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THE
BRITISH AMERICAN JOURNAL
OF
MEDICAL AND PHYSICAL SCIENCE.

Vol. II.]

MONTREAL, MARCH, 1847.

[No. 11

ON OBSTRUCTION OF THE APPENDIX VERMIFORMIS, AND ON "POST MORTEM" APPEARANCES IN PERITONITIS.

By A. F. HOLMES, M.D., Prof. of Medicine, McGill College.

The interesting case of inflammation of the appendix vermiformis, reported by Dr. Nelson in the last number of this Journal, has engaged my attention, for different reasons, one of which is that a case of an analogous nature occurred to myself some years ago, and another, that it serves to establish the appearances which are to be looked for in peritonitis, and thereby to guide our decision in questions of legal medicine. As the subjects alluded to have no particular connexion, they need not be mixed up, but may be considered separately. I shall, therefore, first state (from memory, for I am without notes), the circumstances connected with the very interesting and obscure case to which I have referred, and which was strikingly diverse from that of Mr. S. in the total absence of the intense suffering which characterized the latter; and, then, refer more particularly to the importance of the case of Mr. S., in establishing the nature of the post mortem appearances, which indicate the previous existence of peritoneal inflammation.

CASE.

The subject of the case was a large, healthy, and precocious child, of the age of 20 months. He became indisposed on the night of Thursday (10th March, 1842), being restless and feverish, but not complaining of pain. The next day he was languid and ill, indisposed to exertion, and unwilling to be moved. On the Sunday he appeared better, but on Monday relapsed into a dull, quiescent state, not seeking to leave his bed, disliking the approach of other children, and unwilling to be disturbed, yet without any marked symptom of disorder. He continued without much alteration till Thursday (17th), appearing to have no particular uneasiness, except a feeling of tenesmus, and an inclination to remain a long time at stool. During this time he had taken some doses of mild cathartics. He had made no complaint of pain or griping; there was no swelling of the abdomen; and no pain had been observed to be felt on handling him. On Thursday evening, I was sent for, in consequence

of a sudden change in the symptoms, and on arriving found the child in a state of collapse—his face pale, skin cold, and pulse nearly extinct. The abdomen was free from tension or swelling; there had been no vomiting; and I elicited no sign of uneasiness when I pressed upon it. He had been put into a warm bath before I arrived, and had appeared pleased for a few moments, by slapping the water in a playful manner, but soon let his head fall back as if exhausted. Ammonia and brandy were administered; but he sank rapidly and expired, apparently quite conscious, and uttering his mother's name.

Post Mortem.—On opening the abdomen, the contents, at first view, exhibited nothing anomalous, but upon raising up the small intestines from below, all those portions that lay over the pelvis, or in contact with the cæcum, were seen in a state of intense inflammation, in parts, nearly black from extreme congestion. Endeavouring to discover the cause of this violent circumscribed action, I found the appendix vermiformis in its natural situation, and scarcely changed in colour; but on the left side a small portion was softened and broken down, exhibiting an aperture, the sides of which were quite diffluent. In handling the appendix, which was not enlarged, a hard substance filling its caliber was felt, and upon slitting up the tube, a small concretion, about 3-10ths of an inch long, and 2-10ths of an inch broad, and nearly cylindrical, was turned out. It was of a dull white colour, as if calcareous, and pretty firm, though crushed without difficulty between the finger and thumb. No fecal matter could be detected as having escaped; indeed, the concretion lay between the perforation and the natural opening into the gut.

The preceding case is of so anomalous a character that it may be regarded rather as curious than useful; and the imagination is taxed to give a reasonable interpretation of its phenomena.

The symptoms in the case of Mr. S. were such as we might naturally look for from a highly sensitive part undergoing the intense inflammation antecedent to mortification, but in the other case no indication was afforded of the fatal mischief which was taking place. If permitted to conjecture the course of the disease,

we may suppose that impaction of the solid substance first took place, which, proving a source of irritation, led to a very partial and subacute inflammation, terminating in ulceration and perforation; and that, when the latter occurred, it was immediately followed by general collapse, but with intense local action. I am quite at a loss, however, to account for the total absence of pain while the parts were thus violently affected. If, as I am disposed to believe, the inflammation in the neighbouring parts was owing to the escape of matters from the intestinal canal, they must have been gaseous, as no fecal matter could be discovered, and the concretion, though it did not distend, yet filled the caliber of the tube.

We have, exhibited in this case, one of those strange anomalies which pathology sometimes presents, and which we are obliged to leave undeveloped. It is one of the most striking instances of *latency*, in an affection commonly denoted by unequivocal symptoms, that I recollect to have heard of. It is true, violent peritonitis, pleurisy, pneumonia, &c. (as shown post mortem), do occasionally occur without any of the usual symptoms; but such cases are almost invariably accompanied or preceded by cerebral affection, which, as pointed out by many pathological writers, entirely masks the rational signs of these affections. In the present case, however, that explanation cannot be given, as the intellectual faculties appeared to be retained unto the last.

I was subsequently informed, that, although the child appeared quite well for several days before his attack, and had been remarkably healthy, there had occurred, at two or three different times, during the previous fortnight, fits of violent crying, the cause of which could not be detected, and which (as he, after having been pacified, showed no sign of illness) were attributed to passion.

I shall now proceed to consider the important light which the case of Mr. S. throws on

The Post Mortem Appearances in Peritonitis.

When I allow myself to apply the term "*important*" to the elucidation of a subject, which I believe is not at all a matter of doubt among pathologists in general, I have reference almost entirely to its local influence in removing erroneous opinions, and establishing fixed principles: and in order to make myself understood; and in order also to show the points upon which I desire especially to fix the attention of medical practitioners in the province, it will be necessary that I recall some circumstances of a disagreeable nature which occurred nearly three years ago.

It is obvious, that it is of the greatest importance for medico-legal investigations, that the signs of any lesion should be accurately defined. Vague apprehensions as to the appearances to be expected, might lead to most disastrous consequences. In like manner, for prosecuting merely scientific investigations, accuracy is required, or error might be the result. Hence, in endeavouring to point out what are the sure signs upon which we can predicate the previous existence of peritonitis, I believe I am doing a service to the profession.

In April, 1844, during a riot which occurred, a man, named Champeau, received a bayonet wound in the abdomen, and subsequently, after some days' illness, died. An inquest was ordered, and I was requested by the Coroner to assist at the examination of the body; and was subsequently called upon to give evidence before the Court. Dr. Nelson, having been the man's medical attendant, was likewise examined. He declared that Champeau had laboured under intense peritonitis—an opinion from which I, being necessarily bound to form my judgment from what I saw, entirely dissented. This discrepancy of opinion led to a subsequent controversy in the *Medical Gazette*, in which Dr. N. endeavoured to show that what I stated to be the usual appearances after peritonitis, viz, adhesions, effusion of lymph, of serum, &c., did not occur till the disease had existed a considerable time, and, consequently, that I was not warranted in asserting that peritonitis had not preceded death. His words are: "The medical gentleman seemed to rest his conviction that there was no inflammation on the absence of effusion, coagulated lymph; also, because there were no new adhesions: forgetting that those are the products of slow or sub-acute inflammatory action, and when present, prove that it had been protracted, and passed through some of its phases. It is only when the inflammation is less acute, that it provokes an increased action from the exhalents; that serum first, then coagulated lymph, and finally adhesions result;—this likewise takes place when the inflammation has been in part subdued, and assumes the chronic character; then, indeed, you have 'effusion' enough."—*Montreal Medical Gazette*, p. 169.

Having thus recalled enough of these bygone circumstances to make my subsequent remarks intelligible, I shall only add in reference to them, that I believe the opinion I had given was completely established by the proofs elicited in the discussion. Still, I have reason to think that there are many, (especially among those whose opportunities of post mortem examinations are not frequent), who are still uncertain as to what constitutes, in a dead subject, the evidences of a previous peritonitis. It is to the object of removing this uncer-

tainty, that the remaining portion of this paper will be devoted.

The Rev. Mr. S. was taken ill on Friday afternoon, and died on Monday at 2 A.M.* The symptoms detailed show, that, till Sunday morning, the disease was confined to the appendix: there was not the slightest tenderness except at this one point. "At 9 A.M.," Dr. N. says, "we found a striking alteration, * * abdomen becoming tense; pain more diffused." At 1 P.M. "pain had shot all over the abdomen." Again, "It is worthy of remark, that so long as the pain was confined to the region over the appendix vermiformis, the pulse, skin, and general aspect, were those of inflammation of a mucous surface; but as soon as the pain spread at once all over the abdomen, then the symptoms characteristic of inflammation in the serous tissue, or peritonitis, became very striking."

It is admitted, then, that peritonitis commenced about 9 o'clock Sunday morning, and the patient died next morning at 2 A.M. Seventeen or eighteen hours intervened between the supervention of the peritonitis and the decease of the patient.

Now, if we find that in this very rapid case there were produced adhesions, or effusions, it must entirely overturn the opinion so confidently enunciated, that "when present, they prove that it had been protracted, and passed through some of its phases;" and as entirely confirm the opinion which I was compelled to adopt by the inspection of Champeau's body.

What were the appearances?

Dr. Crawford says, "On laying open the abdominal parietes, the peritoneal coat of the intestines, generally, was of a bright rose colour; and was in many parts covered by an exudation of coagulable lymph, particularly where the bowel doubled on itself; these knuckles were in consequence slightly adherent together: a few ounces of turbid serum commixed with lymph were found in the peritoneal cavity."

Feeling it impossible to increase the conviction that must arise in the mind as to "what are the post mortem appearances in peritonitis?" I shall conclude by quoting one of the most recent and excellent French authors, who broadly asserts that "THIS SECRETION (viz., lymph) IS THE VERITABLE ANATOMICAL CHARACTER OF PERITONITIS."

I thus translate the words of Grisolle, whose work was published in 1844:—

"It has been said that when patients die at a period very near the commencement, as 12 or 24 hours, the

peritoneum was found only injected, red, dry to the touch, and glistening. If, however, in these cases, we examine with much attention, there will be always found on some points an extremely thin coat of an albuminous matter, semi-concrete, which causes the convolutions of the intestines to adhere together. This secretion is the true anatomical character of peritonitis."

Montreal, February 22, 1847.

OBSERVATIONS ON THE CURABILITY OF OPACITIES OF THE CORNEA.

By HENRY HOWARD, M.D., M.R.C.S.L.
Surgeon of the Montreal Eye and Ear Institution.

It will readily be allowed that our information on the therapeutics of the eye, is still in its infancy, and consequently that there is wide scope for investigation under this head. My attention was forcibly arrested by an article quoted from a Dublin Journal, which appeared in the June number of the *British American Journal of Medical and Physical Science*, headed "Prussic acid in ophthalmic diseases."

Opacities of the Cornea and their removal, have engaged my attention for a considerable period; and I have for some time kept a record of such cases, with the results, as have been treated by myself. Dr. Jacob's name must ever secure respectful attention with deference to any of his promulgated opinions; yet, if we always deferred to authority, where would be the boasted progress of the nineteenth century. Ought we blindly to bow to authority, and not seek to extend the bounds of knowledge?

Before proceeding further, I would beg to state that I highly appreciate the honour and opportunity I enjoyed of acquiring the ophthalmic art under Dr. Jacob's tuition, and that I now feel grateful to him for the instruction imparted; and although the statements which follow directly impugn his assertion, I appeal only to facts, at the same time sensible that without his previous advances this point could not have been attained, viz.: the curability of opacities of the cornea.

Dr. Jacob, after alluding to the cases of opacity of the cornea, stated by Dr. Bigger to have been cured by prussic acid vapour, concludes by saying, "That the cures might be only apparent, and might perhaps with more justice be referred to the natural salutary processes of the animal economy, which in the course of time succeed to the formation of those opacities, whether it be the mere subsidence of inflammatory action, or the agency of the absorbents; but for my part, the conviction on my mind, for many years, has

* In the account given in the last Number, p. 31. is erroneously put for A.M. See *La Lancette Canadienne*, where the case is also given.

been, (and nothing that I have heard lately tends to shake it,) that however dense these opacities become, even were they as white as paper, they will be obliterated in time, unless the product of destructive ulceration in the cornea consequent on wounds or ulcers; unless in fact, they are actual cicatrices."

Cold comfort this to give to a poor fellow blind of both eyes from *nebulæ* or *leucomata*, that if he will only have patience, nature will cure him before he dies, or perhaps not; for, says Dr. Jacob, surgery can do nothing for him.

Now, from considerable experience, I assert that a majority of cases of opacity of the cornea are curable or susceptible of great amelioration, and even in many of those opacities caused by cicatrices, their extent may be diminished, and vision restored to a greater or less extent.

If a case of blindness from opacity of the cornea presents itself to me, of many months or years standing, and that under treatment the opacities are removed, and sight restored in from four to six weeks, have I not a right to conclude that the treatment operated the cure, if not, then no fact in therapeutics is sure: *a fortiore*, if many such cases present themselves with similar results, the inference must be irresistible.

I propose to give two cases severally of *albugo*, *leucoma*, and *nebula*, their treatment and results. During thirteen months, forty-eight cases of opacity of the cornea have been treated at the Montreal Eye and Ear Institution, of which twenty-three were *nebulous*. Of these, eighteen were cured and five relieved: of eighteen with *albugo*—twelve were cured and six relieved; of seven with *leucoma*—six were relieved, and one abandoned as incurable. In selecting the following cases, I wish it to be understood that, if desirable, I could furnish many more.

CASE 1.—ALBUGO.

Mary Harrigan, æt. 30, wife of a labourer, April 2r 1846, had had sore eyes for three years; for a year had merely distinguished the light, and the outline of large dark objects, and had not been able to go alone through the streets. She had an *albugo* on the right eye and three on the left, completely obstructing vision. Perfectly cured in six weeks. Treatment—fumigations with hydrocyanic acid every day for ten minutes; after the lapse of another ten minutes, put one drop of a solution of nitrate of silver, gr. x. a. ʒi. into the eyes. For first fortnight took a wineglass full of the following mixture every morning—

R. Infusi Gentianæ, ʒviii.
Sulphatis Magnesiæ, ʒi.
Acid Sulph. Arom., ʒss. m.

CASE 2.—ALBUGO.

Feb. 8, 1846.—Ann O'Berne, æt. 26, a servant, has been gradually losing the sight of the left eye for some time, but had lost it completely for the last eight months. Dr. —, to whom she had applied, told her nothing could be done. On examination, I found an *albugo* completely obstructing the pupil of the left eye, in fact, occupying the whole cornea. Cured in two months. Treatment as in preceding case.

CASE 3.—LEUCOMA.

Dec. 26, 1846.—John Gilliland, æt. 22, a ploughman, had *leucoma* of both eyes, completely occupying the left cornea, and preventing all ingress of light. The lower third of the right cornea was imperfectly clear, allowing of sufficient light to pass to enable him to guide himself through the street. Sufficiently cured in three months to guide the plough, a very small spot only remaining on the right cornea, and that not over the axis of vision; a small round spot over the axis of vision on the left cornea was removed. Treatment—daily fumigation of the eyes with hydrocyanic acid, and the subsequent application of Janin's ophthalmic ointment, and every ten days the application of the solid nitrate of silver to the cornea. Internally took gentian and salts.

CASE 4.

Feb. 11, 1846.—David Wark, æt. 14, some time previously had received a severe blow on the left eye with a stick, which had ruptured the cornea horizontally, and in healing had left a cicatrix about three lines broad across the eye; to the outer side of the cornea the iris had prolapsed and become attached to the cicatrix. He saw only the upper and under part of each object. Dismissed in six weeks with the cicatrix reduced to a mere line, and, by his description, the vision as good as in the right. Treatment—daily fumigations with vapour of hydrocyanic acid, and a small portion of the following ointment put into the eyes each day—

R. Ungt. Ophthal. Jan.
— Hyd. nit.
— Cetacei aa ʒi. m.

CASE 5.—NEBULA.

June 11, 1846.—Robert Hughes, æt. 55, a veteran, was led to the Institution by his wife; the right eye was destroyed, and vision in the left was completely prevented by *nebula*, both the result of inflammation. He had just arrived from New York, where he had been under the surgical treatment of the most eminent practitioners in that city, and hitherto the case had only gone on from bad to worse. After two months' daily attendance, he was discharged with very fair vision, sufficient to enable him to transact his ordinary business. Before returning to Wales, his native country, he left a certificate with

me (as a voluntary effusion of gratitude), stating the benefit he had received under my care.

The treatment consisted in daily fumigations with hydrocyanic acid,—a drop of 10 grain solution of nitrate of silver, and after insulating him, drawing electric sparks from the eye and surrounding orbit.

CASE 6.—NEBULA.

S. M., æt. 13, called on me, May 2, 1846, complaining of dimness of vision of right eye, which had existed since he had had the measles in infancy. Had been treated unsuccessfully in New York by several oculists. The whole cornea was obscured by nebula; was perfectly cured in six weeks. Treatment—daily fumigations with hydrocyanic acid, and application of 10 grain solution of nitrate of silver. During the treatment, he took a considerable amount of the ioduretted solution of the iodide of potassium.

Note.—Janin's ophthalmic ointment is made as follows:

℞ Bol. Armen.
Tutia Prep. aa ʒii.
Hyd. Precip. Alb. ʒi.
Axungia, ʒi. m.

CASE OF PERIOSTITIS, TREATED BY IODIDE OF QUININE.

By J. DUNCAN M'DIARMID, Esq., Staff Surgeon, Prescott.

Mr. M., of Ogdensburg, U. S., a young man of regular habits, and of a generally healthy appearance, and never having suffered (as stated to me), from syphilis in any shape whatever, applied to me under the following circumstances:—

He stated that during the summer, (it being autumn when he consulted me,) he had had an attack of bilious fever, but that his general health had been very good, until about three weeks or a month back, when having travelled all night in a waggon, the weather being very cold at the time, he became affected with severe pains of a rheumatic character—particularly severe at night—by which he was deprived of rest till toward morning, when he experienced some remission, and continued literally free from suffering during the day; complained, at the same time, of night sweats, and loss of flesh and appetite; while over the frontal bone on the left side, and over the upper third of the sternum, were nodes of a considerable size, with symptoms of something of the same nature about the middle of the left humerus, which, however, could not be felt. The "headache" in the frontal region was so severe at times, as to be almost intolerable, the nodes had attained their present size in the course of about three weeks.

I prescribed cold bathing daily, by means of a wet towel out of cold water, rubbing the skin smartly from head to foot; diet as usual; no stimulants; and the

iodide of quinine, as prepared by Staff Surgeon Dr Spence, (*Montreal Medical Gazette*, Vol. i. Page 2.) In the course of about three weeks, the nodes and the distressing symptoms generally, had nearly disappeared; and in less than six weeks he was quite recovered.

Prescott, January 18, 1847.

CRITICAL REMARKS ON THE LABOURS OF E. S. DE ROTTERMUND, ESQ., LATE CHEMIST TO THE PROVINCIAL GEOLOGICAL SURVEY.

By H. CROFF, Esq., Prof. of Chemistry, King's College, Toronto.
To the Editor of the British American Journal.

