly acid, as before the experiment. Examined with Steinhold’s beer test, we obtained 0. p. c. of alcohol, and 7.9 p. c. of extract of malt.

We had, previously, neglected to make a determination of the value of the liquor experimented on. We could only, therefore, compare it with a new sample of fresh wort, taken from the same manufactory. This gave with Steinhold’s test, 0. p. c. of alcohol, and 7. 7. p. c. of extract of malt, therefore of the same value, excepting the slight concentration of the first fluid by evaporation during the experiment. We believe to have established by these experiments, that at the temperature of May and June of this year, a sweet fermentable wort will remain entirely unchanged for weeks, if only such air has access to it as has been previously filtered through cotton.

§8. With the new sample of wort above mentioned, we commenced on the 6th of June another experiment. We wished to see, as a farther check upon the experiment, if upon
the removal of the cotton from the filtration tube, (the treatment otherwise being the same as in §4,) the contents of the flask would remain unchanged. An open flask was again placed beside it. The formation of mould commenced in the latter, within the first week, but not until after 12 days in the one connected with the tubes. It commenced with a rapidly growing speck of mould, exactly on the spot where the current of air came in contact with the surface of the liquid. The liquids in both flasks became cloudy. It was, therefore, evident that protection from these changes could only be found in filtering the air through cotton.

§ 9. It was now of interest to ascertain, whether under like circumstances, boiled fresh milk would remain unaltered, or whether it would curdle or putrefy. But all the experiments which we made in the months of June and July, gave only negative results. The milk coagulated quite as rapidly in filtered, as in open air; and in every case, the odor of putrefying casein was developed, as soon with the former as with the latter. The formation of mould was, however, entirely prevented by the filtration of the air.

This negative result reminded us of a similar one, obtained by one of us, in connection with L. Gmelin, in 1846, in regard to the behaviour of milk, when placed in contact with a large amount of confined air previously heated.

Negative results were also obtained in all experiments with fresh meat that was heated in a water-bath, first being placed in a flask without the addition of water; the flask while yet hot, was closed as in §6. d, with a stopper of cotton, and the neck surrounded by a thick padding of the same material. The meat became offensive, as quickly as in the open air or in a flask, which was corked as in §6. c, communicating with the air only through a long narrow tube. The only difference was, that the greenish-brown liquid, that in the open flask surrounded the putrefying pieces of meat, was observed under the microscope to be alive with infusoria, with Monas termo or at other times with Fibrio lineola; whilst in the same liquor that putrified in filtered air, Fibrio lineola decidedly did not appear, and even Monas termo could not be recognized with certainty; no other infusoria were present. We believe, therefore, that in all these experiments, the meat had not been sufficiently heated to its centre, and that the experiment should be repeated in another manner.

§ 10. On the 18th of July, unfortunately the hottest season of the year, we again boiled meat in water, and while hot, corked it, and padded it over with cotton, as in §6 d.; a test fluid in an open flask beside it, showed, on the 22d of July, the
odor of putrefaction, and on the 23d, a species of large infusoria could be recognized under the microscope, which we were not able to determine. They were globules or cells of the size of yeast globules, in constant voluntary motion, drawing themselves together like balls, and then stretching themselves out; we were, however, not able to perceive any further organization.

Upon the liquor that was under the cotton, we perceived a kind of fat skin, that covered the whole surface and gradually thickened. In the third week, the liquid acquired a reddish color; and upon being opened on the 15th of August, gave the odor of stinking fat, which, however, upon being warmed, was mixed with the odor of fresh broth.

We feel obliged to mention this experiment, because we do not consider ourselves justified in withholding it; not because we think any particular stress should be laid upon it; conclusions must not be drawn from a single experiment of this kind. Our idea is, that the meat in question, had not been boiled long enough, for the boiling had been stopped as soon as the liquid foamed up, so that probably all the serum in the interior of the meat was not coagulated. It is also very possible that from the high temperature at that time, the fat became rancid, which might have taken place even after boiling a long time. This can only be determined by further experiments; but even in this experiment, made during the hottest weather, putrefaction did not occur within 23 days.

§ 11. Although we believe that we have obtained from these experiments decidedly positive results, yet they should by no means be considered as concluded.