Some time since you were kind enough to publish in your excellent Journal (Vol. II. No. 2) some of my notes respecting the Tuscarora Sour Spring, near Brantford, in which I announced the very interesting fact of its containing free sulphuric acid; but the examination was very imperfect, partly from my want of leisure, but more from lack of material. In a note appended thereto, you mentioned that Mr. De Rottermund had discovered antimony in the same spring; and, in my letter of the 12th June, published in the August number, I fully proved the incorrectness of his analysis, or (to speak more correctly) of his statement, for I cannot believe that he ever made any analysis of the water.

In my first paper, I mentioned that it was my intention to proceed during the summer vacation to the spot, and thoroughly examine all the circumstances connected with this very curious spring, which may properly be called one of the wonders of Canada. I was prevented from so doing by untoward circumstances; and, as I wished my next communication on the subject to be a complete and final one, I should not as yet have put pen to paper concerning it, had it not been for the recent appearance of a pamphlet entitled, "Report of E. S. De Rottermund, Esq., Chemical Assistant to the Geological Survey of the Province,—Printed by order of the Legislative Assembly,"* in which this spring and its constituents are mentioned.

Mr. De R. seems to have obtained results widely differing from mine; and, as my character as an analyst must fall to the ground if his statements should be proved to be correct, I have no hesitation in requesting you to insert the following critical remarks on his assertions.

I shall first refute Mr. De R.'s statements contained in that part of the pamphlet most interesting to myself, and shall afterwards take the liberty of making a few remarks on the other portions of this highly amusing and most extraordinary publication.

*In justice to Mr. Logan, the talented gentleman occupying the distinguished position of Provincial Geologist, we must observe, that the report of Mr. De Rottermund was not printed with his sanction, nor has he anything, whatever, to do with its appearance.—Ed.

At page 4, Mr. De R. says, " For the same reason, I have called the acid springs of Brantford antimoni-ferruginous, on account of the presence of that substance (Query, what substance—antimoni-ferruginous?) only recently found in mineral waters; and because it is one of the most remarkable substances as a remedy for several diseases."

Your medical readers, who may be fond of materia medica will no doubt be glad to add this new substance antimoni-ferruginous to their Pharmacopœas, but I doubt whether any one besides Mr. De R. will ever find it in the Tuscarora Spring.

In the succeeding sentences, Mr. De R. objects to its being called an acid spring, and desires that it may be known by the above name proper to its composition. As I have shown it to be strongly acid, from the presence of free oil of vitriol, I do not see any objection to the name, especially as the carbonic acid springs are generally called " carbonated waters."

At page 10, Mr. De R. enumerates the substances contained in the spring, viz., hydrosulphuric and carbonic gas (I quote his words), sulphate of protoxide of iron, sulphate of alumina and potash, chloride of antimony, chloride of zinc, sulphate of magnesia and of lime, resinous substances, and vegetable albumen; from which no one would conclude that there is any free sulphuric acid present at all.

Mr. De R. speaks of three or more springs. That which I formerly examined was from the middle spring. Its specific gravity was 1.0038; it contained peroxide of iron, arising probably from its having been long kept. It contains no antimony, as I have again proved by new experiments. Some time since, through the kindness of Mr. William Boulton, I was put in possession of three small bottles of the water from the north, south, and middle springs. They were well sealed; but the water contained no trace of hydrosulphuric acid, which, however, does not prove that this gas may not be present in the fresh water.

The water from the Middle Spring had a specific gravity of 1.0037 at 50° Faht., and contained no antimony. From the North Spring, spec. grav. 1.0030—no antimony. " South " " 1.0060 "

Mr. De Rottermund says the water contains zinc. Water from each of the three bottles was treated with ammonia in excess—the filtered solution treated with hydrosulphuret of ammonium—

Middle Spring—contains no zinc.
North " " "
South " " "

Mr. De R. says the water contains chlorine. Water from each of the bottles was treated with nitrate of silver—

Middle Spring—a scarcely perceptible opacity, hence little or no chlorine.
North Spring—no change, hence no chlorine.
South Spring " " "

The water from all the three springs contains protoxide of iron, as is shown by the greenish precipitate caused by ammonia; but the south spring contains the smallest quantity of protoxide, the precipitate consisting almost entirely of peroxide.

Several ounces of water from the middle spring were boiled with nitric acid and precipitated with ammonia; the precipitate washed and digested with a solution of caustic potash, the filtered solution neutralized with hydrochloric acid and precipitated with ammonia—a slight trace of alumina was found. In my first analysis I found peroxide, and not protoxide of iron, which was most probably caused by the oxidation of the protoxide from long keeping. This water was clear and colourless, while the specimens given to me by Mr. Boulton were of a yellowish colour.

I have shown in my first paper that the spring contains lime and magnesia; these were removed (after the separation of iron and alumina) by the well known process of precipitation by acetate of baryta, &c. &c. The treated residue boiled with water, the solution filtered, evaporated to a syrup, mixed with alcohol, and inflamed, communicated a slight tinge of yellow to the flame, showing the presence of soda; the residue dissolved in a very small quantity of water, and treated with bichloride of platinum, gave a yellow precipitate indicating potash.

I have, therefore, shown, that of the substances (exclusive of gases) said by Mr. De R. to exist in this spring, viz., iron, alumina, potash, chlorine, antimony, zinc, magnesia, lime, resins, and albumen, three are certainly not present, viz., zinc, antimony, and chlorine. That resinous substances should be present seems to me to be utterly impossible, for any work on Chemistry will inform Mr. De R. that resins are insoluble in water. The presence of vegetable albumen seems equally problematical, for its sulphate is insoluble in acid solutions, and no precipitate is formed in the Tuscarora water, either by ferrocyanide of potassium, or by bichloride of mercury (corrosive sublimate).

Organic substances certainly are present, for if the iron and alumina be precipitated by ammonia, and the residual solutions evaporated to dryness and heated, considerable blackening takes place; but we know that crenic and apocrenic acids are frequently found in mineral springs (Berzelius, Hermann), and we know that these acids combine with protoxide of iron to form soluble salts, while they produce insoluble ones by their union with the peroxide; and knowing, moreover, that

these or similar acids are found in rotten wood, in ochre, and in numberless putrefying vegetable productions, it is easy to account for the presence of organic matters in this water, without reference to such out of the way substances as resins and vegetable albumen.

Of the ten substances found, therefore, five may fairly be presumed to exist only in Mr. De R.'s imagination.

At page 10, five lines from the bottom, we read, "The earth is filled with sulphur crystallized in fine grains." I have some of the earth, which, as might naturally be supposed, exhibits nothing of the kind. It contains iron and sulphuric acid; but of this more on a future occasion.

At page 11, we have a very interesting statistical account of the diseases prevalent at Brantford, and of the cures effected by the water. This portion of the pamphlet I will leave to the critical acumen of your medical readers; but I may remark that the cures are not to be ascribed to the presence of Mr. De R.'s "antimoni ferruginous," but simply to that of free sulphuric acid, for every one, who is even as little acquainted with medicine as I am, must be aware that sulphuric acid is, or has been, employed in inflammation of the eyes, in cutaneous diseases, and in the treatment of sores.

At page 12, we have the following passage, which I am sure you will excuse my quoting at length, inasmuch as I am quite certain you wish to amuse as well as instruct your readers:—

"It may be useful to remark, that if water containing zinc and potash (!), as well as alum combined with potash (!), is very scarce (perfectly true), water containing antimony is still more important on account of its still greater scarcity and medicinal power. The salts of antimony are of the greatest value, on account of the great difficulty of preparing them properly; for the chloride of antimony becomes decomposed in water, while here it is produced by the presence of the acid, and of organic matter. The discovery of antimony in a mineral spring is undoubtedly an immense advantage for the science of medicine; for the same substance prepared artificially is never so efficacious as when found in water, &c. &c. &c. I will take the liberty of explaining the theory of the formation of this antimoni ferruginous spring, and of explaining the cause why the antimony is in solution without being decomposed or precipitated by the water. According to geological researches, it is known that this place contains a great many marshes, iron pyrites, lead ore, zinc, and antimony, as well as beds of gypsum. Water running through iron pyrites or gypsum, by some chemical or other phenomenon, becomes decomposed, and charged with a quantity of sulphuric acid; (the waters of this spring prove its presence in a very decided manner;*) the water so acidulated, passing through turf or a marsh covered with vegetation, must contain vegetable albumen which I have found, organic acids, resinous substances, &c. If the water thus charged passes afterwards

through beds of ore of antimony, it is natural that it will dissolve the substance without precipitating it."

In the first place, the statement of any preparation of antimony prepared artificially not being as efficacious as when found in water, is, *in abstracto*, a decided fallacy. I believe (speaking under correction) that the idea of springs possessing peculiar virtues, not to be imitated by artificial means, is now entirely exploded. But let us look at Mr. De Rottermund's theory, and we will suppose that all the substances he requires do really exist in the neighbourhood. Did any one ever hear of water being decomposed when in contact with sulphate of lime, and becoming charged with sulphuric acid? In that case half the springs known in the world should contain it. Mr. De R. very properly adds, "by some chemical or other phenomenon." But by means of iron pyrites sulphuric acid might be formed; this, says our author, passes through turf or marsh covered with vegetation, and becomes charged with vegetable albumen (?), organic acids (here mentioned for the first time), resinous substances (?), &c. &c. &c. Now, this solution, "passing through beds of ore of antimony, will naturally dissolve the substance without precipitating it;" that is to say, water containing sulphuric acid, resins, albumen, and crenic acid, will dissolve an ore of antimony (probably the sulphuret). I am afraid Mr. De Rottermund is like the facetious old gentleman, who, hearing a friend cry out, "*lapsus linguæ*," when his servant let fall a boiled tongue, caused his own attendant to be equally awkward with a round of beef, and expected to gain great applause by a repetition of the witticism.

Mr. De R. has heard that when the oxide or sulphate of antimony is boiled with bitartrate of potash the oxide is dissolved; and that the precipitancy of the oxide, or basic salt of antimony, by means of water is prevented by the addition of some organic acid, and he wishes us to believe that something similar takes place in the present instance. Let him believe it who can. Besides, he need not have given himself the trouble of accounting for the antimony, because there is none there; which puts me in mind of the twenty-fourth reason for not ringing the bells, viz., because there were no bells to ring.

In the preceding remarks I have shown the utter fallacy of Mr. De Rottermund's statements with regard to the Brantford Springs; and in my next communication I shall take the liberty of endeavouring to prove similar inaccuracy in the remaining portions of the pamphlet. I may, however, remark, that it is scarcely necessary to attempt a refutation, for Mr. De R.'s statements are unsupported by experiments, or the descrip-

* Here mentioned for the first time.—H. C.

tion of one single analysis either quantitative or qualitative. I have not found in the whole pamphlet so much as a statement regarding the specific gravities of the waters spoken of, except in one place, and in that the numbers had, I believe, been determined by other chemists.

Without some such guarantee, Mr. De Rottermund cannot expect that any one of his statements will be believed by the scientific portion of the people of Canada, although it is probable that so flourishing a report may obtain credence among those who may have as inaccurate a knowledge of chemistry as even Mr. De Rottermund himself—a portion of the community which, it is to be hoped for the credit of the country, will be found to be exceedingly small.

Toronto, 9th Jan., 1847.

ANATOMY AND PHYSIOLOGY.

THE ANATOMY OF THE EXCITO-MOTOR SYSTEM.

By MARSHALL HALL.

The history of the protracted disputes on this topic would be full of instruction, but it is not my present intention to write on it. My object is, to lay before the readers of the *Lancet*, in a few words (I am always afraid of occupying their time and my own needlessly), the argument, or rather, the plain and simple proofs, of the distinct anatomy of the excito-motor system.

Does any one doubt the distinct anatomy of the system of cerebral nerves—of the nerves of sensation and volition?

The very same proof which exists of this part of the nervous system, exists in regard to the excito-motor system. It exists in the pneumogastric nerve, or, as it may be better designated, the pneumogastric system of nerves.

If the pneumogastric be sentient at all, it is the least sentient of all incident nerves. What is it then? It is excito-motor! It is, emphatically, the internal, excito-motor nerve!

Why go to complex structures, when a simple one exists? Why go to the lower order of animals when the mammalia, and even the human being, afford us the proof we require?

The superior and the inferior laryngeals are the associated excitor and motor nerves of the larynx.

The bronchials are associated excitor and motor nerves of the bronchia.

The pharyngeals and œsophageals are associated excitor and motor nerves of the pharynx, of the œsophagus, and of the cardia.

Lastly, and most strikingly, the pulmonic part of the pneumogastric nerve is, as the associate of the diaphragmatic and intercostals, the internal excitor of respiration.

There is, in short, as I have said, the same proof of the distinctness of the excito-motor system of nerves, as of the sentient and voluntary, and it is both idle and ridiculous to dispute the fact any longer; or to appeal to other parts of the nervous system than the grand pneumogastric, or to other tribes of animals than the mammalia, for proofs not needed. As confirmations of a truth already established, these researches are, of course, interesting enough. I am myself preparing a paper on the pneumogastric system of animals of limited and of diffused respiration—in the mammalia; and in birds and insects: In birds, the spinal nerves are, doubtless, in their distribution to the diffused, breathing cells, analogous to the pulmonic branches of the pneumogastric. In insects, each segment with its spiracles (analogous of the larynx, trachea, and bronchia), is endowed with a nervous system entirely analogous to the laryngeals, and to the pulmonic branches of the pneumogastric, and the diaphragmatic, or intercostals! Then we have to inquire into the nature and office of the lateral nerve in fishes. As in birds, the respiratory nerves are, probably, equally used for flight and for respiration; so in fishes, the lateral nerve is, probably, for swimming and for respiration.

But to return to my topic. The proof of the distinct anatomy

of the excito-motor system is afforded by the pneumogastric—the internal, purely, or almost purely, excito-motor nerve.

If, however, we would examine other and more complicated tissues, the proof lies, not, I fear, in the dissection and tracing of fibres, but in physiological experiment; the cerebral system is, so tested, in excitor throughout—in its centre, in the nerves of special sense; the excito-motor system is, in its centre, and in its incident and reflex relations, what its designation implies.

It is pitiable that there should any longer be any dispute on the subject, or that detraction should still attempt to wrest the credit of adducing the proof, in any degree, from myself, or from physiology.

Amongst other attempts of this kind, one has been to propose a change in the designation which I had given to the nerves of the reflex arc—and a most unfortunate change, too. The terms incident and reflex imply some very definite association, or *Law of Relation*, between the two—a real phenomenon of the most remarkable kind. But the terms *afferent* and *efferent* are, in this respect, utterly insignificant; whilst the meaning which these words do convey, of something borne to and from, is probably altogether erroneous.

The ray of light, which is now incident and immediately afterwards reflected, is the same ray, modified, directed, and returned by the reflector, whether it consist in locomotive particles, or in vibration. The same idea is attempted to be conveyed by the terms incident and reflex nerve. There is, in these nerves, and in their connection through the spinal marrow, some extraordinary recondit connection, so that, for example, the excitation of the superior laryngeal sends forth some mysterious messenger to the medulla oblongata, whilst this returns it in the just channel, the inferior laryngeal, so as to effect the closure of the larynx; whilst the excitation of the pulmonic branches of the pneumogastric excites, through the diaphragmatic and intercostal nerves, the contraction of the muscles of inspiration, precisely, definitely, and no other.

The ordinary reflexion of a ray of light, or the polarisation of a ray of light, is not more definite.

The effect produced is obviously *designed*, not by the animal—for its brain may be removed without interfering with this process—but by an omniprovident Creator. This obvious design has misled many to think that there are feeling and volition in the spinal marrow.

The terms incident and reflex are therefore full of meaning; whilst the terms *afferent* and *efferent* either convey no meaning at all, or an erroneous one. In this suggestion, the *Law of Association* of the effects of excitement, its incident course, its modification and direction by the spinal marrow, its reflex course and destination, were unperceived.

How much, then, is conveyed or implied in that one word, *reflex*,—incidence, reflexion, appropriate combination, and destination! And how devoid of all meaning are the words *afferent*, and *efferent*, not very modestly attempted to be substituted for it!

I beg my reader to study and compare the physiological movements in the acts of inspiration, with their pathological forms in asphyxia: the first are reflex, normal, and beautifully appropriate; the second are, in respect, abnormal and deranged.

My opponents are much disposed to speak of the class of reflex actions, in general terms, as known to Redi, Whytt, &c. This is another ill-chosen but deceptive phrase. The reflex actions, as I have *always* said, were spoken of by many previous physiologists; but the phrase I have adopted from the very beginning,—for the very title of my first paper,—was *reflex function*; and this expression, with its fulness of meaning, as applied to all the acts of ingestion and egestion in the animal economy, had been used, could have been used by no one; for as the idea of an incident excitor nerve, with its physiological relations, did not exist in anatomy, so the idea of a reflex function, with its anatomical relations, did not exist in physiology.—*Lancet*.

PRACTICE OF MEDICINE AND PATHOLOGY.

ON THE USE OF CAUSTIC FOR THE BITE OF RABID DOGS.

“The best caustic, I apprehend, for you to use on these occasions is the caustic potash; and for this reason: that it dissolves the parts with which it comes in contact, and that afterwards the dissolved caustic penetrates still further beyond the part to which

it has been actually applied. If the tooth penetrate to the cellular membrane, by the time that you are consulted some of the saliva may have reached the cells beyond and if you apply the nitrate of silver, or the nitric acid, these will coagulate the fluids and harden the solids, while the caustic potash becoming diffused will follow the course of the saliva. A convenient way of applying the caustic on these and some other occasions is this: melt it in a silver or platinum spoon, and, when melted, dip into it the blunt end of a probe. It will come out with a varnish of the caustic upon it; dip it in again until the button of caustic has attained a sufficient size. By means of a probe thus armed you may carry the caustic even into a very narrow wound, so that you are sure it will penetrate wherever the dog's tooth has penetrated; after which, from the particular nature of the caustic (as I have just explained) you may be certain that it will penetrate still further, and as far as the poison can have reached."—*B. Brodie.*

CAUTION IN THE USE OF CAUSTIC TO THE SCALP.

The application of caustic to tumours on the scalp must be made with great caution, as appears from the following case:—

"A surgeon applied the caustic potash to the scalp, with the view to make an issue in a man's head, who was labouring under a headache and nothing else. When the slough had separated a piece of the occiput was exposed, as large as half-a-crown or larger. The patient was soon seized with a sort of strange symptoms, and died. It was found that the dura mater had become detached from the inside of the bone, just opposite the part where the pericranium had been destroyed on the outside; and it was clear that the sloughing of the dura mater was the cause of the man's death."

When the caustics are used, it is prudent to have some counter-agent at hand to stop their action on the sound parts around. "Acids may be neutralized by alkalis; caustic potash may be neutralized by vinegar, or by a solution of the diacetate of lead. If you are afraid of the nitrate of silver burning the neighbouring parts, its action may be neutralized by common olive oil. A solution of bicarbonate of potash will decompose chloride of zinc, and so of other caustics."—*Med. Chir. Rev. July, 1846.*

ON THE EFFECTS OF MERCURY ON THE YOUNG SUBJECT.

By JOHN B. BRECK, M. D., Prof. of Materia Medica and Medical Jurisprudence, in the College of Physicians and Surgeons, of New York.

In some previous papers,* I endeavoured to point out the peculiarities attending the operation of opium and emetics, on the infant subject, as distinguished from the effects of these agents on the adult. I now propose to make some remarks on another article of even still greater importance, and that is *Mercury*. That mercury is an agent of immense power, either for good or evil, upon the human constitution, cannot be questioned. While in many cases it is the means of saving life, in not a few it unquestionably destroys it. If this be so, it becomes a question of the deepest practical interest, to determine whether its action is modified in any way by the age of the patient, and particularly so, when it is recollected that it is given by too many physicians, even more freely, and may I not add indiscriminately, to the young subject than to the adult.

The first and most striking peculiarity attending the action of mercury, is that in young subjects, it does not produce salivation so readily as it does in adults. Indeed under a certain age, it appears to be exceedingly difficult to excite salivation at all in them. On this point, besides our own experience, we have abundance of testimony. Dr. Clark says "under various circumstances he has prescribed mercury, in very large quantities, and in a great number of cases; and he never produced salivation, except in three instances, in any child under three years of age."† Dr. Warren, of Boston, observes, "that he has never known an infant to be salivated, notwithstanding he has given in some cases,

large quantities with this view."* Mr. Colles, of Dublin, says, "no man in the present day requires to be told that mercury never does produce pyralism, or swelling and ulceration of the gums in infants."‡ Drs. Evanson and Maunsell speak still more strongly. They say, "mercury does not seem capable of salivating an infant. We have never seen it do so, nor are we aware of any such case being on record." "We have never succeeded in salivating a child under three years of age."†

The same general fact seems to be applicable to the external use of mercury. Dr. Percival, of Manchester, remarks, that he "repeatedly observed that very large quantities of the Unguentum Cæruleum may be used in infancy and childhood, without affecting the gums, notwithstanding the predisposition to a flux of saliva, at a period of life incident to dentition"§

That salivation does not take place so readily in the infant as in the adult, would seem then to be well established. That it never can or does take place, as might be inferred from some of the preceding quotations, is by no means, however, true; and the statement, if implicitly relied on, is calculated to be the cause of much mischief. That very young subjects do sometimes become salivated, is unquestionable. One case, and only one, however, has occurred in my experience, in which a child of two years of age was salivated, and that by a very moderate quantity of calomel, viz., five grains, given in three portions, at intervals, within the space of about twelve hours. In about two days after, the gums became inflamed, the tongue swelled, several ulcers appeared in the mouth, and the flow of saliva was free; after continuing about three days in the same state, it gradually yielded, and disappeared without any further inconvenience. In this case every thing seemed favourable to the development of mercurial action. The child had been labouring under whooping-cough for several weeks, and was a good deal reduced. It vomited freely with every paroxysm of coughing, and this no doubt aided in bringing on salivation, in a constitution peculiarly sensitive and evidently serofulous. Nor is this a solitary case. Dr. Clarke, already quoted, admits that in three cases salivation was produced in children under three years of age. And similar cases have been observed by others. Dr. Blackall relates the case of a child, two years of age, who was salivated in consequence of taking two grains of calomel for several successive nights. The child was a poor serofulous subject, and it sunk under the effects of the mercury.

This, then, is a remarkable peculiarity in the action of this agent upon the infant subject, and the observation of it has doubtless led to the belief, too prevalent among some physicians, that it may be given to them to almost any extent with perfect impunity; an error, which, if not in its immediate, yet certainly in its remote effects, has been the prolific source of more mischief, probably, than any of us are aware of.

Although mercury so seldom salivates infants, yet, notwithstanding this, it cannot be doubted that it affects the system profoundly, and even more so proportionally than it does the adult. That it should do so appears perfectly natural; when we reflect upon the mode of its operation on the human system. On this subject, I am aware that a great difference of opinion exists. By some, mercury is looked upon as a stimulant; while others view it as a sedative. A familiar acquaintance with its effects, however, will show, I think, that it may be the one or the other, according to circumstances—according to the dose in which it is given—the length of time it is continued, and more especially, the condition of the system at the time of using it. A single large dose of calomel will cause nausea and relaxation, and sometimes unpleasant prostration, while if it be given in smaller doses and repeated frequently, it will occasion irritation of the intestines, and general disturbance of the vascular and nervous systems. In the former case acting as a profound sedative, and in the latter as a stimulant, or rather irritant. That calomel given in large doses operates as a sedative, seems to be proved, not merely by the

* View of the Mercurial Practice in Febrile Diseases. By John Warren, M. D., p. 146.

† Practical Observations on the Venereal Disease and on the use of Mercury. By Abraham Colles, M. D., p. 171. American Edition.

‡ Treatise on the Management and Diseases of Children, p. 88.

§ Essays, Medical and Philosophical. By Thomas Percival, M. D., vol. 2. p. 318.

* New York Journal of Medicine and the Collateral Sciences. Vol. 2, p. 1. Vol. 7, p. 153.

† Commentaries on some of the more important diseases of Children, By John Clarke, M. D., p. 182.

nausea and prostration which it frequently produces, but by other considerations. In dysentery, for example, in the adult, a dose of 20 grains of calomel will sometimes allay pain and irritation, with as much certainty as a dose of opium. For the purpose of testing the effects of calomel, some interesting experiments were made by Mr. Annesley, which would seem still further to show, that in large doses the action of this agent upon the mucous membrane of the stomach and intestines, is that of a sedative. He took three healthy dogs, and gave to one, ʒj. of calomel, to a second, ʒij., to a third, ʒiij. After this they were tied up in a room.

"The dog which took ʒj., did not appear to feel any kind of sickness, till six or seven hours afterwards, when he vomited a little. He was lively the whole time, and ate his food well; had been purged two or three times; dejections of a black gray colour.

The dog which took ʒij., was likewise lively, and ate his food well, vomited two or three times, and was purged more than the other; he passed tape worms, and the dejections were black.

The dog which took ʒiij., was heavy, and apparently uncomfortable the whole day, and did not vomit at all; he was purged, and passed a very long tape worm; dejections also black."

Twenty-four hours after they had taken the calomel, the dogs were all hung, and in five minutes after they were dead, they were examined, and the vascularity of the stomach was found to be in the inverse ratio of the calomel they had taken; *i. e.*, in the dog which had taken ʒiij., the vascularity was the least, and so on. For the purpose of comparing this with the condition of the stomach of a dog which had taken no calomel at all, an examination of another dog was made; and here the stomach was found to be more vascular than in any of the others. From these experiments, Mr. Annesley drew the conclusion, that "the natural and healthy state of the stomach and intestinal canal is that of high vascularity, and that the operation of calomel in large doses, is directly the reverse of inflammatory."

The foregoing considerations would seem to show that calomel in full doses is a local sedative, and in its general effects, is debilitating to the system at large. Hence its great utility and value as a remedy in many inflammatory diseases.

When, on the other hand, it is given in small and repeated doses, it acts not infrequently as a local, as well as a general irritant, producing immoderate action of the bowels, and general irritation of the nervous and vascular systems. Now, these, we know, are the effects observed continually in the adult, and it is but reasonable to suppose that all of them, must, as a matter of course, be aggravated in the more delicate and sensitive system of the infant.

What shows incontestibly that the action of mercury is actually more energetic on the infant than the adult, is the fact, that when salivation does take place in the former, as it sometimes does, its effects are more disastrous. Sloughing of the gums and cheeks, general prostration and death are by no means uncommon occurrences. On this subject, Dr. Blackall justly remarks, "a general opinion prevails, that the constitution of young subjects resist mercury. Its entrance into the system they certainly do resist, more than we could expect; but they are greatly overcome by salivations, and the possible occurrence of such accidents may well set us constantly on our guard."† Dr. Ryan, too, says, "Ptyalism of infants is often followed by sloughing of the gums and cheeks; and this I have known to occur after the use of it in Hydrocephalus."

Besides being more energetic in its action on the infant, mercury is also more uncertain. This must necessarily be the case, and for the same reasons that every other active agent is so. In the adult we know that mercury varies in its effects, according to the condition of the system, and the peculiarities of the patient's constitution. Thus some persons are salivated by the smallest quantity of this metal, while others resist the influence even of the largest quantities. In some, febrile action; in others, diarrhoea and exhaustion take place, even from moderate doses. Hence it

is, that every prudent physician, if unacquainted with the previous history of his patient, makes it a special subject of inquiry to ascertain whether he has ever taken mercury previously, and how it affects him. Now, in the young infant, of course, as we cannot so well have the benefit of this information, mere uncertainty must necessarily attend its operation.

These, then, are the peculiarities attending the operation of mercury on young subjects, viz: that they are salivated with great difficulty, and that notwithstanding this, the effects of it are frequently more energetic and uncertain, than they are in the adult. And it is upon these as the basis, that I propose to make a few remarks bearing upon the practical application of it in young subjects.

1. If salivation occurs so rarely in children under a certain age, then it is evident that it can never be made a criterion by which to judge of its influence on their systems. To attempt, therefore, to produce this effect, as we do in adults, is manifestly improper. In cases where it is desirable to get the system under the full influence of the remedy, other modes must be resorted to for the purpose of judging to what extent the use of the article should be carried. Now this is by no means easy. Even in adults, where we have the benefit of salivation as a test, all practical physicians are aware how difficult it is, frequently, to decide when it is proper to stop the use of the remedy. How much more so must this difficulty be increased in the young infant, where we are left without this guide. The only modes of judging, of course, are the character of the evacuations from the bowels, and the general impression made upon disease for which it is administered. Both these are evidently, however, uncertain. It is to be feared, therefore, that for the want of a more certain guide than we at present possess, the use of this remedy is, in many cases, unnecessarily protracted to the great detriment of the little patient. From all this the conclusion is obvious, that in the use of this article in the young subject much greater caution is necessary than in the adult.

2. The fact that mercury may prostrate and destroy a young child, even though it does not cause salivation, it is to be feared is not sufficiently appreciated, at least by some. We have known calomel given without weight or measure, to a young child, and the reason assigned to justify it was, that it could do no harm, because it would not salivate. Now it appears to me that no opinion can be more unfounded, and no practice more mischievous. Although a single dose of calomel, even though large, may be well borne by children of ordinary strength of constitution, yet even this is not entirely safe in all cases. And when these doses are frequently repeated, particularly in delicate habits, the most serious consequences may result.

3. The use of mercury in young subjects as an alternative, should in all cases be conducted with great caution. There is no practice more common than that of continuing the use of this agent in small doses, for a considerable time, and certainly none which is more liable to abuse. Under the idea that the dose is so small and from no salivation appearing, we are apt to infer that even if the medicine is not doing any good, it is certainly not doing any harm. Any improvement, too, which occurs during the use of the article, is sure to be attributed to the silent operation of it on the system. Now, although this is not infrequently the case, yet it is not invariably so; and every observing physician must have been aware of cases, in which, in this way, the article has been unnecessarily and injuriously continued. In bowel complaints, under the idea of altering the secretions, it has frequently, no doubt, helped to keep up the very intestinal irritation which it was given to correct. In other cases it has developed the latent tendency to other diseases, such as Scrofula, Phthisis, Pulmonalis, etc. In adults we know this to be very often the case. How much more likely is all this to happen in the young infant.

4. In the use of mercury in young children, great care should be exercised in ascertaining, as far as possible, their constitutional peculiarities. This, of course, is not in all cases easily to be done. A good deal, however, may be learned from an acquaintance with the tendencies of the parents. Wherever the parents show indication of scrofula, or where there is an hereditary predisposition to consumption, great caution ought to be exercised in the use of mercury in their offspring.

5. Mercury should be administered with great caution, in cases where a child has been sick for a considerable length of time, and when the strength of the child has been very much reduced. In this state of constitutional depression, a single cathartic dose of

* Transactions of the Medical and Physical Society of Calcutta, vol. 1, p. 211.

† Observations on the Nature and Cure of Dropsies. By John Blackall, M. D., p. 126.

‡ Manual of Midwifery. By Michael Ryan, M. D., p. 477.

calomel sometimes proves fatal. We think we have seen more than one case, which has been irretrievably prostrated under these circumstances, under the false impression that calomel is an innocent purgative to a child.

6. The too common practice of giving calomel as an ordinary purge, on all occasions, is certainly unjustifiable. From the facility with which it may be given, it is unquestionably resorted to in a great number of cases, where it is certainly unnecessary, and in a great number where it positively does harm. The misfortune is, that its use is not limited to an occasional dose, but it is too often given in every slight indisposition of the child. Now, in this way, there can be no question that the use of it has laid the foundation for the ruin of the constitutions of thousands. It ought to be a rule laid down and rigidly followed, that in very young children, mercury ought never to be used as a cathartic, unless there is a special reason for resorting to it. In a great majority of cases, milder cathartics are decidedly to be preferred.

In concluding these observations, I trust, it may not be supposed, that my intention has been to undervalue the importance of mercury as a remedy in the diseases of children. On the contrary, no one appreciates it more highly than myself. In many cases, nothing can supply its place, and its judicious use has been, and is, the instrument of saving multitudes of lives. Notwithstanding, however, the many cautions to the contrary, it is to be feared that its use is still too general and indiscriminate. Indeed, the amount of it which is taken by the human race in one way or other, is incalculable. What is given by regular physicians, is perhaps the smallest quantity. If the public really knew how much of this article is swallowed unknown to themselves, in the shape of bilious pills, worm lozenges, and the white powders of the Homœopaths, they would be amazed at their credulity in deserting their old medical advisers, because they have the boldness to give them an occasional dose, and the honesty to tell them so.—*N. Y. Analyst.*

DIAGNOSIS OF NEURALGIA AND NEURITIS.

Although in some cases the symptoms of these two affections of the nerve are so nearly similar, that it is difficult to distinguish at first sight the one from the other, the confusion will cease in general, if, instead of inquiring into the actual condition of the patient, our inquiries are directed to the prior history of the attack, its progress, and exciting cause. While, in fact, *neuralgia* is a very common affection, arising without appreciable cause, or from causes the most opposite in character, *neuritis* is a rare affection, and is determined by causes which are readily appreciated. In analyzing the best authenticated cases of neuritis, it will be found that, with the exception of some few cases, in which it followed parturition, neuritis has almost constantly been produced by physical lesions of the nerve,—such as wounds, punctures, contusions, ligature, compression by a tumour, &c.; in fact, neuritis is always, or nearly always, the result of mechanical injury, while neuralgia originates spontaneously, and depends upon a particular, and little understood, condition of the economy. But if it is sometimes possible and useful to establish this distinction in practice, especially in neuralgia and neuritis of recent date, it cannot be denied, that in a certain number of cases of chronic neuritis, the distinction becomes impossible; for although it has been ascertained that neuralgia of very old standing (thirty or forty years for example), may have preserved its original character throughout, and yet left no traces of disease after death, it happens in the majority of cases, that under the influence of the repetition of the paroxysms, the texture of the nerve eventually becomes altered to such a degree, as to render it quite impossible to decide whether the inflammation has been secondary, or has depended upon an original neuritis. These cases shew the inutility of attempting a diagnosis in the chronic forms of the affections.—*Gazette Médicale de Paris, No. 40, 1846.*

THE URINE IN ASCITES.

In ascites, dependent on lesion of the liver, the urine is always more or less deeply coloured; whilst in renal ascites, (Bright's disease or otherwise), the urine is white and colourless—(Rayer.) This characteristic condition of urine in ascites was perfectly known to the Arabian physicians.—*Monthly Journal of Medical Science, December, 1846.*

SURGERY.

TREATMENT OF SPRAINS.

The means which Dr. Poullain and some other authorities recommended in lieu of leeching and cataplasms is the *immediate and continued application of cold by immersing the part in water.* The cure is not only prompt but complete, inasmuch as there is no remaining *engorgement* to lay the foundation of future mischief, and the patient is enabled to employ the joint as actively as heretofore. This would be a great point gained, even if the time consumed in the treatment were as great in the one plan as the other, which it is not. Many cases of its success are related in the paper, and although, of course, in the great majority of instances, the ankle is the joint affected, sprains of other joints may be treated in just the same manner, except that in those, such as the knee, in which immersion may be difficult, the application of wet compresses or irrigation may be sustained. The treatment, indeed, is not novel, for it was recommended by Boyer, and more recently by M. Begin.

"Of 90 patients whom I have treated by the aid of cold and resolvents, 23 were cured in 6 days, 10 in 8 days, 22 in 11 days, 28 in from 11 to 15 days, 4 in 20 to 25 days, and 3 at the end of a month. None of these persons have continued lame. Seven felt the effects of their accident for several months, without, however, being prevented attending to their duties, and becoming quite cured. * * * * * If this mode of treatment has incurred blame at the hands of some surgeons, it is because it has not been sufficiently, promptly, and freely employed, and it is therefore necessary to lay down some rules upon this point.

"The immersion should be resorted to as soon after the accident as possible. Recourse may be had to it also three, four, five, six, or even 12 hours after, but then its sedative effect is less prompt and the cure more tedious. The foot should remain at least *two hours* in the bath, and oftentimes much longer. It may sometimes be left in for entire days; and as a general rule the part should not be removed until it becomes completely cooled, the water being renewed as often as it becomes warm. This prolongation is easily obtained, for, after the first hour or so (during which the pain is sometimes almost insupportable) the immersion becomes bearable, and the patient is himself very desirous for its continuance. Iced water does not possess any superior efficacy to that of a temperature of 37° or 39°, provided this be equally maintained. As soon as the limb is removed from the bath it must be surrounded by a roller previously moistened with Goulard water or camphorated spirits, some of which must afterwards be applied to it from time to time. So effectually are congestion and swelling in this way diminished, that the bandage usually becomes loose within 24 hours. It must be re-applied until all swelling and pain have disappeared, which is generally the case in from three to six days. The patient may now be allowed to walk, continuing however the use of a bandage for ten or twelve days.

"If 14, or even six or twelve, hours after the application of the wet bandage, pain still continues, or throbbing is felt by the patient it must be taken off, and the limb again immersed in the water for a longer period than at first, even for a whole day if requisite. This second immersion is sometimes unsuccessful, but fortunately it is very rarely required, as the first almost always suffices.

"If the sprain is several days old, the limb swollen and painful, while nothing has been done for it, a free local bleeding is a necessary preliminary, after which the bandage and cold lotions, or perhaps immersion itself, should at once be resorted to. These means are, however, now of far less service than when employed soon after the occurrence of the accident."

When the sprain has been badly treated the joint may become the seat of a chronic enlargement, which is dissipated with difficulty and only after the persevering use of compression. MM. Begin and Velpeau, indeed, employ this in the earliest stage of sprain as a powerful means of preventing inflammatory swelling. Dr. Poullain employs to this end a starched many-tailed bandage. Whatever means are used, the case is tedious and may also require the aid of stimulating liniments, or, if very obstinate, of the *douche* as employed at the mineral springs, and even this does not always dissipate the enlargement.—*Rev. of Poullain, in the Brit. & F. Rev., July, 1846.*

TREATMENT OF SLOUGHING ULCERS.

In a great majority of cases, the sloughing process had advanced far before the patients were received; in some, as formerly stated, bones were denuded, and tendons and ligaments destroyed. But in very few, even where the destruction was greatest, was treatment by incision omitted; and in those only where, from fever or flux, there was great constitutional debility. The amount of incision was regulated by the extent of disease in the tissues under and around the ulcer. In some instances, where it did not descend below the integuments, the ulcerative process being phagedemic rather than gangrenous, and the destruction neither very rapid, nor reaching under tissues, it was sufficient to relieve the more superficial vessels, and to substitute scarification for what is understood by incision. More frequently, however, it was necessary to use the knife freely, passing it through the skin, and into the underlying cellular structure. Whatever the proper depth might be, the scalpel was carried quickly from beyond the limit of surrounding disease to the ulcer, often through it. The distance between the incisions varied, but was generally less than a quarter of an inch; their direction was most frequently parallel in the line of the limb, occasionally radiated from a circle, clear of the affected integuments, to the ulcerated centre, according to the position of parts, and degree of vascular action. In many cases, it was necessary to repeat the practice; in some, frequently.