It appears then settled that there is a spontaneous decomposition of organic substances, as the putrefaction, of meat without water,—of the casein of milk—as well as the transformation of the sugar of milk in the milk into lactic acid, that requires for its commencement only the oxygen of the air, and that there are other phenomena of fermentation and putrefaction, which are improperly placed in the same category; viz: the fermentation of malt wort, and the putrefaction of meat under broth, which require for their commencement, besides the oxygen, some unknown admixture in the atmospheric air; which according to Schwann's experiments, is destroyed by heat, and according to ours, removed by filtration through cotton. It will be a problem for future experiments, to divide into two classes those phenomena, which are now united under the general idea of fermentation and putrefaction. Our attention hereafter will be particularly devoted to certain simple organic combinations, viz: pure albumen, pure fibrine, casein free from fat, &c. &c.
§ 12. We have as yet only made use of cotton, as a means, of filtration. It will be the object of future experiments to try a number of other substances for this purpose. We shall first use coal, sulphuret of lead, pumice stone, powdered glass, gypsum, &c. &c.

It is yet to be investigated, whether certain filtering substances will not remove the germs of one species of putrefaction and fermentation, permitting those of another to pass through which in its turn may be removed by some other filtering medium; thus dividing the filtering substances into different classes.

Since there is still so much that remains undecided in an experimental way, we at present withhold all theoretical deductions from our researches.

The experiments mentioned above, will require from their nature, so long a lapse of time, that we do not think it right to withhold any longer from the public, the positive results already obtained.—[Leibig's Annalen. Med. Examiner.

EDITORIAL AND MISCELLANY.

Scarlatina—The subjoined extract from the letter of an esteemed correspondent, who resides in middle Georgia, will explain the object of the remarks which follow it. We have had, in Augusta, no epidemic of scarlet fever this year, although a few cases have occasionally occurred.

"We have had an eruptive fever here for a month or two, and I understand you have had the same, or a similar disease, in Augusta. There is diversity of opinion among the "faculty" here, as to its character. I think the majority of the cases are Roseola, and a few cases of Scarlatina; the other physicians say it is all scarlatina. If you have had the same epidemic in Augusta that we have had, I, for one, would like to have your views on it through the Journal."

Scarlatina was, we believe, first introduced into Georgia in 1834, by a family who came from New York to Augusta, and one of whose inmates was seized with the disease on the day after their arrival. From that case it spread over the whole city, causing the most frightful mortality ever experienced among the children of this community. From this place it was carried into the country and has ever since been domiciliated in Georgia, and has from time to time manifested itself in the form of distinct epidemics—in various localities.
Epidemics of Scarlatina, like epidemics of any other disease, must be induced by a deleterious agent, which is probably diffused through the atmosphere. It is also probable that this morbid principle is unequally distributed, and that the mildness or the severity of the epidemic depends upon the relative quantities brought to bear upon the human system. Judging from analogy, may we not infer that those who have the disease in a mild form have imbibed a smaller quantity of the scarlatina poison than those who suffer a violent attack. May we not indeed admit that the degrees of intensity of the disease must be as various as the doses of poison inhaled—or that the manifestations of the disease are in a direct ratio with the potency of its productive cause. Scarlatina being induced by a specific agent, must necessarily in each case be an indication of the force of the active cause; and this force may vary ad infinitum, from the smallest quantity susceptible of being felt by the human system, to the overwhelming amount which produces inevitable and prompt annihilation of the vital forces.

Hence it is that during the prevalence of epidemic scarlatina we find it sometimes revealing itself in such a mild form as to induce a mere efflorescence of the surface without febrile disturbance, which may well be mistaken for simple erythema or non-specific roseola. In other cases an equally slight anginose affection, either alone or in combination with the cutaneous element, will constitute the only symptom.

The propositions just enunciated, although true in the abstract, must be viewed in connection with other circumstances, which are equally well known to exert more or less influence upon the causation and development of disease. Climate, seasons, age, sex, temperament and stamina, are fruitful sources of perturbation and modification in the sequence of cause and effect, which ought not to be overlooked in estimating the relation of the one to the other. As in the administration of remedial agents, which may be weighed or measured accurately, the dose is known to act with various power, according to the circumstances just referred to, so will equal quantities of an atmospheric agent inhaled be followed by very different results, in different climes, seasons, ages, sexes, temperaments, and degrees of resistance. With regard to scarlatina, for example, its violence and consequent fatality will be usually found to be much greater in northern latitudes than in the more temperate—in New England than in Georgia. Yet we have not observed any special difference induced in its intensity in Georgia by the seasons. While, in our section of country, adults are very
generally exempted from its inroads, they are much more susceptible in New England. Of the few adults who have taken this disease in Georgia, the great majority have been females; and it is among these alone that the writer recollects ever knowing of a fatal termination.

Although the stamina or power of resistance to disease is usually regarded as less developed in the lymphatic than in other temperaments, our observation does not authorize us to consider this to be true with regard to scarlatina—for we are rather, on the contrary, disposed to the belief that the greatest proportion of mortality occurs in children of apparently robust constitution. In this respect scarlatina obeys the same law as yellow fever, which is, we think, more apt to take off the hale and hearty, than the more delicate members of society. But yellow fever is more fatal with adults than with children, whereas the converse obtains in scarlatina.

The name by which mild cases are to be designated is by no means unimportant; for while simple erythema, roseola, or sore-throat, need but little attention, there is no case of scarlatina so slight in its first stages, that it may not, by contagion, impart the more formidable disease to other persons, or be itself followed by the most serious sequelae. Some of the worst cases of anasarca we have ever seen to follow scarlatina, occurred in patients who had had only a roseolar eruption and been subject to no treatment directed to the kidneys. It is therefore not a matter of indifference whether we call such cases roseola, or mild scarlatina. Every parent knows that scarlatina ought to be avoided by his children; but he may not be equally careful to keep them away from those who are said to have mere roseola.

There is a peculiarity in relation to the fatality of scarlatina in children, which we do not recollect to have seen noticed by authors, and which we cannot account for satisfactorily. It is the fact that by far the greatest number of deaths occur in subjects between the ages of 4 and 8 years. As a general rule, the younger a child is, the less effectually will he resist the inroads of disease—although the non-professional not unfrequently seem to think it better for their children to take measles, hooping cough, &c., in early life. We believe that statistics will always show the mortality from these diseases to be in a direct ratio with the youthfulness of the patient. In scarlatina, however, there seems to be (in Georgia) comparatively little danger prior to the age of two years—the danger reaches its maximum of intensity at 4 years of age and continues equally so until 8 years—gradually lessening from this until 12 years of age, and then becoming, as in infancy, very rarely fatal.
As we have so much digressed from our original purpose, we may be permitted to add another remark. Writers generally affirm that the eruption in scarlatina occurs on the second, or within a few days after the inception of the febrile symptoms. According to our observation of the disease in this section of country, from its first appearance in 1834 to the present time, the eruption may always be seen upon a portion of the body, or upon the whole of it, within the first 24 hours of febrile excitement; and in the majority of cases, it may be seen upon the trunk, especially the back, within the first 12 hours. Such has been the uniformity of this rule, that the writer has for many years felt quite safe in declaring that the case would not prove to be one of scarlatina if the patient had had fever 24 hours, and that no eruption was yet perceptible.

Finally: the mililiary vesicles which sometimes show themselves in this disease, are much more frequently seen in negroes than in whites—and the desquamation is also more extensive or in larger plates in the African than in the Caucasian.

The above remarks, hastily thrown together, and consequently without much regard to order, are based upon a long observation of the phenomena of scarlatina as manifested in this part of Georgia. That these may differ in various climates and during other epidemics, we readily admit. If what we have observed here does not accord with the experience of other Southern practitioners, we would like to be apprized of the fact

*Delirium Tremens among the Troops in Canada.*—The Medical Chronicle (of Montreal) contains an interesting statistical account of the occurrence of delirium tremens amongst the troops in Canada for the last thirty years, by Dr. Henry—from which we derive the following Recapitulation:

<table>
<thead>
<tr>
<th>Number of cases of Delirium tremens in thirty years</th>
<th>1769</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of deaths</td>
<td>143</td>
</tr>
<tr>
<td>Ratio of cases to army, first fifteen years, as 1 to</td>
<td>175</td>
</tr>
<tr>
<td>Do, second fifteen years, as 1 to</td>
<td>75</td>
</tr>
<tr>
<td>Do, the whole thirty years, as 1 to</td>
<td>93</td>
</tr>
<tr>
<td>Ratio of deaths to cases, first fifteen years, as 1 to</td>
<td>16</td>
</tr>
<tr>
<td>Do, second fifteen years, as 1 to</td>
<td>12</td>
</tr>
<tr>
<td>Do, the whole thirty years, as 1 to</td>
<td>12½</td>
</tr>
</tbody>
</table>

*Death from the sting of a Wasp.*—We perceive in the newspapers the statement that a gentleman in Madison county died in 15 minutes
from the sting of a wasp. Will some physician who knows the circumstances of the case, oblige us with an account of them. It would be interesting to know by what kind of wasp the injury was inflicted—and whether any blood-vessel or nerve was stung.