The effects of this mode of treatment were generally prompt and most satisfactory. The relief to pain and irritation was often immediate; and, although the remedy was a painful one, patients sought rather than shunned its repetition, upon the recurrence of bad symptoms; so unequivocal was the benefit derived from it.

Instead of sanious fetid discharges from the ulcer—its ashy, livid, or black surface, and abrupt margin—there was secretion of pus, separation of sloughing matter, and a crop of florid healthy granulations; the surrounding parts, which had been tumid, darkly inflamed, or œdematous, or having both conditions combined, became flaccid and shrunken, assuming the pale complexion of health. In no instance did the sloughing action extend to the incised surfaces, which either healed speedily by adhesion, or more slowly, but not less surely, by granulation.—*J. Wilson. Med. Notes on China.*

TREATMENT OF FRACTURE.

Sir B. Brodie gives no sanction to the operation of cutting down on the broken ends of the bones, and sawing off a portion of each of them, for the purpose of procuring union, and supposes that no modern surgeon, having a moderate share of prudence, would undertake it. In reference to the introduction of the seton in these cases, he remarks, that the result of the practice in this country appears to be, that sometimes it is successful in the upper extremities, but that where it has been performed on the lower extremities, as far as he knows, it has only succeeded in a single instance. The operation is uncertain and the result tedious. Sir B. Brodie speaks favorably of the treatment by pressure proposed by Mr. Amesbury, and states that it succeeded perfectly in three cases which were attended with him. This success, in one of these at least, was not so complete as is represented, since it appears that there was so much yielding motion between the upper and lower portions of the fractured femur, "that it was plain that the union could be merely ligamentous." In this mode of treatment the pressure must be considerable, so as to cause some inconvenience to the patient, both from pain and from swelling of the limb below. But the inconvenience is only temporary.

"The principle of Mr. Amesbury's practice is simply that of keeping the ends of the bones in perfect repose, and at the same time applying pressure, particularly on the broken surfaces, so as to keep them in the closest possible contact with each other. Of course no general rule can be laid down as to the mode of attaining this object. In a case of transverse fracture, one kind of apparatus must be employed, in one of oblique fracture another, and in one of comminuted fracture a third. The apparatus will also differ accordingly as it is a fracture of the arm, the fore-arm, the leg, or the thigh. In a case of oblique fracture a very simple apparatus will do all that is required. Secure the limb by fastening it to a single rather broad wooden splint. Apply a pad of thick leather on each side of the fracture, and then a tourniquet, by which the two opposite surfaces of bone may be kept firmly squeezed against each other. By means of the tourniquet the pressure may easily be regulated, and increased or diminished as the

patient can bear it. The best kind of tourniquet is not the common one, known under the name of Petit's, but one which occupies a smaller space, invented by the late Mr. Savigny, and sold by Philip and Whicker in St James' street.

"I do not say, however, that this method always succeeds. I have tried in the case of the little boy whose case I have already mentioned (on whose leg I afterwards used the seton), and without advantage. There was another patient in this hospital on whom it was tried for a considerable time under Mr. Amesbury's observation, and no union was effected; and it appears that Mr. Amesbury met with some cases in his own private practice, in which he has adopted it, and no doubt done ample justice to it, but in which it has failed. Still it has proved a very successful method on the whole, and certainly very much more successful than any other."—*Sir B. Brodie.*

INHALATION OF SULPHURIC ETHER VAPOUR.

Proceedings of the Surgical Society of Ireland.

Dr. Macdonnell said he proposed to bring under the notice of the society this evening a case illustrative of (what, no doubt, every one in the room had heard,) the use of inhalation of sulphuric ether vapour, as a means of producing insensibility under surgical operations. He supposed that at all events many gentlemen present had seen what had been published by him in the *Medical Press* regarding this case; he was, therefore, unwilling to detain the society with a lengthened statement of it now, his principal object at present being to elicit from the society an expression of their opinions respecting the cases to which this wonderful agent may be considered applicable—namely, the cases in which its use might be dangerous or improper, and the cases in which it is only to be resorted to occasionally. Lastly, and not least, he expected that there would proceed from this society—as the proper place for such a purpose—an expression of opinion, pronounced against the incautious use of this agent, either on the part of persons actually ignorant or of those but imperfectly acquainted with its properties. By an incautious use of it a serious injury to this agent would arise as well in public estimation as in that of the profession, and it was a power, he believed, that would prove of immense benefit to suffering humanity. Great discoveries have often been known to suffer materially from an improper application. On these grounds, therefore, he thought it right that the incautious use of the agent under consideration should be protested against, and the earlier the better. He would now state in a very few words the heads of the case, and the principal points that occurred both at the time of the operation and since.

A young countrywoman received a wound over the elbow-joint six or seven weeks ago, and after a fortnight (during which the case was greatly mismanaged) she came under Dr. Macdonnell's care. On examining the wound he had no doubt that the joint was involved, there being profuse discharge, ulceration, and a sinus through which a probe could be passed into the joint. Almost all the cartilage of the latter was found to have disappeared at the time of the operation; a portion of that which covered the cup of the radius and the olecranon process alone remained. Caries had extended through the whole of the rest of the joint, and separated a portion of the inferior extremity of the humerus from the shaft.

In spite of every endeavour to arrest the progress of the disease, hectic very soon set in, together with diarrhœa and bed-sores, the latter having been caused by the patient's inability to move from the dorsal position, the slightest attempt at motion being productive of excessive pain. Under this rapid sinking, it was unanimously decided to operate with the least possible delay. The use of the ethereal vapour had just been heard of in Dublin, so it was determined to try it here. Owing to slight imperfection in the apparatus there was at first some delay in bringing the patient under the influence of the vapour;

but on a second trial, complete insensibility was produced in from a minute and a half to a minute and three-fourths, and from this time we have the patient's positive assurance that she remained perfectly insensible up to a point of time, which, as nearly as might be, amounted to three minutes after the operation; that all this time, in fact, she felt nothing. One particular observation made by Dr. Hutton, and mentioned in the communication to the *Medical Press*, was the complete power possessed in managing the stump; it could be moved about in every direction while securing the arteries, which were in unusual number in this case. It was also remarked by Dr. Hutton that there was rather more blood than usual: as regards this observation, however, Dr. Macdonnell was not sure that much importance is to be attached to it, but under circumstances where every point is deserving of accurate note, it was worth recording. The patient suffered unusually little from aching of the wound, but complained slightly of it after her removal to bed. The notes of the pulse were published from Mr. Tyfnell's observations. Before the operation her pulse usually numbered 120 or 130, but it has since returned very near the natural standard; the pulse being now 100, or very little above it. He (Dr. Macdonnell) had never seen any case turn out more completely favourable; he now looked on the patient as perfectly safe; the only circumstance out of the ordinary course that had occurred was this—on the sixth day after the operation (Thursday) two ligatures fell, and one yesterday, the single one he believed was from the humeral artery; three ligatures still remain.

As his object was (Dr. Macdonnell said) to elicit opinions, he would trespass very little longer on the time of the society, but would merely state a few of the cases in which it occurred to him the employment of the ether would prove highly serviceable. In cases of tetanus and hydrophobia, if inhaled just before the approach of the fit, it might, he thought, turn out to be extremely useful. Again, where operations have to be performed on subjects in whom there is a marked deficiency of moral courage—a perfect effeminacy in fact—he had often observed even slight operations prove fatal under such circumstances. In one such instance he had known the man express his conviction, several days before the operation of amputation, that he would not recover, and in three days after he died without anything having occurred to account for the man's death except the shock upon the nervous system. Many of the gentlemen present had, no doubt, experienced similar results in persons of that highly impressible nature. In many cases of compound dislocation and fracture, it would also, he was sure, be of much service, as a means of allaying the strong spasmodic action of the muscles which so greatly interferes with reduction of the former and coaptation of the latter. How very fortunate, he would say, might not a knowledge of such a remedy have been on an occasion of late occurrence; he alluded to the case of dislocation of the hip which had excited so much sympathy amongst the profession and the public. In conclusion, Dr. Macdonnell suggested the necessity of using the ether with great caution in all cases where disease of the brain is at all suspected, or any apoplectic tendency. He again begged to remark that his great object was to ascertain the opinions of the society generally on this important subject. One or more of the gentlemen present would perhaps be willing to be experimented upon.

Mr. Ellis begged to ask a question or two respecting a couple of points which he supposed Dr. Macdonnell had unintentionally omitted to mention. First, with respect to the effects produced by the vapour; whether Dr. Macdonnell had observed anything like collapse before or after the operation performed by him; secondly, as to the effects experienced by Dr. Macdonnell himself, for having seen that gentleman's interesting letter in the *Press*, he (Mr. E.) was

aware of Dr. Macdonnell's having himself inhaled the vapour.

Dr. Macdonnell replied, that in the communication alluded to by Mr. Ellis, he had stated all he knew on both the points referred to by him; and being under the impression that almost all present were aware of the contents of that communication, he had purposely avoided occupying the attention of the society with a repetition. The first unusual sensation experienced by Dr. Macdonnell on the inhalation of the vapour, was a disagreeable feeling of heat referred to the centre of the chest—that is, to the larger bronchial tubes, together with a sensation of slight obstruction in them, accompanied with cough. These symptoms disappeared on the approach of insensibility, and the breathing became apparently easy; then for a few seconds a low humming sound was felt in the head, and in a few seconds more a total deprivation of sense ensued, as if from receiving a heavy blow on the head, but without the sensation of a blow being inflicted. Both in Dr. Macdonnell himself, and in every person on whom he operated, there was dilatation of the pupils, and it had been observed by his friend, Mr. Macdonnell, who assisted him, that in the beginning his pulse rose slightly, but fell again as insensibility became established, and was rather stronger than usual, his friend thought. It was observed by Mr. Tufnell, too, that the pulse varied very much at different periods of the operation subsequent to the production of insensibility. The only signs of collapse observed in Dr. Macdonnell himself, or in others, was a total loss of muscular power, complete inability of motion in the limbs. He believed the effects of the agent as just described were exactly in accordance with what occurred in a case in which Mr. Collis had on Wednesday last removed a toe-nail from a patient at the Meath Hospital, and in which the application of the ether was perfectly successful. The effects of the vapour have, Dr. Macdonnell thought, no similarity whatever to those of intoxication; he certainly felt nothing of the kind himself, neither did any one else, as far as he was aware. Immediately on recovering from the state of insensibility, too, which was in five or six seconds from the time of its being complete, he (Dr. Macdonnell) looked about and saw distinctly two objects on a table at a distance; there was, therefore, nothing of double vision or other sign of intoxication.

Mr. Tufnell next related the particulars of four cases in which he had tested the effects of the ethereal vapour. The first was that of a stout healthy dragoon, aged 25, who had an abscess, or, as it is commonly called, a gumboil, caused by a carious slump, in the removal of which considerable additional pain would of course be suffered in consequence of the abscess.

The vapour was inhaled gradually and slowly, and as soon as the brain began to be affected by it, the man showed signs of distress and dyspnoea. Owing to deficiency in the apparatus, the atmospheric air was not entirely excluded, so that at the end of six minutes insensibility was not complete; there was, however, loss of muscular power and other evidences of the action of the vapour, and this being the first experiment, Mr. Tufnell was unwilling to push it further. The tooth, however, was extracted without any manifestation of suffering, further than a slight effort on the part of the man, but he did not touch the hand of the operator. He soon after stood up and looked about vacantly. The condition of the pulse was not noted in this case, but the pupils were observed to be very slightly altered.

The subject of the second experiment was a man of a weakly leucophlegmatic temperament. Before a minute had elapsed there was slight spasm of the respiratory muscles and short cough, which, in a few seconds more, increased to such a degree that the points of the fingers shook from the violence of the inspiratory efforts. The head dropped on one side of the neck and the eyelids fell. In three

minutes this man was so completely insensible that a person entering the room would have supposed he was in a fainting fit, until the pulse were felt, which had risen three degrees above what they had been when the man was first seated for the purpose of experimenting. The respiration was normal too. He remained thus without any symptom of returning vigour for three minutes, and was now brought round by the application of a wet towel which caused evaporation. During this man's insensibility Mr. Tufnell took occasion to pinch him violently with a pair of forceps through his trowsers, and even ran the point of a pair of scissors several times into him, but failed to produce the slightest symptom of sensation.

The man first operated on was now summoned again, and the completeness of the experiment ensured by the substitution of a leather mouth-pipe through which the man could not possibly inspire unimpregnated atmospheric air. In two minutes and a half he was perfectly insensible, there being at first considerable dyspnœa, during which he made several efforts to raise his hand to the mouth-piece as if to remove it. He now appeared to lose all control over himself, snatched the instrument with which Mr. Tufnell was about to draw a second unsound tooth that lay next the former one. He rushed about the room, swearing and kicking violently; the tooth, however, by a little management was removed, and in two or three minutes the man recovered his usual condition, perfectly unconscious of the removal of the tooth. On questioning him as to his sensations while under the influence of the vapour, he stated that he fancied himself seated in a room with four persons whom he had never seen before, and altogether he appeared to have felt agreeably. The veins of his neck were observed to be turgid and distended.

The subject of the fourth experiment was another stout healthy dragoon, very short necked, but as he was apparently a temperate man, Mr. Tufnell saw nothing to contra-indicate a trial of the vapour upon him. He coughed violently till under the influence of the agent, the effects of which are quite perfect at the expiration of two and a half minutes. The head, as in the second case, fell on one side, and there was slight lividity of the countenance, which Mr. Tufnell said he had not observed in the others. This man immediately after became very violent also, he jumped up from the chair, declaring that nobody should take him to the guard-room, and kicked about, just as a man would do if drunk. After wandering about the room for a little time, he gradually recovered in two minutes.

In all these cases the countenance wore a partially idiotic expression during the transaction from insensibility to complete recovery, and this man, like the last, was under the impression that he was amongst numbers; he fancied himself dancing in a public-house with many others. The latter sensation, Mr. Tufnell suggested, might be conveyed to the brain as the last impression which the person, just becoming completely insensible, was capable of receiving from seeing a number of persons about him. This point, however, as also the fact of the outward expression of pain, while the subjects of the experiment declared their sensations to be extremely pleasurable, Mr. Tufnell found some difficulty in reconciling to himself. Mr. Tufnell was not able to note the character of the pulse in any case but one.

Dr. Macdonnell would be glad to have Dr. Jacob's opinion respecting the applicability of this agent in some of the delicate operations on the eye. It struck him (Dr. Macdonnell) that in many of these the vapour might be employed with great advantage.

Dr. Jacob said that he felt some doubt as to the applicability of the plan in operations of the eye generally. In that which he was most frequently called on to perform, there was really little or no pain unless the iris was touched or pressed upon, and he feared that the insensibility and

consequent loss of support to the head might cause embarrassment. In the operation for extraction, he would have much dread in placing the patient under its influence, lest in any violent efforts which might be made by a person in a semi-conscious condition, the vitreous humour might be expelled. In operations for artificial pupil it might most probably be resorted to with benefit, and in operations for strabismus, more than all others, he thought its application might be found useful. In extirpating the globe on account of malignant disease, or removing orbital tumours, it might also be resorted to with great advantage and relief from excruciating agony.

Mr. L'Estrange might be permitted, he hoped, to add his testimony respecting the properties of the agent under consideration. He had seen the operation at the Meath Hospital referred to by Dr. Macdonnell, in which the matrix of the toe-nail was excised, and in this instance there was complete absence of suffering, together with collapse of the muscular energy already alluded to. The next case was that of a lady brought to his (Mr. L'Estrange's) house for the purpose of having a tooth extracted that she had been for a long time trying to summon courage enough to submit to. It was at first found very difficult to put her under the influence of the vapour, and upon a second trial it was only after eight minutes that its effects were manifest. Her condition then was not that of collapse, but rather resembled the state alluded to by Mr. Tufnell in one of his cases. She was conscious of what was going forward; at one period she heaved twice heavily, and it was just then that the tooth was removed without the lady having suffered the slightest pain, so much so that she insisted on having a second tooth extracted. She described her sensations to have been as if travelling on a railroad.

Dr. Hargrave inquired whether Dr. Macdonnell had noticed any alteration in the character of the blood that flowed from the stump in his case?

Dr. Macdonnell replied, that there was certainly no remarkable difference from what is usually observed. The arteries at least could readily be distinguished by the colour of their blood, though it could hardly be said that the blood flowing from them was of so bright a scarlet as is usual.

Dr. Hargrave continued to say, that as cases in which this agent ought not to be tried, he would suggest instances where persons had suffered from severe accidents, and had just recovered from the state of collapse consequent on such accidents. Here an operation may be immediately necessary, and the propriety of employing the vapour under such circumstances, was, he thought, very questionable. He might take this opportunity of mentioning the case of a young girl of sixteen or eighteen, who came under his observation some years ago, and on whom ether had a very remarkable effect. After an illness of some length, she was seized with spasmodic attacks of a violent tetanic character for which there was no assignable cause. Before the tetanic spasm set in, she suffered from the most violent spasm of the larynx, almost threatening suffocation. Dr. Cheyne saw this case, and ordered a caustic issue on the vertex of the head, and to have the patient go to the country. Happening on one occasion, at the approach of a fit, to observe some other by her bedside, I placed a little of it in the palm of my hand, which she inhaled, with the immediate effect of arresting the laryngeal spasm, and the tetanic spasm. For a week or ten days she used this remedy with much benefit, and then went to the country, where, after a lapse of six or seven months, she completely recovered, but has ever since been subject to occasional attacks of a peculiar nervous affection. Now, with respect to the applicability of the ether in military surgery, Mr. Tufnell might afford the society some satisfactory information. He (Dr. Hargrave) believed that the necessity of taking advantage of recovery from the shock or collapse attendant

on severe wounds, was particularly dwelt on by most writers on military surgery. In such cases, then, as also where cardiac disease exists, and in very old persons, he was inclined to think the inhalation of the vapour might be attended with unpleasant effects. At all events, before inhalation were resorted to for the purpose of an operation, he would say that the effect of the vapour on the individual ought to be carefully ascertained. Mr. Orr would detail to the society a case in which he that morning made a preliminary trial of the vapour, and which was attended with circumstances that completely marred the efficacy of the remedy in his hands.

Mr. Orr said, that being anxious for an occasion to test the properties of this new agent, he took advantage of an opportunity afforded him in the person of a patient affected with lipoma of the testicle, on whom he wanted to perform Mr. Syme's operation. He had purposely avoided saying anything to the patient on the subject until that morning, and he then objected to it, said he felt a disposition to faintness, &c. After a little reasoning, however, he was induced to submit, and when the apparatus was applied, the mouth-piece was found to fit badly from a disproportion between it and the mouth of the patient. But with some management he was forced to inspire the vapour, Dr. Hargrave having his finger on the pulse, while Dr. Williams took notes of it. In a few moments the man started up, and asked in a wild manner what they were about to do with him? After two or three attempts it was found impossible to produce the degree of insensibility ascribed to the vapour, perhaps (Mr. Orr said) on account of its being involuntarily regurgitated by the man, and his partially respiring unimpregnated atmospheric air. At the conclusion, he stated that he had felt as if in an agreeable dream, but was far from being entirely unconscious, for he felt, he said, a disagreeable sensation on being pinched, which was done each time a renewal of the attempt was made. The pulse varied considerably during the experiment; it was exceedingly weak just before the inhalation was commenced, and rose again both in frequency and force when the man appeared most under its influence. He expressed his willingness to submit to another trial of the vapour, but Mr. Orr feared his being rather too much excited just then for a repetition of the experiment.