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The Science and Art of Surgery: being a Treatise on Surgical injuries diseases, and operations. By John Erickson, Professor of Surgery in University College, and Surgeon to University College Hospital. Edited by John H. Brinton, M. D. Illustrated by 311 engravings on wood. Philadelphia: Blanchard & Lea. 1854. 8vo, pp. 905. (For sale by T. Richards & Son, and McKinne & Hall. Price $5 00.)

This work is the substance of the course of Lectures delivered for some years by the distinguished successor of Liston—and bears the impress of learning, wisdom, and experience. Although works upon Surgery have been very much multiplied of late years, the advances of art demand their reproduction at short intervals, in order to facilitate the diffusion of new facts and opinions. The work before us appears to be well posted up, so far as it goes. It contains nothing, however, upon the very important class of diseases peculiar to the eye—the author having preferred to leave this branch to be studied in special treatises. The wood cuts are not as good as they might be; but will answer very well. We think that we may honestly recommend the book to the profession.


Having already had occasion to notice this work, it is at present merely necessary to state that a third edition of any work upon medical subjects is at least prima facia evidence that it has been approved by the profession. We doubt not that in the present instance, the patronage will be continued.


As physician to the "Hospital for Sick Children," Dr. West has had practical advantages probably not inferior to those of any practitioner in England. The substance of his course of Lectures is here
presented with such improvements as have been suggested since the publication of the first edition. The work is eminently practical, and as such must be favorably received by the profession.

Clinical Lectures on Pulmonary Consumption. By Theophilus Thompson, M. D., F. R. S., Physician to the Hospital for Consumption and Diseases of the Chest, &c., &c. Philadelphia: Lindsay & Blakiston. 1854. pp. 240. (For sale by T. Richards & Son.)

A monograph upon pulmonary consumption would seem to be scarcely worth reading when it is remembered that we know of no radical cure for this disease. Yet no physician ought to be satisfied to rest with folded arms, and witness the ravages of such a scourge, without making some attempt to stay its effects. It is only by monographs that we may expect to perfect ourselves on any medical subject; and the lectures before us are calculated to do a great deal of good. Many persons annually die in our country with consumption, who might have been saved by judicious management.

On Rheumatism, Rheumatic Gout, and Sciatica, their pathology, symptoms and treatment. By H. W. Fuller, M. D., Cantab., F. R. C. P., assistant physician to St. George's Hospital, &c. New York: S. S. & W. Wood. 1854. 8vo., pp. 322. (For sale by T. Richards & Son.)

This is a very complete monograph upon a class of affections alike painful and intractable. The learned author has advanced many original views worthy of mature consideration. As we hope to notice some of them hereafter, we will at present merely say that practitioners should welcome any new light upon the subject, and buy this book.


We are happy to announce a new and improved edition of this, one of the most valuable and useful works that have emanated from an American pen. It would do credit to any country, and will be found of daily usefulness to practitioners of medicine. It is better adapted to their purposes than the Dispensatories.
Handbook of Chemistry—Theoretical, Practical, and Technical. By F. A. Abel, Professor of Chemistry at the Royal Military Academy, Woolwich, &c, &c, and C. L. Bloxam, formerly first assistant to the Royal College of Chemistry. With a Preface, by Dr. Hoffmann: and numerous illustrations on wood. Philadelphia: Blanchard & Lea. 1854. Svo., pp. 681. (For sale by McKinne & Hall. Price $4 00.)

This is a very elaborate work, containing, besides the Introduction, divisions treating of chemical manipulations, elementary chemistry, and analytical chemistry. The numerous beautiful illustrations added to this American edition enhance the value of the work very much. It is a very valuable addition to our stock of chemical works.

In addition to the works above noticed, we have to acknowledge the reception of a large number of pamphlets, &c., among which we may name the "Transactions of the Medical Association of the State of Alabama," a very creditable volume of 190 pages, to which we expect to refer again. Alabama is among the foremost in medical talent and industry.

Quarterly Summary of the Transactions of the College of Physicians of Philadelphia, from February to April, 1854—a work containing much valuable information.

Remarks on Croup and its Treatment. By Horace Green, M. D. Quite an interesting paper.

The American Journal of Science and Arts. A capital number of a work which ought to be extensively patronized.

Archives de Physiologie de Thérapeutique et d’Hygiène. This is the first No. of a series of monographs to be issued in Paris, semi-annually, by M. Bouchardat, Professor of Hygiène in the Medical School of Paris. The present No. contains 376 pages upon Digitalis and Digitaline, by MM. Homolle and Quevenne, and is calculated to satisfy the most inquiring mind with regard to this remedial agent.