Dr. H. Kennedy observed that, from the remarks made to-night, it would appear the effects of this agent were not peculiar to it. He had seen very nearly identical effects produced by the nitrous oxide, or laughing gas, which, it was not generally known, perhaps, produce in some constitutions that semi-comatose or apoplectic state described to-night, in which the individual is entirely deprived of the power of motion, but not of all consciousness. He had seen instances of this kind, while in others, which were certainly in the majority, a state of the most violent excitement was induced by the use of the gas. Dr. Macdonnell had properly spoken against the use of the ether in cases marked by any tendency of blood to the head; to these it might be prudent to add, cases where there was anything like disease of the heart, or delicate lungs. The great difference existing between persons in rude health—such as were chiefly the subjects of Mr. Tufnell's experiments—and those likely to have to undergo operations, should not be forgotten either. As a whole, and speaking of surgical cases only, he could not help feeling that the case suited for the use of this agent would prove to be the exception, and not the rule.

Mr. Carmichael said that, notwithstanding the objections urged by previous speakers against the agent under consideration, he looked upon it as a most valuable boon to society at large, offering as it did so powerful a means of alleviating pain. He had closely watched the woman whose case had been detailed by Dr. Macdonnell, and had

observed that soon after commencing the inhalation the woman's countenance fell, at which moment Dr. Macdonnell commenced the operation. While the skin was being divided (which is admitted to be the most painful part of the operation,) the woman's countenance did not betray the slightest evidence of pain; and on being asked, when all was over, whether she had even felt the sawing of the bone, she said she had no sensation of the kind whatever. The objections made to the employment of this agent, Mr. Carmichael said, might with equal justice be urged against opium and the many other means of alleviating pain. He had that day seen in one of the public papers a paragraph attributing the suggestion of this highly valuable aid in surgery to M. Ducros, who, it was stated, had introduced the subject to the notice of the Academy of Sciences at Paris in March last. Now, in a society, and more particularly one so numerous as the present, he (Mr. Carmichael) considered that merit should, if possible, be awarded to the individual possessing the clearest title to it; and as the author of the paragraph alluded to was, perhaps, now present, he might in furtherance of his statement, give to the society additional details on the subject. It had been stated at the same time by M. Ducros, that opium acted as an antidote, or a means of recovering the person from the state of stupor induced by the vapour. This (Mr. Carmichael said) reminded him of the homœopathic doctrine—*Similia similibus curantur*. He did not know whether in protracted operations this remedy would be applicable, or whether its renewal might be attended with safety.

Dr. Power, having been present at Dr. Macdonnell's operation, wished to mention one or two circumstances connected with the patient and the operation that had struck him at the time. It was after a second attempt, as the society were aware, that the woman was brought fully under the influence of the vapour, and then Dr. Power observed great difficulty of respiration, complete alteration in the expression of her countenance, and great turgescence of the vessels. As to the fact of her not having felt pain, he felt perfectly convinced that she did not suffer the slightest; he might as well have held the arm of the dead subject, so little evidence was there of any sensation under the knife. These facts had struck him most forcibly. As to the applicability of the remedy to protracted operations, he could not now pretend to say anything; or as to the amount of improvement of which the agent may yet be susceptible, so as to render it available under such circumstances.

Dr. Eades made some observations tending to establish an identity of effect between the ethereal vapour and protoxide of nitrogen, or laughing gas, and showing that both these agents equally exhibited alternately exciting or depressing influences, according to the variety in constitutions of the individuals experimented on. Ether, he also observed, had been a remedy in use long and long ago in cases of chronic catarrh and other affections of the respiratory system, and the rationale of its exhibition appeared to be quite in accordance with the homœopathic doctrine. The stupefaction produced by the vapour appeared to be clearly referable to its action on the nervous system.

Mr. Jameson mentioned the case of a boy of 17 or 18, under his care in Mercer's Hospital, for some affection of the eyes, on whom he had tried the effect of the vapour two or three times, but with an imperfect apparatus at first, so that stupefaction was not induced; but the boy always declared that he could see better after its use. One or two mornings ago Mr. Jameson so far induced stupefaction as to render the boy entirely insensible to pain, but consciousness was present to such an amount that he answered any question put to him. His pupils were dilated, the conjunctiva considerably suffused, the symptoms being altogether little

short of apoplectic. Mr. Jameson meant to test the properties of this agent more fully in a day or two in a case for amputation; at the same time it was a remedy which ought to be used, he thought, with some caution. While the boy above alluded to was under its influence, there was each time an irresistible fit of crying, the tears ran down the cheeks. He had subsequently no recollection of what had occurred further than that the sensation had been a pleasurable one, and that the hearing was obstructed.

Mr. Cusack said it was by experience alone that we could hope to arrive at any definite results on this interesting subject; he would therefore only detain the society for a moment or two while he added the little that he himself had as yet any opportunity of ascertaining respecting it. A strong healthy peasant girl was brought to Steevens' Hospital, into whose foot a needle had passed some time before through the ball of the great toe along the sole of the foot. Notwithstanding the extensive use of the knife required here, amongst structures, too, so abundantly supplied with nerves, this girl, placed under the influence of the vapour, never winced or expressed the slightest sensation of pain. He might (Mr. Cusack said) as well have cut a piece of board, so complete was the insensibility. Mr. Cusack observed the blood to be of a very dark colour, and the muscles very flaccid; the dark colour of the blood, it was possible, he said, might have been partly owing to his having cut across a vein while incising the muscles.—*Dublin Medical Press.*

OBSERVATIONS ON THE EMPLOYMENT OF COMPRESSION IN ANEURISM.

By O'BRYEN BELLINGHAM, M.D., F.R.C.S.I., one of the Medical Officers of St. Vincent's Hospital.

Advantages of compression over the ligature—Compression effects the cure of aneurism by simpler and safer means than the ligature; this method of treatment more certain and more permanent than that by the operation—Cause of the return of pulsation in an aneurism subsequent to the operation.—Objections to the treatment of aneurism by compression answered—Concluding remarks—Summary.

Notwithstanding that the amount of evidence which has been adduced in favour of the treatment of aneurism by compression is perhaps greater than has ever been brought forward within so short a period in support of a novel method of treatment, the cases in which it has been employed, forming, I may say, one unbroken chain of successful results: yet surgeons who have not seen it used, or who are accustomed to rely solely upon the ligature, may be slow in adopting what they may still look upon as an innovation. I shall therefore now endeavour to point out the advantages which this method of treating aneurism has over the ligature; and by instituting some kind of comparison between them, endeavour to do away with the objections which have or may be urged against compression.

It will, I believe, be admitted, that an aneurism once formed, has a constant tendency to increase in size; that, as it enlarges, the parietes of the sac become thinned (more particularly if its progress has been rapid); and that these effects are due to the distending force of the blood exercised upon the interior of the sac. Now, if the latter can be taken off, the enlargement of the aneurismal sac would necessarily be put a stop to, before it had attained any considerable size; and its parietes would thus be prevented from becoming thinned. The first effect, then, of pressure upon the artery between the aneurism and the heart, is to diminish or take off the distending force of the blood, the tumour is immediately reduced in size, and becomes more or less flaccid; the distension from pressure to which the parts about it had been subjected being thus removed, the pain which many patients labouring under aneurism suffer from in the affected limb is relieved. When the aneurismal sac has thus been kept stationary for some time, although no other change should take place, its parietes will be strengthened rather than thinned; and the danger of rupture of the sac will be greatly diminished. Indeed many of the cases on record, where a circumscribed aneu-

risms became diffused, necessitating amputation of the limb, would probably have been saved by the timely application of compression to the artery between the aneurism and the heart. However, we know that the same measure which takes off the distending force of the blood from the interior of the sac, and which checks its further increase, will at the same time bring about other changes in the sac itself, which will not only prevent it from enlarging, but will, if persevered in, effect the cure of the disease.

That compression effects the cure of aneurism by more simple and safer means than the ligature—that the treatment also is more certain—and that the result is likely to be more permanent than when the ligature is employed—I shall now endeavour to prove.

That compression effects the cure of aneurism by more simple means than the ligature is evidenced by the facts—1st. That the mode in which the consolidation of the aneurism is brought about by compression is exactly the same as that in which a natural or spontaneous cure occurs; and 2dly, because when a cure is effected by compression, the vessel is obliterated merely at the site of the aneurism; whereas when a ligature is applied in the usual situation at some distance from the tumour, the artery is obliterated both at the seat of the ligature and at the seat of the aneurism. Hence it is easy to understand why, when secondary hæmorrhage followed the operation, the application of a second ligature higher up so seldom succeeded; and we can hardly be surprised at gangrene attacking a limb, the main artery of which is obliterated at three different points.

That compression effects the cure of aneurism by safer means than the ligature is also evident, because its employment can be intermitted and resumed according to circumstances; and because no ill consequences have hitherto resulted from its use. On the other hand, the ligature of a large artery is always a precarious operation; when it is once applied, we must await its separation before the patient can be considered out of danger; and when it fails, which frequently happens, the case almost always terminates unfavourably, not from the increase of the disease, but from the operation performed for its relief. The artery in which aneurism (after the aorta) is most frequent, is the popliteal, and the ligature of the femoral artery for popliteal aneurism is more frequently unsuccessful than that of any other artery of equal size. Mr. Benjamin Phillips collected fifty-nine cases from various sources in which this vessel had been tied, in thirty-nine of which it failed; and although (as Mr Storks observes) the accuracy of these statistics may be denied, "yet every surgeon must allow that the deligation of a main artery for aneurism is an operation (notwithstanding the successful results some practitioners can boast of) attended with great risk." On the other hand, I have given a list of twenty-seven cases of aneurism treated by compression of the femoral artery, in twenty-five of which it succeeded perfectly; of the other two, one died of erysipelas before the cure was completed; the other was operated on at the patient's urgent request, and recovered. A mode of treatment therefore which is exempt from all risk has many advantages on the score of humanity, which alone ought to constitute a strong argument in its favour.

The treatment of aneurism by compression is more certain than that by the ligature. We have already seen that the operation by ligature, however carefully performed, is a precarious one, and that it frequently fails; that secondary hæmorrhage from ulceration of the artery at the site of the ligature or phlebitis not unfrequently follow it; or that suppuration of the sac, hæmorrhage from it, or gangrene of the extremity, may ensue. Now, none of these unfortunate results have ever attended the treatment by compression, nor are any of them ever likely to follow it; because, in the first place, no injury whatever is inflicted upon either the artery or vein at the site of the pressure; and secondly, because the aneurismal sac, and the part of the artery from which it springs, are gradually filled up by fibrine, separated from the blood, and deposited in the same way as when nature cures interstitial aneurism.

That a cure effected by compression is more likely to be permanent than when the ligature has been used, and that pulsation cannot return after the employment of compression, as sometimes has happened after the operation, might be inferred from the manner in which the cessation of pulsation is brought about; and it is proved by the pathological facts already adduced. In one instance, where the patient had been under treatment by compression, but died previous to the cessation of pulsation in the

aneurism, the sac was found to be in a great measure filled up by fibrine, deposited in concentric laminae. In another, whether the patient had laboured under popliteal and femoral aneurism in opposite limbs, and died some time after their cure by compression, the subsequent changes were shown; the contents of the sac had been absorbed, and the artery at the seat of the aneurism was converted into an impervious ligamentous band, proving that the cure had been permanent, and that it was physically impossible for an aneurism again to form at the part. A cure effected by the ligature can only be permanent when it causes the aneurismal sac to be filled up, and the artery to be obliterated at its seat, after the same manner as compression. If a loose coagulum of blood merely forms in the sac as the result of the ligature, there is danger either of the sac suppurating (for the reasons already mentioned), or a secondary aneurism may form at the part; neither of which have ever occurred after the treatment by compression.

When pulsation returned in an aneurism shortly after the operation, it was always a source of considerable anxiety to the surgeon; but if the principle upon which aneurism is cured had been understood, it need not have occasioned any; it merely indicated that a rather stronger current than usual passed through the sac, and that the process by which it was to be filled up was proceeding. When pulsation returned long after a supposed cure by operation; in other words, when a second aneurism formed at the part, it was naturally looked upon as a much more serious matter, indicating that the operation had failed; and amputation of the limb was supposed to be the sole resource. But if again the theory upon which aneurism is cured had been understood, surgeons would have known that the formation of a secondary aneurism indicated that the sac had not been obliterated by the ligature, that nature had now set up the process by which this would be accomplished, and that a little assistance from art only was required to secure its permanency.

In addition, there are some other circumstances which tend to confirm what has been already said respecting the advantages of compression over the ligature. For instance, aneurism not unfrequently occurs in individuals in whom the coats of the artery leading to the sac are so much diseased, that the vessel, instead of taking on the adhesive inflammation after the application of the ligature, ulcerates, or the ligature cuts its way through, giving rise to secondary hæmorrhage. The disease is also not uncommon in individuals labouring under valvular or other disease of the heart, in subjects of intemperate habits, or broken down constitution, or in cases of the aneurismal diathesis, and where more than one aneurism exists at the same time; cases in which the ligature is contra-indicated, and where the surgeon with great reluctance would perform any operation; whereas pressure may be applied under these circumstances with nearly the same prospect of success as where the patient is perfectly healthy.

Again, aneurism occasionally occurs in individuals who have so much horror of a surgical operation that they cannot be induced to consent, although made aware of its absolute necessity and of the risk of delay. Such persons will gladly embrace any means by which they may be relieved from the necessity of undergoing an operation, and will cheerfully submit to any method of treatment which promises a chance of cure without it. Now, as the treatment of aneurism by compression involves no operation, patients labouring under this disease will probably make application at an earlier period, and for this reason will come under treatment at a more favourable period for effecting the cure than heretofore.

It will now be necessary to notice some of the objections which have or may be urged against compression as a mode of treating aneurism.

In the first place, it has been urged as an objection to this method of treatment, that the arteries are few in number to which it is applicable. But what is really the fact? The artery, above all others, in which aneurism is most common (after the aorta) is the popliteal, and next in frequency are the femoral and brachial. Lisfranc has given a table of one hundred and seventy-nine cases of aneurism, exclusive of those of the aorta, collected from various sources, out of which number the popliteal artery was engaged in fifty-nine instances, while the carotid was engaged only seventeen times, the subclavian sixteen, and the external iliac but five times. But even this is probably much below the average, because few cases comparatively of popliteal aneurism have been recorded (owing to its frequency) unless there happened to have been some peculiarity in the case; whereas most of the operations upon the carotid, subclavian, and iliac arteries, have been

published. It must be recollected, also, that aneurism of the subclavian, carotid, or iliac arteries near their origin, which do not admit of the application of compression, do not admit either of the employment of the ligature. It surely, therefore, is no argument against this method of treating aneurism that, because the disease occurs in arteries beyond its reach, we should refuse to apply it to vessels which admit of its application, or that the practice should be undervalued or condemned, because it cannot be used in every case.

It has been also urged as an objection to this method of treating aneurism, that it is more tedious and more painful than that by the ligature. Undoubtedly the treatment of aneurism by compression is often tedious, but that it is occasionally less so than the ligature, several of the cases already noticed prove, the pulsation having ceased after it had been employed for a few days only; while in the cases in which it proved very tedious, some of the causes previously enumerated may have operated to retard the cure. As to compression being a more painful method than the operation of applying a ligature to a large artery, including the subsequent dressings, until the wound is healed, this might be a question if the chances of recovery were equal in both, or if compression was employed upon the old theory of endeavouring to obliterate the artery at the site of the pressure; but we have already seen that compression not unfrequently actually relieves the severe pain from which the patient suffers in the affected limb; and that when it has been carefully graduated at first, tolerance of the remedy becomes established, and the patient is then able to maintain it for a longer period and with less inconvenience than under other circumstances.

I am far, however, from denying that compression as employed now, is not painful; indeed, some patients may find it so irksome as to refuse to continue it, or to call for the operation, the pain of which they suppose will be only momentary, and of the danger of which they are ignorant. But there is a great difference in this respect in different individuals, sometimes owing to the difference of sensibility to pain in different subjects, sometimes to a greater degree of pressure being required in one case than another. For instance, a moderate amount of pressure will diminish materially the current of blood in the artery in one subject, while a much stronger pressure will be required in another. This sometimes evidently depends upon the condition of the limb, more particularly the degree of development of the muscles, or the amount of superficial fat: a slighter degree of pressure will obviously be necessary when the limb is thin, and the muscles are poorly developed, than when this part is very muscular or much loaded with fat. Again, the condition of the heart and of the arterial system must likewise have some influence; thus, if the patient is plethoric, if the arteries are distended, or if the heart is hypertrophied, a greater degree of pressure necessarily will be required than under opposite circumstances. It is in such cases that venesection, by diminishing the tension of the arteries, and reducing the amount of blood, will generally be found serviceable; a less degree of pressure will be then required, less inconvenience will be experienced by the patient, and the pressure can be maintained for a longer period.

That there is a great difference in the sensibility to pain in different individuals has long been familiar to surgeons, and is frequently witnessed in operations; consequently some patients will be found who will maintain strong pressure for a long time without a murmur, while others will soon begin to complain, although the degree of pressure may be very moderate.

It is not, however, by contrasting the amount of pain experienced in these two opposite modes of treatment that we are to judge of their comparative merits. Admitting that, on the whole, the actual pain experienced is greater in the treatment by compression, when we contrast its perfect safety, its almost absolute certainty, with the risk and uncertainty which attend the operation, the advantages preponderate greatly in favour of compression. This appears to be the correct view to take of the question; and I am convinced that no medical man who had witnessed the treatment of aneurism by compression, and who likewise had experience of the ligature, would think of employing in his own person any other method; but would consider himself fortunate, if he could be relieved of so formidable a disease, by submitting to a greater degree of pain even than compression upon the artery occasions.

It has been objected to this method of treating aneurism that the pulsation is more likely to return than when the ligature has

been employed, owing to the artery not being obliterated at the point at which the pressure is made; consequently, that the patient cannot be considered safe from a relapse for a considerable time; while the period which has elapsed is too short to allow us to conclude that the cures which have been effected will be permanent. In reference to these objections, I shall merely observe, that the description, previously given, of the appearances found on a post-mortem examination of subjects who had been submitted to this method of treatment, proves that from the manner in which a cure is brought about, it must be permanent; and I may add, that in every case in which compression has been successfully used hitherto, the patient has remained well subsequently.

The foregoing are the principal objections which have been urged within the last few years against this method of treating aneurism; but they all admit of being readily answered. There are, however, one or two circumstances to which I may here allude, which probably have had as much influence in retarding its general employment, although they have not been ostensibly put forward as objections. Thus, it is difficult to do away with the prejudices of early education; surgeons have been taught, and have been in the habit of teaching for years, that the ligature is our sole resource in aneurism; besides they have so often witnessed the sudden and complete cessation of pulsation in the tumour on the ligature being tightened, that they cannot bring themselves to believe the disease can be cured in any other way. Again, in the treatment of aneurism by compression, the *éclat* to be gained by the successful performance of a capital operation is wanting; while this method of treatment imposes a far greater amount of trouble on the surgeon than the ligature.

Having now enumerated the advantages which compression possesses over the ligature, and having replied to the objections which have or might be urged against this method, I wish it to be understood that I do not advocate it as being free from inconvenience, free from trouble, or free from pain; the process by which compression effects the cure of aneurism is necessarily gradual, and requires time to be accomplished, and the surgeon, if he expects to succeed, must make up his mind to exercise a degree of patience which may be seldom called for in other cases; on the part of the patient likewise a considerable share of forbearance will be necessary; the former must be prepared to witness his exertions thwarted, and his endeavours fruitless for a long time; while the latter must be content to submit to confinement to bed for perhaps many consecutive weeks, and to the additional inconvenience of wearing a compressing apparatus during the greater part of that time. Although this is taking rather an unfavourable view of this method of treatment, and although in many of the cases which have been detailed, the cure was accomplished within a comparatively short period, yet it would be misleading those who have not seen this method employed, or who are about to try it for the first time, to let them suppose that it has no drawbacks; and that it does not occasionally prove both tedious and painful. Compression, however, possesses this advantage over the ligature, that if persevered in, it cannot fail of effecting a cure; the cure may be impeded or protracted owing to a variety of causes, but from the manner in which the aneurismal sac becomes filled up, it is evident that every day will contribute a little, and every hour the pressure is applied something will be gained; and no matter how long the treatment may last, if the patient and surgeon have sufficient perseverance, a permanent cure will ultimately be accomplished, while the employment of the pressure does not involve the slightest risk.

Having brought these remarks upon the treatment of aneurism by compression to a close, I shall conclude with a summary of some of the most material points bearing upon this method of treatment:—

1. The arteries to which compression is applicable being far more frequently the subject of aneurism than those to which it is inapplicable, compression is calculated to supersede the ligature in the great majority of cases.
2. The cure of aneurism by compression upon the artery between the aneurismal sac and the heart, according to the rules laid down here, is accomplished by the gradual deposition of the fibrine of the blood in the sac, until both the latter and the artery at the part are completely filled. The process is in fact exactly similar to that by which nature effects a spontaneous cure of aneurism.

3. Such an amount of pressure as would cause inflammation and adhesion between the opposite sides of the artery at the point compressed is never required.

4. The pressure should not be so great as to interrupt the circulation in the artery at the point compressed; an essential agent in the cure being that a current of blood should pass through the sac.

5. Compression by means of two or more instruments, one of which is alternately relaxed, is much more effectual than by any single instrument, and in many instances the pressure can be maintained by the patient himself.

6. The treatment of aneurism by compression does not involve the slightest risk to the patient, and if persevered in cannot fail of effecting a cure.

7. A cure of aneurism effected by compression, according to the rules laid down here, must necessarily be permanent; and in every case in which a cure has been accomplished, the patients have remained well subsequently.

8. The femoral artery remains pervious after the cure at the point at which the pressure had been applied, and no morbid change of any kind is to be detected in either the artery or vein at the site of the compression.

9. When a cure is effected by compression, the vessel is obliterated only at the seat of the aneurism, and the artery at this part is eventually converted into an impervious ligamentous band.

10. Compression effects the cure of aneurism by more simple and safer means than the ligature, while it is applicable to a number of cases in which the operation is contra-indicated or inadmissible.

11. Compression is not necessarily a more tedious or more painful method of treating aneurism than the ligature, while it is much more certain, more likely to be permanent, and is free from all danger.

12. Compression, according to the rules laid down here, has little analogy with the old method which went by this name; and in fact has no greater resemblance to it than the Hunterian operation had to the operation for aneurism which it superseded.—
Dublin Medical Press.

MIDWIFERY.

UTERINE POLYPI AND ULCERATION.

Dr. Montgomery has published in the August No. of the *Dublin Quarterly Journal of Medical Science*, a very valuable paper on *Uterine Polypi and Ulceration, with cases*. Dr. M. deduces the following conclusions, as the results of his extensive observation on this class of diseases.

1. That small uterine polypi, or polypoid excrescences, are of frequent occurrence.
2. That they are often not discoverable by touch alone, and so escape notice.
3. That they may even elude detection with the speculum, especially if the instrument used is not capable of separating the lips of the os uteri.
4. That they are a common cause of ulceration and menorrhagia, one or both; the cure of which requires, as a preliminary step, the removal of the polypus.
5. That while, on the one hand, a small polypus may escape detection, there is, on the other hand, a peculiar condition of the anterior lip of the os uteri liable to be mistaken for a polypus, and requiring a long time for its removal.
6. That very small polypi of the os uteri, when occurring in women of advanced age, especially if they are of the vesicular kind, are often the precursors of a malignant form of uterine disease.
7. That polypus being very frequently accompanied by ulceration of the os and cervix uteri, and its concomitant pain and structural alteration, the symptoms are occasionally mistaken for those of cancer; which error is most likely to be committed if an examination should happen to be made just when a polypus of rather large size is passing through, but still engaged in, and distending the os uteri.
8. That in cases of larger sized polypi, ligature is the means most generally eligible, as being safer than excision, though not so expeditious; its application having, in general, the immediate

effects of restraining the morbid discharges, and alleviating other symptoms, and ultimately curing the disease.*

9. That polypi and polypoid excrescences of small size are best removed by *torsion*; or in some instances, their destruction may be conveniently effected by *caustic*.

10. That with large polypi, torsion is unsafe, and should not be attempted.

11. That even with a polypus of small bulk, and slender pedicle, excision is not free from the risk of troublesome hemorrhage, while with those of larger size, there is great reason to apprehend such an occurrence taking place to a very dangerous degree, even though the precaution may have been taken of firmly constricting the pedicle with a ligature previous to its division.

12. That in ordinary cases of benign polypus, when no other disease exists in the uterus, the removal of the tumor by ligature, or other suitable means, is, in the vast majority of cases, completely successful, even under circumstances apparently quite hopeless.

13. That in malignant growths, such as cauliflower excrescence, removal by ligature will sometimes effect a complete cure, and that where the success is not so decided, much good may be done by the operation.

14. That the situation from which a polypus springs makes a considerable difference in the symptoms which it produces; a polypus of the lip of the os uteri giving rise to lower symptoms and much less discharge, than one of very inferior size growing from any part within the os uteri.

15. That a polypus of only moderate size, growing from the lip of the os uteri, is not likely to interfere injuriously with gestation or delivery, and its removal may be effected by, and as consequence of, the pressure which it sustains during the expulsion of the child.

16. That if a polypus, already detached, be too large to pass readily out of the vagina, it ought not to be allowed to remain there; but should be removed with the least possible delay, as its putrefaction may be attended with very unpleasant consequences.

17. That a fibrous tumor, originally formed in the substance of the uterus, may thence descend, pass through the os uteri, and form an ordinary pediculated polypus in the vagina.

18. That in the unimpregnated state of the uterus, this change will be effected gradually, and in general very slowly; but that should pregnancy occur, the descent and expulsion of the tumor may take place quickly under the expulsive action of labor.

19. That a polypus, even of large size, may thus make its appearance for the first time, immediately after delivery, no suspicions having been previously entertained of its existence.

20. That the cure of long-standing polypus, with large discharges, is liable to be followed by a condition of the system requiring precautions against determinations to the head.—*Dublin Quar. Jour., Aug.*

MISCELLANEOUS.

LE VERRIER'S PLANET.

In our last number (vol. 2, p. 439) was announced the discovery of the planet beyond Uranus, in accordance with the predictions of Le Verrier. This discovery must be considered one of the most remarkable recorded in the annals of science, and elevates Le Verrier to the first rank among astronomers. Of its history, we have room at present only for the following brief sketch.

Omitting to cite various notices which indicate that for several years past there has been among astronomers a growing suspicion of the existence of some unknown body in our system, by which the motions of Uranus is disturbed, we may quote the following as one evidence:

In the Comptes Rendus Acad. Sci., (Session Sept. 1, 1845.) xxi, 524, is an extract from the preface to *New Tables of Uranus*, by Eugene Bouvard, communicated to the Academy, in which, after speaking of the impracticability of reconciling, by any existing theory, the computed and the observed places of this planet, he adds: "the discordances between the observations and the theory induce me to believe that there is much probability in the idea

proposed by my uncle, (Alexis Bouvard, whose tables of Uranus, &c., were printed in 1821,) as to the existence of another planet, disturbing Uranus. This opinion, moreover, is further strengthened by the analogy which appears in the periodicity of these discordances, and those which Saturn would present if we should suppose Uranus unknown."

At the session of Nov. 10, 1815, (Comp. Ren., xxi, 1050.) Mons. U. J. LE VERRIER presented his *First Memoir on the Theory of Uranus*. Having alluded to the discrepancies between the observed and computed places, he says, "in the course of the last year, M. Arago represented to me that the importance of this question made it the duty of every astronomer to do his best to clear it up. I abandoned at once, in order to investigate Uranus, the researches on comets which I had undertaken, and of which several portions have already been communicated. Such is the origin of the work which I have the honour to day to present to the Academy." He proceeds to state in general his investigations of all the known perturbing causes operating on Uranus, and his determination of the actual amount of departure of Uranus from the places assigned by the theory.

In his second communication to the Academy, (at the session of June 1, 1846.) Le Verrier presents a history of the observations upon Uranus, and of the mode in which the tables of its motions have been constructed, and the errors which they involve; and a sketch of various hypotheses proposed to account for the inequalities of the motions of the planet. Having set these aside, he asks—*Is the other hypothesis of the existence of an unknown planet disturbing Uranus, more plausible?* After showing where this new planet cannot be situated, he arrives at this question—"Is it possible that the inequalities of Uranus are due to the action of a planet situated in the Ecliptic, at a mean distance double that of Uranus? And if so, where is the planet actually situated? What are the elements of the orbit which it traverses?" As one result of a rigorous discussion of this question, he gives, as a first approximation, this momentous conclusion, *that in assigning to the planet a heliocentric longitude of 325° for Jan. 1, 1847, there cannot be an error of 10°*. This assigned place he then promises to bring within narrower limits, by new computations. In recapitulating the labours required by his undertaking, he adds—"The existence of a planet hitherto unknown being thus established beyond a doubt, I have reversed the problem hitherto proposed in computing perturbations. Instead of measuring the action of a given planet, I have been obliged to set out from the inequalities observed in Uranus, in order to deduce the elements of the disturbing body, to give the place of this planet in the heavens, and to show that its action perfectly accounts for all the apparent inequalities of Uranus."

This remarkable prediction of the position of a planet hitherto entirely unknown, uttered with calm confidence by the mathematician in his closet, seems to have been received with faint faith even by the astronomical observers of Paris. For it is evident that the observer furnished with a good map of that region of the Ecliptic, which might have been made in a few hours from star catalogues, would have quickly detected a bright star not laid down. With a large telescope and a high power, this stranger would have presented a plain disc, and would thus have instantly disclosed its true character. Or, if, with a smaller instrument, its place had been carefully measured, the observation of the next morning would have shown its proper motion.

On the 31st of August, 1846, Le Verrier, with implicit reliance on the truth of his computation, presents to the Academy, a memoir "On the planet which causes the anomalies in the movement of Uranus," with a determination of its mass, its orbit and its actual position, (Comp. Ren., xxiii, 428.) In this paper he gives the elements at which he had arrived, as follows:—

Semi-axis major of the orbit,	36.154
Period of sidereal revolution,	217 yrs-387
Eccentricity,	0.10761
Long. of perihelion,	284° 45' M. Eqx. 1847-0
Mean long. Jan. 1, 1847,	318 47
Mass,	$\frac{1}{9300}$

From which he derives the following position of the planet; Jan. 1, 1847.

True heliocentric longitude,	326° 32'
Distance from the Sun,	33.06,

and remarks that the planet was in opposition August 19th pre-

* We are informed that the operation of excising a uterine polypus was recently performed in this city, with a fatal result, owing to excessive hemorrhage.—*Ed. N. Y. Journal.*

vions, and that the present was a favourable time to discover the body.

The semi-axis major might vary from 35.04 to 37.90, and the period from 207 to 233 sidereal years. The brilliancy of the planet ought to be about one third that of Uranus at its mean distance, and its angular diameter at opposition 3".3.

The action of the new planet, with elements as above determined, reconciles with theory, within very narrow limits, the observations of Uranus, both modern and ancient.

Even this memoir seems not to have overcome the incredulity or the indifference of astronomical observers, for it appears hardly possible that search could then have been made in the place pointed out by Le Verrier, without immediate success.

On the 5th of October, (Comp. Ren., xxiii, 657,) Le Verrier presented the fifth and last part of his researches, in which he gives his reasons for concluding that the plane of the orbit of the new planet is inclined at least $4^{\circ} 38'$ to the plane of the orbit of Uranus. In a postscript, he adds, that on the 18th of September, he addressed a letter to M. Galle of Berlin, asking his aid in discovering the planet, and that this astronomer discovered the body on the very day on which the letter reached him. Its observed place Sept. 23, 12h 0m 14s, Berlin m. t., was R. A. $325^{\circ} 19' 17''$ and S. Dec. $13^{\circ} 24' 8'' 2$; only $52'$ from the place assigned by Le Verrier. M. Gall was furnished with the Berlin Academy Star-map of the 21st hour, (by Bremiker,) then just published, yet other astronomers could with very little labour have made for themselves from the star-catalogues, charts abundantly sufficient for the detection of a new body of such brilliancy. The whole history of the affair evinces much distrust or apathy on the part of the astronomical observers, and undoubting confidence on the part of the mathematician,—confidence which the event has most fully justified.

The annals of science show that a discovery has often been made about the same time in different countries, and by persons unconscious of each other's labours. The present case offers another instance of this nature. In the Lond. Edinr and Dub. Phil. Mag., Vol. xxix, No. 197, Suppl. No., Dec., 1847, G. B. Airy, Esq., the Astronomer Royal, has published numerous letters and other documents, (most of which had already appeared in the London Athenæum of Oct. 3, 17, 31, and Nov. 28, 1846,) proving that Mr. J. C. Adams, of St. John's College, Cambridge, undertook, as long ago as 1843, an investigation of the anomalies of Uranus. As a result of his labours, he left, on one of the last days of October, 1845, at the Royal Observatory, Greenwich, a paper of which the following is an extract:—

"According to my calculations, the observed irregularities in the motion of Uranus may be accounted for by supposing the existence of an exterior planet, the mass and orbit of which are as follows:—

Mean distance, (assumed nearly in accordance with Bode's law.)	38 4
Mean sidereal motion in 365.25 days,	$1^{\circ} 30' 9''$
Mean longitude, Oct. 1, 1845,	$323^{\circ} 34'$
Longitude of perihelion,	$315^{\circ} 55'$
Eccentricity,	0.1610
Mass,	0.0001656."

If the English astronomers had now searched the Ecliptic, through but a few degrees on each side of the point here indicated by Mr. Adams, they would, with clear weather, undoubtedly have discovered the new planet within a week. That they did not do this, must probably be attributed to a want of confidence in the computation. Or if Mr. Adams' note had then been printed, he would have secured the glory which is now, according to the recognized rule, due to M. Le Verrier. So easily is a glorious opportunity lost forever!

The coincidence between the position for the planet assigned in Le Verrier's paper of June 1, 1846, and that which Mr. Adams had given, was so remarkable, that Prof. Challis undertook to search for the body, with the aid of the Northumberland telescope of the Cambridge Observatory, one of the largest refractors in the world. He commenced his sweeps July 29, 1846, and between this date and the time of the arrival of the news of the discovery at Berlin, he actually secured two observations of the planet, but without recognizing them until then. These places are.

	R. A.	N. P. D.
1846, Aug., 4d 13h 35m. 25	21h 58m 14s. 70	102 57 32.2
12 13 3 26	21 57 26.13	103 2 0.2

In a letter to Mr. Airy, dated Sept. 2, 1846, Mr. Adams gave results somewhat different from those communicated in October, 1845; the difference being due to the assumption of a mean distance about one-thirtieth less. He suggested, moreover, that "by still farther diminishing the distance, the agreement between the theory and the late observations might be rendered complete, and the eccentricity reduced at the same time to a very small quantity.

The new planet has doubtless been seen at all the observatories in this country, and may be easily detected by a good spy-glass. In the *Sidereal Messenger* Vol. i, No. 6, Prof. Mitchel, the Director of the Cincinnati Observatory, has given an interesting account of his first observation upon the body with the large refractor. Having received, Oct. 23th, the news of the discovery, he directed the telescope, soon after 6 p. m., to the region of the heavens occupied by the planet, taking his place at the finder, the assistant being seated at the large telescope. "The planet was described as a star of the 8th magnitude. On placing my eye to the finder, four stars of this magnitude were seen. The first was brought to the centre of the field of view of the Equatorial, and after examination by my assistant was rejected—a second was examined critically, and in like manner rejected. The third star, a little smaller and whiter than the other two, was now brought into the field of view, and instantly I heard the exclamation from my assistant—'There it is! there's the planet! with a disc round, clear, and beautiful as that of Jupiter!' My own eye was now placed to the eye piece of the great refractor, and to my unspeakable pleasure, I found a beautiful disc, so well defined, that without any knowledge of a previous discovery, it never would have been passed over for a moment." Prof. Mitchel immediately proceeded to measure the diameter of the disc, six measures being made by his assistant, and six by himself; the mean of the whole gave 2.523. This is somewhat less than the result given by Schumacher. The real diameter of the planet is probably more than 40,000 miles.

The name of the new planet seems not yet quite determined. The mythological designations of *Janus*, *Oceanus*, *Neptunus*, *Atlas*, &c., have been proposed. M. Le Verrier, to whom the right of imposing the name undoubtedly belongs, has delegated this right to M. Arago. The latter denominates it *Le Verrier*. It seems unwise thus to depart from the received system of nomenclature; as *Uranus* and the five small planets must then change their titles; and it is also quite possible that the names of future discoveries may be either unpleasantly short or immoderately long, or otherwise unsuited for this celestial use.—*American Journal of Science and Arts*.

THE

British American Journal.

MONTREAL, MARCH 1, 1847.

INHALATION OF SULPHURIC ETHER VAPOUR.

For the last two or three months, our exchange periodicals have teemed with remarks upon the narcotizing influence of the inhalation of sulphuric ether vapour, in surgical cases. We have attentively watched the progress of the discussions to which this novel mode of relieving the pain incident to surgical operations naturally led; and while we cannot but reprobate the method adopted by Mr. Morton, a dentist of Boston, (who claims the discovery,) in patenting the process, and endeavouring to render it tributary to his own pecuniary advantage, nor less, the encomiums passed upon it by Drs. Bigelow, Warren, and Hayward, of Boston, who lent themselves and their high names to the furtherance of the plans of the patentee, by recommending and countenancing what was

then, certainly, a secret nostrum, we yet conceive that there exist ample grounds for announcing this important act, that the pain attendant upon surgical operations may, in a great majority of cases, be very considerably alleviated, if not entirely allayed, by a recourse to the means of which we are now writing. The material employed is freshly washed sulphuric ether, with which a sponge is saturated, after having been placed in a two-necked bottle, to one of the necks of which is attached a mouth-piece. We have heard and read, that the ether contains a narcotic ingredient in solution, such as morphia, added with the intention of increasing and insuring the narcotic influence. This, however, is by no means essential. We do not believe that any of the morphia is inhaled under such circumstances, the narcotic and tranquilizing effects being entirely due to the ether—effects which that pharmaceutical agent has been for years known to be capable of inducing, although never thus specially applied until now.