Inhalation and other Treatment in Phthisis. Read before the Suffolk District Medical Society, May 27th, 1854, by Wm. M. Cornell, M. D.—In the Boston Med. and Surg. Journal for April 19, are the following editorial remarks:—"It is said that the benefits of inhaling the vapor of iodine in pulmonary affections, have been underrated. Until within a short time, there has been but little notice taken of iodine in this form of medication; and we are pleased to learn that several medical gentlemen in Europe have lately given their attention to the practical application of it in cases of confirmed phthisis. It is hoped that their use of it will be attended with good results, and
that it may prove a successful remedy in some cases of this intractable disease."

In 1851, in the 43d volume of the same "Journal," the writer communicated some remarks on the subject of "Inhalation in diseases of the air passages and of the lungs." In that paper the belief was expressed that "more benefit may be derived from inhalation than has generally been experienced." The vapor of iodine is there named, among other articles, for inhalation, and cases given from Sir Charles Scudainore, Murray, Barton, and Corrigan, where it had been advantageously employed. Some physicians in this country were disposed to give it, and other articles there named, a trial; among whom, was the late Dr. Peirson, of Salem. The American Medical Association, by their committee on the treatment of these diseases, referred respectfully to these inhalations; and Drs. Wood and Bache, in the last edition of the United States Dispensatory, have mentioned the inhalation, quoted from the Boston Journal. But others looked upon the whole scheme as chimerical, and useless, if not worse.

Since that period, however, medical men abroad, and some in this country, have looked more carefully into the matter, and put the treatment in practice. The writer has watched the reported cases, he believes, in all the Journals, as they have appeared, and still believes, as then expressed, that, if we are ever to find realized the remark by Dr. Armstrong—that "at some future period the whole class of tubercular diseases will be arrested" by medical treatment, no small share of that treatment will consist in remedies addressed, by inhalation, directly to the diseased organs, and to the integument, rather than the stomach of the patient.

It is by no means strange, when phthisis is weekly sweeping to the grave a larger number of victims than any other disease, and this under the most approved treatment, that physicians should look with a somewhat sceptical eye upon any new plan of treatment which promises to be in any measure successful. They doubt (and they ought to doubt) the utility of any plan of treatment, till they have evidence of its success. But they ought, also, to give any feasible plan, which has been stated to be successful, a fair trial. No one doubts that phthisical patients sometimes recover, both with and without treatment. But it does seem incumbent upon the medical profession to put in action all possible means to stay this great ravager of our land.

During the last three years the writer has treated (among many who have died) some phthisical patients who have recovered, among whom were the following:

Mr. M., a young man aged 23 years; well marked phthisis; right lung cavernous. Several remedies were tried, but without any good effect. At last, he was put upon the following:—B. Cod-liver oil, 5iv.; sol. carb. potash, 3ss.; pulv. g. Arabic, 3j; syr. orange peel, 3j; spirit peppermint, 3j. Of this compound, a large tablespoonful was taken four times a day. The patient began to grow better from the commencement of taking the medicine. He also inhaled the vapor of tinct. iodine and creosote, five drops of each in conjunction,
several times a day. The inhalation quieted rather than provoked coughing. But what part of the cure is to be ascribed to the medicine taken, and what to the vapor inhaled, or whether any to either or both, he is not prepared to say. This was the treatment under which he recovered, and he has now remained apparently well more than a year.

Miss C., a young lady from the country, had *pluisis*. Cough excessively harassing; expectoration of viscid pus profuse; night sweats, &c. Was treated the same as the above. She gradually improved in strength and flesh, the cough and expectoration diminishing daily, for eight weeks, when she returned to her home in New Hampshire. Two months after her return, she wrote me a letter, in which she says, "my health is now good, and I am married."

Mr. L., a gentleman aged 27 years, married; residence one hundred miles from this city. Wishing to visit a brother in this vicinity, was recommended to my care by Dr. H., his physician at home. He had disease of the right lung, but did not appear to be a *strumous* patient. He was short, stout built, and had a broad chest. His cough and night sweats were very troublesome. The cod-liver oil he was reluctant to take; and when, at my earnest request, he did take it, it would not set on his stomach. He therefore soon abandoned its use. He was put upon the *leontodie*, the concentrated chemical extract of the *taraxacum*, and the *cornine* of the *cornus Florida* of Kei'h & Co., according to the following:—R. Cornine, grs. iij.; Leontodine, grs. ij. M. Sum at once, three times a day. He also inhaled the vapor of iodine, gutt. x., three times daily. Under this treatment, for eight weeks, he gradually improved, and returned home in much better health than when he first came to me. Each of these patients was freely rubbed with equal parts of warm sweet oil and New England rum, morning and evening.