We confess ourselves generally sceptical as to the good results which are frequently promised to flow from the employment of new remedies, and new modes of treatment, by those who, from laudable or interested motives, usher them before the notice of the profession. The very equivocal circumstances under which this one was laid before the profession rendered us even more sceptical and distrustful than usual: but we have perused a mass of evidence which has certainly convinced us most forcibly, that a means is now presented for mitigating, to a very marked extent, the excruciating pain and agony which are necessarily attendant upon operations of any magnitude.

How beneficial soever the employment of this agent may be, it appears that its use is not unfrequently attended with very unpleasant consequences; which may be, and probably are in many cases dependant on idiosyncrasy, and indicate great caution in its use. The symptoms of narcotism have occasionally proceeded to such an extent as to demand a counter treatment, and the cessation of the inhalation. Apoplectic symptoms, denoted by stertor, slow labouring pulse, and cold extremities, have been also witnessed; and, in some cases, violent muscular exertion, an effect the very opposite of that intended to have been produced. Great caution is, therefore, on these grounds, demanded, in the employment of the remedy; and a sufficient number of observations have scarcely yet been made, on which to found rules for its proper exhibition, although sufficient to prove its valuable narcotizing influence in a generality of the cases specified. Our readers will, we are satisfied, find the proceedings of the Surgical Society of Ireland on this

subject, which we have given on another page, full of interest.

THE MONTREAL SCHOOL OF MEDICINE AND SURGERY AND ITS DIPLOMAS.

In our last number we considered it our duty to apprise the profession and the public, of the proceedings of the School of Medicine and Surgery of this city in having issued diplomas without authority; we quoted their own words, from their own circular, as confirmatory of the fact; we gave the names of the parties who received them, and, we have been since informed, paid for them; and we exhibited a copy of the document from a diploma which we had seen. Sensible that the integrity of the profession was menaced by the proceeding, we felt ourselves compelled, not only to notice, but to condemn it; and we freely opened our pages to any reply which the members of that school might, under the circumstances, have been induced to make. But no reply has been received. We were either wrong or right in the course which we adopted. If the former, then were we highly censurable; but if the latter, then was the proceeding adopted by the school worthy of the severest reprobation. Had we been assured that the proceeding which called forth our animadversion would have been discontinued, we should have offered no further remarks on the matter; but when we have ascertained, and that on good authority, that the practice is to be continued, and that, in the course of a couple of months, a repetition of the scenes of last year is to be witnessed, and an additional number of their diplomas is to be foisted upon the profession and the public, we purpose to examine more closely into the legality of the proceedings; and we anticipate, that, however much our researches may "adorn a tale," they will not, in their results, be found particularly well qualified to "point a moral."

Of the three faculties, divinity, law, and physic, none is open to the same abuses as the last, in its practice, and in the honours which appertain to it. The credulity of the public mind is notorious; were it not so, quacks and quackery would long since have ceased to exist. But the case is far otherwise; and consequently, not only is the practice of quackery, discountenanced, because fraught with evils of incalculable magnitude to the public, who are the real sufferers, but the slightest tendency to it on the part of the regular practitioner, is certain to be visited by reprehension, because casting discredit on the science of medicine, and violating its ethics. So is it, also, with the honours of the profession, whether in the shape of degrees or diplomas. They are passports to the favourable consideration of the holder on the part of the public. They are the public's guarantee of quali-

fication. To be of value, they must be genuine; that is to say, must be granted by corporate institutions, legally empowered to that effect; if otherwise, they are spurious; and although in themselves, under such circumstances, of no intrinsic value, yet the offence committed against the interests of the profession is so much the greater; for they not only tend to throw discredit on its real honours, which every member of the profession, jealous of its integrity, is bound to protect, but at the same time it becomes a *specious document, palmed off upon the community*, who have not the means generally of estimating its authenticity.

But it is contended that the diploma granted by the School of Medicine is an *honorary diploma*, and that they possess a right to grant such a document. By reference to their act of incorporation, proof abundant may be found that they are endowed with no authority to grant an *ordinary diploma*; *a fortiore*, they have none to grant an *honorary one*. But what a perversion of the term! The time was when honorary degrees and diplomas were the spontaneous tributes of universities and colleges to high scientific and literary attainment; now, and in this city, are they pretended to be granted to the mere students of a school. Formerly, money could not purchase them; now are they obtainable for the sum of \$15 each. *O! tempora! O! mores!*

But let us contrast the diploma of the School, with that of the Royal College of Surgeons, of Edinburgh, of which we now give a copy:—

COLLEGIUM REGIUM CHIRURGORUM CIVITATIS
EDINBURGENSIS.

Hicce literis testatur, virum ingeniosum, St. G. studiis præscriptis rite peractis, examini sese subiacisse, atque ita ad interrogata de iis respondisse, ut numeri tam Chirurgico quam Pharmaceutico, suscipiendo omnino par esse videretur.

Edinburgi die primo mensis Aprilis, anno millesimo octingentesimo trigésimo quarto. (S. P.)

JOHANNES CAMPBELL, PRÆSES,
GULIELMUS BROWN,
JOHANNES GAIRDNER,
ALEXANDER M'CAULAY,
JOHANNES M'FARLANE, &c. &c.

Let our readers now compare this document with the diploma of the school published in our last number. The *spirit* of both is the same: the one is a testimonial of qualification, so is the other; and the one possesses exactly the same claim to be considered an honorary diploma as what the other does. But there is this differ-

ence, (one not easily appreciable by the public in general, as the analogy is drawn as closely as the difference of name and locality would permit, the year of incorporation of the school having been also affixed,) that the one is a valid document, emanating from a college in whose charter the authority is expressly delegated, while the other is the invalid document of a school, whose act of incorporation has delegated to them no power whatever, for such a purpose.

But we have stated in our last number, and we repeat it in this, that without an expressly delegated authority, the whole proceeding is illegal. Putting altogether out of the question the exceeding absurdity of the position, that because their act of incorporation, does not in express terms prohibit the issuing of diplomas, the school has a right to grant them, we will now show, that an *expressly delegated authority is required*, and we shall refer to British precedent and British practice, at the same time, challenging the proof of a single corporate Institution, in any part of the world, issuing degrees or diplomas without such duly transmitted power from the proper authorities of the respective countries.

The right of the Society of Apothecaries of London, to examine and grant their letters testimonial of qualification, will be found in the following extract from their charter, granted by King James the First, and printed by order of the House of Commons, on June 22, 1825.

“Et postquam hujusmodi septem anni servitij sive tyrocinij (ut præfectorum, fuerint elapsi et exacti quod tunc unusquisq. talis apprenticius coram magistro et custod., pro tempore existentibus appareat et præsentetur, ac per eosdem magistrum et custodes, (advocat. sibi president. collegij sea communitatis facultatis medicinæ London pro tempore existen. aut aliquo medico aut aliquibus medicis per dict. president. nominand., et ad hoc de tempore in tempus assignand. si super moineoem inde factum tal. prædict. medicus vel talis prædicti medici, adesse voluerit et advisament. cum eodem vel eisdem, habit. circa cognitionem et electionem simplicium et circa medicament. preparationem dispensationem, tractionem, commixtionem et compositionem, examinetur, probetur, tentetur, ac per eosdem medicos, magistrum et custodes, spectatur et approbatus fuerit, priusquam officinam Pharmacop. haberè, tenere, instruere aut medicamenta quacunq. præparare,” &c. &c.

A similar power was delegated to the Royal College of Physicians of London, in their charter, granted in the year 1522, by Henry VIII., and couched in the following words:—

III. “And where that in Dioceses of England, out of London, it is not light to find alway men able sufficiently to examine (after the statute) such as shall be admitted to exercise physick in them, that it may be enacted in this present Parliament, That no person from henceforth be suffered to exercise or practise in physick through England until such time as he be examined at London, by the said President, and three of the said Elects; and to have from the said President or Elects, letters testimonial of their

approving and examination, except he be a graduate of Oxford or Cambridge, which hath accomplished all things for his form without any grace."

The University of London is thus empowered. The extract is taken from its charter, dated 5th December, 1st Victoria :—

"And we further will and ordain that the said Chancellor, Vice-Chancellor, and Fellows, shall have power after examination to confer the several degrees of Bachelor of Arts, Master of Arts, Bachelor of Laws, Doctor of Laws, Bachelor of Medicine, Doctor of Medicine, and to examine for medical degrees in the four branches of Medicine, Surgery, Midwifery, and Pharmacy," &c. &c.

In the supplemental charter granted to the Royal College of Surgeons of Ireland, dated at the Court of St. James, 25th day of November, 1843, we find the following relating to the examination of candidates for letters testimonial :—

"And we do for us, our heirs, and successors, further ordain and appoint, that the examiners of the said College, or so many of them as may hereafter be declared necessary to constitute a Court or Board by any by-law, shall from time to time, upon request made to the President, or, in his absence, to the Vice-President, or any two of the Council of the said College, examine in such form and manner, and on such subjects as the Council may from time to time direct and prescribe, every person who shall be desirous of obtaining the certificate or letters testimonial of the said College, of his qualification to practice under the common seal of the said College," &c. &c.

Irrespective, however, of what we have advanced, the circumstances attending the application of the school for the power, and the actual refusal of that power by the Legislature, by deliberately striking out of their Act, every clause which could have admitted or conceded it, are, we think, sufficiently demonstrative, that it was not the intention of the Legislature that they should have exercised it, and that their doing so is necessarily illegal.

We think we have now furnished proof abundant, that, in the first place, honours in medicine, whether in the shape of degrees or diplomas, are not granted without especially delegated powers; in the second place, we have exhibited that no such powers have been granted to the School of Medicine by the Legislature, which conceded to them their Act of Incorporation; and, thirdly, it follows that, possessing no due authority, the diplomas which they have issued are illegal and invalid; and their proceeding, not only on these grounds, but also as affecting the best interests of the profession, to be condemned.

A curious corollary may be deduced from the conclusions just drawn from our argument. It will naturally suggest itself to every mind, and it consists in the fact that the school charges, and has received, the sum of £3 15s. for each diploma, the issue of

these diplomas being illegal, and the documents themselves invalid.

We do not wish to be considered the enemy of the school, or that we are writing against it through any acrimonious spirit. None more than ourselves more sincerely wish them well; but the path of rectitude is open to them as well as to all, and if they cannot walk in it themselves, they must excuse us if we direct them. We would wish them to preserve the character of the profession, not to impair it. To protect, not to injure its interests in its honours. The assumption of powers and privileges to which they are not entitled, is a procedure less likely to secure to them the confidence and the esteem of the profession at large, than to cause them to forfeit both.

In conclusion, we have to remark, that, however desirous certain parties may be to assign to the present article, and the one which appeared on the same subject in our last number, motives of a medico-political character, we utterly disclaim any such principle as guiding us. Politics have nothing whatever to do with the matter. It is a question simply of right or wrong. We think that the school, in the course which it has adopted, has far outstripped the bounds of propriety, and our earnest desire is to see it retrieve its position, by retracing its steps, and to desist from a practice, which cannot enhance its reputation in the eyes of the profession or the community at large.

RETIREMENT OF DR. MACDONNELL FROM THE CO-EDITORSHIP OF THIS JOURNAL.

It is our duty to announce to our readers, that Dr. MacDonnell has retired from editorial connection with this journal. In announcing this circumstance, it is incumbent on us to observe, that although not officially connected with it, he yet will afford the journal the benefit of his counsel and advice on important matters, and that his pen will not be idle in its favour, when favourable opportunities and leisure permit. We have sincerely to thank him for the judicious assistance hitherto afforded to us, and it is with no slight feelings of pleasure that we state, that although his name be no longer officially connected with us, an unabated interest on his part in the prosperity of the journal still exists.

Manuel de la Société de Tempérance, dédié à la Jeunesse Canadienne, par le Rev. Pere C. Chiniquy, Ptre. N. Oblat de Marie Immaculée. Seconde édition, etc. Montreal: Imprimeurs, M. M. Lovell & Gibson. 1847.

This little work, comprising 180 pages, written in the French language, is devoted to the cause of the temperance reformation, through the instrumentality of

temperance societies. No men possess greater opportunities of witnessing the deplorable consequences of intemperance, morally as well as physically, than medical men. Far too frequently are they summoned to the bedside of suffering, disease, and a premature death, the seeds of which may be clearly traced to indulgence in that baneful habit. Philanthropically disposed, from the very nature of their profession, means destined to alleviate this fertile source of misery, receive general countenance at their hands. The author has treated well his subject, and we only regret, that so much of a religious character has been imparted to the work, that its general use will, of necessity, become much restricted, and its utility proportionately diminished. The reverend author seems to have forgotten, that among the Canadian youth are Protestants as well as Roman Catholics, and that there are to be found among the former, too many upon whom this manuel might have produced great good, had it been presented in a form to which religious scruples on the part of friends could have urged no objection.

ACADEMY OF MEDICINE AND SURGERY IN THE CITY OF NEW YORK.

A meeting of the most influential members of the Profession, was lately held in the City of New York, for the purpose of organizing themselves into an association under the above designation. The proceedings were characterised by great unanimity of feeling and sentiment, and a committee having been appointed to draft a *constitution*, the objects of the association will be seen by a perusal of the Articles which were adopted at a subsequent assembly. We augur important and substantial benefits from the establishment of this academy; benefits, which, if the institution be properly managed, will reflect themselves on the profession at large, elevating not only its moral tone, but enhancing very considerably its social and political influence. The following is the constitution:

ARTICLE I. This Association shall be called the "NEW YORK ACADEMY OF MEDICINE," and be composed of Resident and Corresponding Fellows.

ART. II. The object of the Academy shall be:—

- 1st. The separation of Regular and Irregular Practitioners.
- 2d. The association of the Profession Proper for purposes of mutual recognition and fellowship.
- 3d. The promotion of the character, interests, and honour of the fraternity, by maintaining the union and harmony of the regular profession of the City and its vicinity, and aiming to elevate the standard of Medical Education.

4th. The cultivation and advancement of the Science, by our united exertions for mutual improvement, and our contributions to Medical Literature.

ART. III. The Resident Fellows shall be Regular Practitioners of Medicine or Surgery in the city of New York or its vicinity; shall be proposed by a Fellow of the Academy to the Committee on Admissions, which shall satisfy itself of the regular standing of the candidate, by credentials or otherwise, and upon its recommendation he may be admitted by vote of the Academy at a regular

meeting. A residence of three years in this city or vicinity shall be necessary to eligibility in the Fellowship of the Academy.

ART. IV. No Proprietor or Vendor of any patent or secret remedy or medicine, or any Empirical or Irregular Practitioner, shall either be admitted to, or retained in, the Fellowship of this Academy.

ART. V. Corresponding Fellows may be elected on the nomination of the Committee on Admissions, which shall vouch for their being duly qualified practitioners; but the votes of three-fourths of the Fellows present, at a regular meeting, shall be necessary for such election. The number of Corresponding fellows shall be limited to one hundred.

ART. VI. The Officers of this Academy shall be, a President, four Vice-Presidents, a Recording Secretary, two Corresponding Secretaries, designated for Domestic and Foreign Correspondence; a Treasurer, and a Librarian; who shall be elected annually by ballot at the regular meeting in January. They shall severally perform the duties indicated by the title of their respective offices.

ART. VII. The President shall appoint, immediately after his election, the following Standing Committees, each of which shall consist of five Resident Fellows:—

- 1st. A Committee on Admissions.
- 2d. A Committee on Finance.
- 3d. A Committee on Medical Ethics.
- 4th. A Committee on Publication.
- 5th. A Council of Appeal.

ART. VIII. Alterations of this Constitution shall not be made except at a meeting subsequent to that at which such alteration shall have been proposed in writing.

Progress of the Cholera.—We copy from the Glasgow *Constitutional* the following additional information respecting this singularly fatal disease:

Trebizonde, December 7.
The cholera is pursuing its march towards Europe. We have received accounts from Tabriz to the 24th of November, by which we learn that the scourge had ceased in that city. During eight days no case had been declared. The disease had carried its ravages to Choi, Makou, and Bajasid. This last city, of which the population had been several times decimated by the plague, is situated on the Turkish territory, adjoining the Persian and Russian frontiers, to the south of Mount Ararat. Thus the cholera is approaching the Black Sea by the road taken by the caravans, whilst it ascends, in another direction, the banks of the Euphrates and the Tigris, shaping its course towards Syria. Europe, therefore, is menaced on two sides. Notwithstanding the intense cold which prevails in Aserbeidjan, and on the table lands of Turkish Armenia, the scourge has extended thither: Neither the elevation of a town, nor the cold, appear to destroy the terrific epidemic. The population at Tabriz, which counted 125,000 souls, is now reduced to 100,000. More than 15,000 perished, and all the foreign merchants have quitted it.

La Lancette Canadienne.—This journal has increased its dimensions: the last number contained sixteen pages, instead of four, with which it commenced its existence. The journal is a neat specimen of Messrs. Lovell & Gibson's style of work. The selections are made with great judgment, and bespeak able management on the part of its editor, Dr. Leprohon. We wish it prosperity.

New Medical College in Philadelphia.—We have received the circular of a newly established College at Philadelphia, called "The Philadelphia College of Medicine," making, with those previously instituted, the fifth Medical Institution in that ancient seat of learning.

Only four Professors compose the Faculty. James M'Clintock, M.D., who gave popular lectures in this city a few years ago on Anatomy, is the Professor of Anatomy, Surgery, and Physiology. The number of lecturers in the different branches is far too limited. It is impossible that the subjects can receive justice, especially when we find that one lecturer's duty embraces the extensive fields of the Theory and Practice of Medicine, Midwifery, and Medical Jurisprudence; and, as in Dr. M'Clintock's case, Surgery, coupled with Anatomy. They must have associates, and the sooner they are added to the number now existing, the better will it be for the lecturers themselves, and the students who may attend their classes.

The following was the mean temperature of the air, as taken at 7 o'clock, a.m., and at 3 and 10 p.m., at Her Majesty's Magnetical Observatory near the city.

January,	26.1	May,	55.77	September,	63.41
February,	20.8	June,	63.82	October,	44.81
March,	33.4	July,	68.22	November,	40.82
April,	44.11	August,	68.41	December,	27.64

STATISTICS OF THE TORONTO GENERAL DISPENSARY, FOR THE YEAR 1846.

Medical Officers.

Dr. Joseph Hamilton, | Dr. E. M. Hodder,
Dr. J. E. Rankin, | Dr. G. R. Grasett.

	Between 1st Jan. and 31st March.	Between 1st April and 30th June.	Between 1st July and 30th Sept.	Between 1st Oct. and 31st Dec.	Total.
Remained at last Report,
Admitted during the year 1846,	208	213	298	158	907
Cured,	155	185	262	94	696
Relieved,	32	37	24	5	98
Discharged for non attendance,	15	12	4	5	36
Transferred to General Hospital,	1	3	2	3	9
Died,	5	6	6	4	21
Remaining,	47	47
Per centage of Deaths, 2 1/2.					