I am not prepared to say how much of the benefit, which appeared to result from this treatment, was to be ascribed to either the medicine, the inhalation, or the unguent, or whether either did any good. The patients improved while being treated, and that it is all I would say about it.

I have employed inhalation of *creosote*, for more than three years, in phthisis, in bronchitis, and in a few cases of gangrene of the lungs. I was first led to use it in *gangrene*, by supposing, from its name, and the Greek words from which it is derived (*kreas, flesh*; and *soter, preserver*, that is, *flesh preserver*), that it might possibly be useful, as an antiseptic, in a gangrenous lung; and as the best way to introduce it to the lung, resort was had to *inhalation*. At the time when I commenced the inhalation of creosote, I had not seen any account of its having been employed, as a remedial agent, in that form, not even in European journals. But in the second volume of the last edition of Pereira's Materia Medica, re-published in this country at the commencement of the present year, I find the following remarks:—"The inhalation of creosote vapor is occasionally useful in relieving excessive bronchial secretion. This inhalation may be effected by diffu-
sing a few drops of creosote through water or a mucilaginous liquid, and breathing through this by means of the ordinary inhaling bottle."

In the first volume of the same work, Dr. Pereira says, "Inhalation of iodine vapor has been used in phthisis and chronic bronchitis. In the first of these diseases, it has been recommended by Barton, Sir Charles Scudamore, and Sir James Murray. I have repeatedly tried it in this, as well as in other chronic pulmonary complaints, but never with the least benefit."

I think I can say it has been beneficial in my hands, and I shall continue to use it.

I have also, up to the present time, used with benefit the powder of nitras argentii and lycopodium, prepared according to the formula of Dr. Chambers, as published in the 43d volume of the Boston Medical and Surgical Journal, in chronic laryngitis, bronchitis, and incipient phthisis.

I will add in conclusion, that though I never use inhalation of any kind to the exclusion of a general treatment, yet I consider it a useful adjuvant in the management of these generally chronic and often fatal diseases; and if a patient puts himself under my care, I never feel that I have done my whole duty to him, unless I have tried both general and local treatment. I believe the pathology of phthisis has been but imperfectly understood; and that, in most cases, the skin is first in fault. But this is a point on which I will not enlarge at present, only to say, that every one knows how very intimate is the sympathy between the integument of the body and the respiratory organs. Hence, in the treatment of all phthisical patients, I have always paid special attention to the skin. For the last five years I have been in the habit of applying the alcoholic lotion, recommended in such cases by Marshall Hall; but for three years I have directed my patients to be rubbed over the whole surface of the body with equal parts of warm olive oil and New England rum; and it is my opinion that no part of my prescriptions has been more beneficial. If opportunity should offer, I may say more on this treatment at a future time.

I have recently treated a case of aphonia of more than a year's standing, with the alcoholic tincture, iodine 3 j. to 3 j. of alcohol, according to the recommendation of Dr. Bennett, in the Medical Times and Gazette, and republished in the 28th Part of Braithwaite's Retrospect. The tincture was applied, externally, over the larynx, with a camel-hair pencil, every other day for a week. The pain produced by so strong a solution was considerable, and the friends of the young lady, seeing her suffering, desired her to desist. I had concluded to forego its further application, when she suddenly surprised us by the return of her voice. Galvanism had been used for two weeks, previous to the application of the iodine; the current being passed, for fifteen minutes daily from the larynx to the cervical vertebrae. In this case there was no disease of the lungs, but chronic inflammation and enlargement of the chordae vocales. The voice still continues.—[Boston Med. and Surg. Journal.]
On the Production of Local Anaesthesia.—Dr. Snow read a paper before the Physiological Society on the above subject.

He said that the only means they as yet possessed of causing complete local anaesthesia was that of applying cold, as recommended by Dr. James Arnott. When a piece of folded lint, wetted with chloroform, was applied to the skin, and covered with oiled silk, or any other impermeable material, it caused redness of the part, and a sensation similar to that caused by a mustard poultice. In about half an hour there were numbness and a diminution of sensibility, so that pricking with a needle caused less pain than at other times; but he had not been able to produce complete anaesthesia either with this or any other medicine applied to the sound skin. The reason of this was the very slow and sparing manner in which fluids permeated the cuticle, and the circumstance that the small quantity which did permeate the cuticle was afterwards carried away from the part in the circulating blood. When denuded of cuticle by a blister, or any other means, the skin was readily made insensible, even by the vapour of chloroform, so that the raw surface might be rubbed without causing pain. The insensibility caused by the application of a mixture of pounded ice and salt extended only to a very slight depth, as he had ascertained in the following way:—He had congealed part of the palmar surface of the hand and fingers, making a space of two inches square quite hard and insensible, when, on examining the skin which connects the roots of the fingers on the dorsal surface, he found it quite sensible to pricking, even when separated from the palmar surface by a thickness of only the tenth of an inch. The application of ice and salt caused great pain, and the pain was still greater as the sensibility returned in the part, which remained tender for upwards of twenty-four hours. He, therefore, considered this application hardly suited, even for operations confined to the skin, except in cases where the surgeon or patient had an objection to the inhalation of chloroform. The French had lately introduced a plan of refrigeration by dropping ether on the part, and increasing the evaporation by a current of air from a bellows. Some minor operations had been performed in Paris with scarcely any pain, when this process had been applied, and he (Dr. S.) had tried it on a patient of Mr. Ure, in St. Mary's Hospital, with an ulcer of the leg, the callous edges of which it was desirable to divide. The incisions on that edge of the ulcer where the ether had chiefly been applied were not felt at all, and the others caused very little pain. This process was attended with less pain than the application of ice and salt, and probably might be much improved in efficacy.

Mr. Richardson had succeeded in producing local anaesthesia by placing folds of cloth over an ulcer, with an opening in them; over this he placed a piece of lint saturated with chloroform. The whole was enveloped in oil silk covering, and insensibility in the part exposed to the chloroform was the result.

Dr. Crisp had seen Dr. Simpson perform some experiments with
Editorial and Miscellany. [July,

chloroform in 1848. The hand was found to become numbed when placed in a jug containing the agent, although the liquid was not touched. When certain portions of a worm were touched by chloroform, they become paralyzed. There was one curious circumstance about chloroform—viz., nineteen out of twenty persons might take it with impunity, whilst the twentieth, though apparently healthy, might die. The same fact held good with respect to cats, the influence being upon the heart, and dependent upon some peculiarity of constitution.

Mr Potts mentioned a case of severe rheumatism which had been treated with complete success by the application of cold, as recommended by Dr. Arnott. In this case the cold was applied for the space of five minutes, and the patient did not complain of pain.

Dr. O'Connor had succeeded in producing local anaesthesia on ulcerated surfaces by saturating folds of linen with chloroform, and placing them over the part.

Dr. Gibbs said that strong extract of tobacco, when applied locally, acted in some cases as an anaesthetic. — [London Lancet.

Vegetable growth in the "fur" of a coated tongue.—Prof. Alonzo Clark presented to the New York Pathological Society, the results of some recent inquiries made by him, into the nature of the "fur" found in large quantities on the tongue of a patient in a very low condition.

The fur had a mossy white appearance, and was even \( \frac{1}{8} \) of an inch thick, or more, covering the mucous membrane of the lips and cheeks as well as the tongue."

It was found under the microscope, to consist of
1st. Epithelial scales.
2d. Vibriones, and
3d. An abundant vegetable growth.

In the scale, there was nothing remarkable. The vibriones were very large and active animalculæ. The spores from which the vegetable fibres were produced, were "sometimes imbedded in the epithelial cells, and sometimes packed between them"—its quantity was astonishingly great. Dr. Clark found, after research, that he had been preceded by M. Robin a few months in this discovery. This growth is probably common in many exhausting diseases. It is believed to be constantly present in the disease called "muguet" by the French. The sulphurous acid is found to destroy this parasitic vegetation, and may be considered the most appropriate remedy for it.


Death from Chloroform.—Three more cases are to be added to the melancholy list of deaths caused by the inhalation of this potent article.

A woman 37 years of age, who was to have a chronic mammary tumour removed from her left breast, by Mr. Hawkins, at St. George's Hospital, was given chloroform; not more than twenty minims were inhaled, when death quickly ensued.
A man, 18 years of age, died May 3, 1854, at the Locke Hospital, after inhaling chloroform, prior to the operation for phymosis.