General Observations.

The *epidemic* diseases of the present year were, *rubeola* during the spring months, and *variola* during the autumn and beginning of the winter. The former, in general, was of a mild character, and has wholly disappeared. The latter is spreading rapidly, and many cases of the confluent form have occurred. The cases of remittent fever were from the neighbourhood of the Don, a sluggish stream at the east end of the town, where a tract of land is always, more or less, partially submerged. An extensive marshy surface is thus left, and the soil naturally rich, produces an abundant growth of wild grass, and other aquatic plants, and extensive vegetable decomposition takes place during the dry and warm weather of summer. Intermittent fever is therefore endemic during the autumnal season.

Admitted during the year.

DISEASES AND ACCIDENTS.

Abcessus,	9	Icterus,	2
Abortio,	2	Laryngitis,	1
Amenorrhœa,	16	Leucorrhœa,	3
Ambustio,	6	Lichen,	1
Amaurosis,	1	Luxatio,	1
Aphthæ,	5	Melœna Cruenta,	1
Anasarca,	5	Meningitis,	1
Asthma,	3	Menorrhagia,	5
Apoplexia,	1	Metritis Chronica,	1
Ascites,	1	Morbus Cordis,	5
Arthritis,	8	Necrosis Tibiæ,	1
Bronchitis Acutus,	5	Neuralgia,	2
" Chronicus,	13	Nymphomania,	1
Cancrum Oris,	1	Obstipatio,	35
Cardialgia,	1	Obstructio Œsophagi,	1
Catarrhus Acutus,	66	Odontalgia,	16
" Chronicus,	14	Ophthalmia,	24
Cephalalgia,	2	Orchitis,	1
Chlorosis,	3	Otitis,	1
Cholera Canadensis,	10	Paralysis,	4
Colica,	6	Palpitatio,	1
Contusio,	17	Parturitio,	2
Cynanche Parotideæ,	1	Pertussis,	8
" Tonsillarior,	4	Plegmon,	3
Debilitas,	17	Phthisis Pulmonalis,	14
Dentitio,	10	Phlebitis,	1
Diarrhœa,	94	Pleurodynia,	6
" Chronica,	4	Pleuritis,	4
Dysœcœa,	3	Polypus Uteri,	1
Dysenteria,	6	Porriigo Capitis,	16
Dysuria,	3	Pneumonia,	11
Dyspepsia,	43	Prurigo Scroti,	2
Enteritis,	2	Psora,	1
Entropium,	1	Psoriasis,	4
Enuresis,	1	Pyrosis,	1
Epilepsia,	1	Rheumatismus Acutus,	21
Erythema,	2	" Chronicus,	18
Excoriatio,	2	Rubeola,	49
Febris Continua Communis	19	Schirrus Pilon,	2
Intermit. Quotidiana	36	Serofula,	5
" Tertiana,	43	Sequelæ Morborum,	10
" Remittens,	4	Spasmus,	1
" Infantilior,	6	Splenitis,	2
Fractura,	2	Subluxatio,	3
Gastritis,	1	Syphillis Primaria,	3
Gastrodynia,	6	" Consecutiva,	2
Gastro-enteritis,	2	Suppressio Mensium,	1
Gelatio,	2	Tabes Mesenterica,	2
Gonorrhœa,	3	Tinea Capitis,	1
" Dormientium,	1	Ulcus,	17
Hæmorrhœis,	2	Variola,	6
Hæmoptysis,	1	Varicella,	1
Hæmorrhagia Uterina,	1	Vene, Varicosæ,	2
Hæmatemesis,	3	Veneficium,	1
Hæmiplegia,	2	Vermes,	27
Hepatitis Chronica,	7	Vertigo,	1
Hernia,	2	Vesicæ Irritatio,	1
Hæpes,	5	Vulnus Incisum,	4
Hydrops Saccatus,	1	Total,	907
Hypertrophia Splenii,	1		
Hysteria,	6		

Remarks.

A fatal case of contusio, was the result of chronic injury, produced by severe concussion.

A fatal case of acute rheumatism was in a boy, and terminated in extensive abscess. Death took place suddenly.

The majority of cases of intermittent fever, both of the quotidian and tertian form, was from March Street, which is almost exclusively in the possession of persons of the poorest class, many of whom take up their temporary abode there, and hence the probable reason that a majority did not proceed from the neighbourhood of the Don, (see remarks to 1st table,) from which, notwithstanding, a large proportion was admitted.

The frequency of dyspepsia is attributed to the readiness of obtaining ardent spirits, which are freely used by the class of patients receiving relief from this Institution.

Males,	367
Females,	540
	907

Married, as far as ascertained, 277

Labourers,	109	Sailmakers,	1
Shoemakers,	7	Blacksmiths,	1
Carpenters,	10	Sawyers,	1
Tailors,	2	Millers,	1
Curriers,	1	Distillers,	1
Gardeners,	10	Limeburners,	1
Shoehinders,	3	Servants, Nurses, &c.,	44
Sailors,	8	Char-women,	41
Butchers,	1	Laundresses,	36
Tinsmiths,	4	Employed in Needlework,	46
Apprentices,	3	" in Domesticwork,	159
Stonemasons,	1	Quilt & Stocking-knitters,	
Printers,	1	Spinning, &c.,	14
Fruiters,	6	Straw-bonnet makers,	1
Pedlars,	4	Midwives,	1
Ostlers,	1	Agents,	1
Tin-roofers,	1	Teachers,	2
Weavers,	2	At School,	45
Bookbinders,	2	Without any Occupation,	324
Hucksters & Market-women	8		
Carters,	2	Total,	907
Ropemakers,	1		

Remarks.

Those named as being without occupation, are infirm persons; aged pensioners; and young children, &c.

Natives of—England, 78; Ireland, 541; Scotland, 19; Wales, 1; Isle of Man, 1; West Indies, 1; Canada, 232; United States, 34; Total, 907.

General Observations.

The natives of Ireland have a large preponderance, as exhibited in this table, and this number would be greatly augmented, were their children included, who were born soon after their arrival in this country. Many of those entered, as belonging to the United States, are the children of parents of British origin, and a few consist of coloured persons.

Toronto, January, 1847,

TO CORRESPONDENTS.

A letter from Dr. Grasset, Toronto, has been received. A reply will be transmitted in the course of a few days.

We have been advised of a communication from — of —, a village about 30 miles distant from this city. Intemperance in medical practitioners cannot be too severely reprobated; but we do not see what redress can be afforded in the case specified. It is melancholy to contemplate to what results this habit leads. However strong the suspicion may be, that through the intemperance of the individual alluded to, and his consequent improper practice in the cases, death may have been the consequence; yet it would be a very difficult matter to afford or adduce sufficient proof of such a circumstance. Could such proof be afforded, and the case be sustained, an indictment for manslaughter would undoubtedly lie. The community, however, has in its own hands the means of punishment; and it is one which would infallibly tell; it is not to employ him or consult him. The reason would soon force itself upon his mind, and in self-defence he would be compelled to quit the neighbourhood or reform his habits.

ERRATA IN OUR LAST.—The name of Dr. Fenwick recently graduated at McGill College, should have been George Edgeworth, instead of George Augustus.

For "November," in the heading of the Meteorological Table, for the City of Montreal, read "December."

From an oversight, the three following articles in our last number were not accredited, namely, that on Gun Cotton, Ammonia as a Vesicant, and Citrate of Iron and Ammonia. The first was taken from the American Journal of Science and Arts; and the two last from the Southern Journal of Medicine and Pharmacy. We have frequently laid our esteemed cotemporaries under contribution to fill our pages; and we would not repay them by robbing them, as it were, of their articles, without proper acknowledgement. "Sum cuique tribuito," is an adage which we would not willingly violate.

TO SUBSCRIBERS.

We beg to apprise our subscribers in Canada West, that Mr Wood is at present collecting subscriptions due to this journal.

BOOKS etc. RECEIVED DURING THE MONTH:

- Boston Medical and Surgical Journal, February 3, 10, 17, 24.
- New York Medical and Surgical Reporter, February 6, 13, 20.
- Western Lancet, January, 1847.
- Southern Medical and Surgical Journal, February, 1847.
- Medical News and Library, February, 1847.
- The Medical Examiner, February, 1847.
- La Lancette Canadienne, February, 1, 15.
- Annual Announcement of the Philadelphia College of Medicine, Session 1847.
- Buffalo Medical Journal, No. 9.
- Dublin Medical Press, January 6, 13, 20, 27, February 3.
- Stockton's Dental Intelligencer, February 1.
- Twenty-sixth Annual Report of the Bloomingdale Asylum for the Insane. By P. Earle, M.D. New York, 1847.
- Illustrated Botany, December, January.
- Report of the Pennsylvania Hospital for the Insane, for the year 1846. By Thomas, P. Kirkbride, Physician to the Institution. Philadelphia, 1847.
- Fourth Annual Report of the Managers of the State Lunatic Asylum, made to the Legislature, February 2, 1847. Albany.

BILL OF MORTALITY for the CITY of MONTREAL, for the month ending JANUARY 31, 1847.

DISEASES	Male.	Female.	Total.	Under 1.	1 & under 3	3 — 5	5 — 10	10 — 15	15 — 25	25 — 35	35 — 45	45 — 55	55 — 75	75 upwards
	EPIDEMIC OR INFECTIOUS	Scarlatina,.....	2	2	2	1	2	2	1	2	1	1	2	1
	Small Pox,.....	1	2	3	1	2	2	2	2	1	1	2	1	1
	Fever,.....	10	6	18	2	5	2	2	1	2	1	2	1	1
DISEASES OF BRAIN AND NERVOUS SYSTEM	Hydrocephalus,....	2	2	4	1	2	1	1	1	1	1	1	1	1
	Paralysis,.....	2	2	4	1	2	1	1	1	1	1	1	1	1
	Convulsions,.....	1	4	5	4	1	1	1	1	1	1	1	1	1
	Dentition,.....	10	4	14	7	7	1	1	1	1	1	1	1	1
	Inflam. of Brain,....	1	1	2	1	1	1	1	1	1	1	1	1	1
	Apoplexy,.....	1	1	2	1	1	1	1	1	1	1	1	1	1
DISEASES OF RESPIRATORY ORGANS	Consumption,.....	14	11	28	8	2	1	1	6	4	3	3	3	3
	Pleurisy,.....	1	1	2	1	1	1	1	1	1	1	1	1	1
	Croup,.....	1	1	2	1	1	1	1	1	1	1	1	1	1
	Diarrhea,.....	1	1	2	1	1	1	1	1	1	1	1	1	1
DISEASES OF ABDOMINAL VISCERA	Dropsy,.....	2	3	5	1	1	1	1	1	1	1	1	1	1
OTHER CAUSES AND DISEASES, AND DISEASES NOT SPECIALLY DESIGNATED	Debility,.....	2	7	9	1	2	1	1	1	3	1	1	3	6
	Inflammation,.....	8	7	15	8	2	1	1	1	1	2	1	1	1
	Unknown,.....	4	2	6	2	2	1	1	1	1	1	1	1	1
	Still-born,.....	2	2	4	1	1	1	1	1	1	1	1	1	1
	Sudden Death,.....	2	1	3	1	1	1	1	1	1	1	1	1	1
	Accidental,.....	2	1	3	1	1	1	1	1	1	1	1	1	1
	Total ,.....	62	59	122	33	20	9	4	10	10	6	9	10	7

MONTHLY METEOROLOGICAL REGISTER AT MONTREAL FOR JANUARY 1847.

DATE.	THERMOMETER.				BAROMETER.				WINDS.			WEATHER.		
	7 A.M.	3 P.M.	10 P.M.	Mean.	7 A.M.	3 P.M.	10 P.M.	Mean.	7 A.M.	Noon.	6 P.M.	7 A.M.	3 P.M.	10 P.M.
1,	+23	+31	+17	+27-	29.92	29.98	30.07	29.99	W.	W.	W.	Snow	Fair	Fair
2,	" 22	" 32	" 31	" 27-	29.88	29.96	30.03	29.97	W. by S.	W. by S.	W. by S.	Snow	Sleet	Fair
3,	" 34	" 27	" 32	" 30.5	30.20	30.24	30.34	30.26	W.	W.	W.	Fair	Fair	Fair
4,	" 13	" 25	" 29	" 19-	30.43	30.22	29.67	30.11	N. W.	N. W.	W.	Fair	Snow	Snow
5,	" 33	" 36	" 33	" 34.5	29.33	29.41	29.60	29.45	W. S. W.	W.	W.	Rain	Fair	Fair
6,	" 28	" 35	" 27	" 31.5	29.95	29.97	29.93	29.95	W.	W.	W. by N.	Fair	Fair	Fair
7,	" 22	" 34	" 15	" 28-	29.47	29.21	29.03	29.23	S. E.	S. E.	S. E.	Fair	Rain	Snow
8,	- 1	" 11	" 11	" 5-	29.64	29.82	30.02	29.83	W.	W.	W.	Fair	Fair	Fair
9,	+ 11	" 19	" 16	" 15-	30.20	30.12	30.10	30.14	W.	W. by N.	W.	Fair	Fair	Snow
10,	" 10	" 15	" 10	" 12.5	30.21	30.23	30.17	30.20	W.	W.	W.	Fair	Fair	Fair
11,	" 9	" 16	" 13	" 12.5	30.04	29.93	29.87	29.95	W.	W.	W.	Foggy	Fair	Snow
12,	" 5	" 14	" 9	" 9.5	30.16	30.29	30.29	30.25	W.	W.	W.	Fair	Fair	Fair
13,	" 15	" 23	" 17	" 19-	30.31	30.08	29.90	30.11	W. by S.	W. by S.	W. S. W.	Fair	Fair	Fair
14,	" 21	" 32	" 30	" 26.5	29.82	29.72	29.89	29.81	S.	S.	S. W.	Cloudy	Fair	Fair
15,	" 12	" 11	" 22	" 11.5	30.00	29.65	29.46	29.70	N. E.	N E by N	N E by N	Fair	Snow	Sleet
16,	" 23	" 30	" 6	" 26.5	29.38	29.55	29.07	29.67	N. E.	N. W.	N. W.	Rain	Fair	Fair
17,	- 9	- 0	- 7	- 4.5	30.49	30.50	30.43	30.47	N. W.	N. W.	N. W.	Fair	Fair	Fair
18,	" 10	+ 4	+ 10	+ 7-	30.03	29.74	29.58	29.78	N. W.	N. W.	N. W.	Fair	Snow	Fair
19,	+ 8	" 8	0	" 8-	29.85	30.07	30.30	30.07	N. W.	N. W.	N. W.	Fair	Fair	Fair
20,	- 6	" 14	+ 7	" 4-	30.23	30.38	30.22	30.28	S. W.	S. W.	W. S. W.	Fair	Fair	Fair
21,	" 2	" 9	- 8	" 3.5	30.08	30.01	30.10	30.06	N. E.	N. E.	W.	Fair	Fair	Fair
22,	" 8	" 7	+ 7	- 0.5	30.10	30.06	29.94	30.03	W.	W.	W.	Fair	Fair	Cloudy
23,	+ 11	" 26	" 16	+ 18.5	29.62	29.55	29.80	29.66	S.	S. W.	S W by W	Snow	Fair	Fair
24,	" 20	" 21	" 10	" 20.5	29.82	29.87	30.18	29.96	W.	W. N. W.	N. W.	Snow	Fair	Fair
25,	" 5	" 11	- 0	" 8-	30.33	30.42	30.40	30.40	N. W.	N. W.	N. W.	Fair	Fair	Fair
26,	- 2	" 10	+ 12	- 4-	30.16	30.11	29.96	30.08	N. by W.	N. E.	N. E.	Fair	Snow	Snow
27,	+ 1	" 6	- 4	+ 3.5	29.85	30.00	30.13	29.99	W.	W.	W.	Fair	Fair	Fair
28,	- 10	" 9	0	- 0.5	30.22	30.17	30.08	30.16	W.	W.	W.	Fair	Fair	Fair
29,	+ 7	" 10	+ 10	+ 8.5	30.10	29.95	29.57	29.87	W.	W. N. W.	W. N. W.	Fair	Snow	Snow
30,	" 14	" 21	" 2	" 17.5	29.33	29.44	29.70	29.49	N. E.	N.	N.	Snow	Fair	Fair
31,	- 7	" 7	- 7	- 0-	29.90	29.89	29.91	29.90	W. N. W.	W. by N.	W.	Fair	Fair	Fair

THERM. } Max. Temp., +36° on the 5th.
 } Min. " -10° " 18th and 28th.
 Mean of the Month, +13° 5.

BAROMETER, { Maximum, 30.50 Inches on the 17th;
 } Minimum, 29.08 " " 7th;
 Mean of Month, 29.96 Inches.

TO MEDICAL STUDENTS.

A Gentleman who has had extensive experience in preparing **MEDICAL STUDENTS** for their **CLASSICAL EXAMINATION**, at the various Medical Boards in Great Britain, being now resident in this City, offers his services during the ensuing winter to such students as may be desirous of availing themselves of the opportunity of augmenting their knowledge of Latinity.

Should sufficient applicants present themselves in the first week of November, a class will be formed to meet at an hour that will not interfere with any of the Lectures.

Cards of Address, &c., may be obtained at the **GENERAL HOSPITAL, of the House Surgeon;** **M'GILL COLLEGE, of the Demonstrator of Anatomy;** **SCHOOL OF MEDICINE, of do. do.** And at the **OLD MEDICAL HALL, Notre Dame Street.** Montreal, October 26, 1846.

**UNIVERSITY OF M'GILL COLLEGE,
MONTREAL.**

THE CAPUT of the COLLEGE having this day received, through the Principal, an Official Communication of the Confirmation by Her Majesty of the **STATUTES** of the COLLEGE, avails itself of the earliest opportunity of announcing the **COURSE** of **LECTURES** to be delivered in the College during the Current Term:—

On Classical Literature—By the Rev. W. T. LEACH, A.M., Professor.

On Mathematics and Natural Philosophy—By ED- MUND A. MEREDITH, LL.B., (F.C.D.) Principal of the College.

On History—By the Rev. JOSEPH ABBOTT, A.M

On French Literature and the French Language—By LEON D. MONTIER, Esquire.

Fees, £3 6s. 8d. per Term, or £10 a-year.—Board, including Fuel and Candle, £3 5s. a-month.

J. ABBOTT, A.M.,

Sept. 21, 1846.

Secretary.

**SCHOOL OF MEDICINE AND SURGERY,
MONTREAL.**

THE LECTURES of this "School" will commence on the First Monday in November, 1846, and will continue till the 1st of May, 1847.

A *Concours*, for the purpose of **ELECTING LECTURERS** to **CHAIRS** of **INSTITUTES** of **MEDICINE, MEDICAL JURISPRUDENCE** and **BOTANY**, will be held at the School of Medicine and Surgery, on Saturday, 28th November, 1846, at Ten o'clock, A.M.

And, on the 30th November, (Monday following,) another *Concours* will be held, for the **ELECTION** of a **SECOND DEMONSTRATOR** of **ANATOMY**. Candidates must understand the French and English Languages.

Any information in relation to the Institution can be obtained by applying to the Secretary, Dr. SUTHERLAND, Little St. James Street.

Montreal, September 29, 1846.

**SURGICAL INSTRUMENTS, AND DRUGGISTS
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