The subject of the third case was a woman, about 40 years of age, who was to be operated on by M. Richard, of Paris, for uterine poly- pus. M. R. feared the consequence of giving chloroform, as the patient was much weakened by loss of blood per vaginam, but she required its administration. Chloroform was poured on a piece of folded linen, and held a short distance from her nose and mouth by M. Richard himself. She breathed regularly and quietly, and chloroform was twice added to the compress. The patient became, after a few minutes, very much excited, and required to be held; she was then turned to one side of the bed, and hardly had the surgeon disposed his ligature, when he was told by an assistant, to whom he had given charge of the chloroform, that the pulse had ceased. All efforts at revival were fruitless. The post mortem did not afford any feature of interest.—[Medical News.

Treatment of Psoriasis. By A. Hunton, M.D.—Medical writers have so many different names and descriptions of squamous and papular diseases, that it is rather irksome to follow them; and the multiplicity of varieties serve only to perplex. I would call attention to one form of psoriasis, which appears to be a hereditary disease, and vulgarly called salt rheum,—as also are many other varieties of this as well as some other diseases.

If the following remedy will succeed as well in other hands as it has in mine for thirty years past, it will be found very valuable. For an external application, night and morning, in case of the above complaint, in its varieties, (which are many, where writers split hairs) for chapped hands, sore lips, or for cicatrizing ulcers:

B. Mutton suet, yellow wax, pine resin, lard—of each equal quantities; melt and stir a long time, as the ingredients do not readily incorporate.

In addition to the above an internal alterative is demanded. The article I use for this purpose is one grain of corrosive sublimate dissolved in one ounce of water, and its taste covered by some saccharine substance. For an adult administer of this gtt v. morning and evening; increasing one drop every seventh day till the dose reaches to ten drops. Minute doses of mercury in some form, administered internally or applied externally, or both, constitute the most potent alterative and discutient known to me.

The above preparation applied to indolent tumors, or any tumor which it is desirable to discuss, is a judicious application. Brethren try the above remedies.—[New Hampshire Jour. of Medicine.

Medical in Spain.—The Gazette publishes a Royal decree to the following effect: "Art 1. Every town and locality in the kingdom are in future to be provided with physicians, surgeons, and apothecaries, whose duty it will be to dispense medical aid to the indigent classes, and any other persons who may require their attendance. 2.
The existence of these physicians shall not prevent the free exercise of the medical professions in the same localities. 3. The authorities will maintain in the free exercise of their profession the persons who have been legally accredited, in virtue of the present decree and other ordinances in force. 4. The physicians, independantly of their attendance on the sick, will have to take charge of fondlings, to decide whether substitutes are fit for the military service, and to visit sick soldiers passing through their districts. They are not to absent themselves from the town in which they practice during more than twenty-four hours without the permission of the Alcalde, and for a longer period without providing a substitute. Their salary is to be proportioned to the population of the district, the wealth of its inhabitants, and other local circumstances. They will be entitled to a pension after practising thirty years in the same district.”—[Times.

The worst enemy of the Soldier.—Civilians think that shot kills most soldiers, but Colonel Queach, a Peninsular officer of some experience, and an authority upon the subject, having served throughout the Peninsular campaigns with the old 95th Rifles, says that 40,000 men were killed in action or died of wounds—120,000 died of disease, a great deal of which was rendered fatal by the want of proper medical attendance; whilst 120,000 more were, by disease, rendered unfit for service. What a melancholy retrospect; a numerous and well appointed medical staff is of the first importance in military operations, and would be cheap at any cost, however high or beyond the usual rate.—[London Lancet.

The late Dr. Dalton.—The subscriptions towards a memorial of this highly gifted and distinguished man, the author of “Atomic Theory,” already amounts to £5,312, of which £1,175 is to be expended upon a bronze statue, to be erected in front of the Royal Infirmary, Manchester, close to those of the Duke of Wellington and Sir R. Peel, and £4,125 is to be used in founding scholarships in the New College, Manchester. Of the latter sum £2,500 will be sunk in 4 per cent. corporation bonds, to be divided into two scholarships of £50 per annum, in chemistry, £1,250 for two mathematical scholarships of £25 per annum, and £375 towards an annual prize of £15 for natural history.—[Ibid.

Venereal Ulcers.—The use of iodide of zinc in venereal affections is highly recommended by Dr. Sewell, of Canada, in the Montreal Medical Chronicle. In venereal ulcerations of the throat and nose, it was found effectual, after the disease had resisted the use of the acid permanganate of mercury. To apply it, twist a little cotton wool round the point of a wooden skewer, and having dipped it in the caustic, apply it to the ulcer. One application usually suffices to destroy the morbid action, and is to be followed up with the metallic washes for the throat, and dilute citrine ointment for the nose.—[Virginia M. and S. Jour.