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[No. 3

OBSERVATIONS ON THE NATURE AND TREATMENT OF VARIOUS DISEASES,

BY ROBERT L. MACDONNELL, M.D.,

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No. I.—ON THE UTILITY OF ELECTRO-GALVANISM IN AMENORRHEA AND DYSMENORRHEA.

Although Electro-galvanism has been for some years past employed in Europe,* in cases of amenorrhœa and dysmenorrhœa, yet I believe I am correct in stating that it was never used in Montreal until introduced by me, within the last year.

Having been requested to meet a physician of this city, in consultation in a case in which I recommended a trial of electro-galvanism, in order, if possible, to induce a return of the catamenia, he mentioned that he had no experience of the remedy, or of the class of cases in which it should be employed, or of the method of employing it; and, as many of my readers may be similarly circumstanced, I have thought that a few practical remarks on the utility of this agent, and on the peculiar cases to which it is suited, would not be useless or uninteresting.

In an early number of Guy's Hospital Reports, Dr. Golding Bird published a paper shewing the value of electro-galvanism in various diseases; and, amongst others, in derangements of the uterus. Since then, it has been extensively employed in Dublin by Drs. Graves and Stokes, and other eminent physicians; and whilst acting as clinical assistant to those gentlemen, as well as in my own practice, both in Dublin and in this city, I have had many opportunities of witnessing its good effects in these troublesome diseases.

The practitioner often meets with instances where females have suffered for months, and even years, from complete arrest of the menses, or from their being secreted scantily, and with difficulty and pain, or where the discharge comes on abundantly and without pain at one time, whilst at the next period the patient suffers excessively, and scarcely any discharge presents itself; and in

another class, severe dysmenorrhœa has existed for years before the physician is consulted. Now, there are not, perhaps, any diseases in which the ordinary courses of treatment are more unsuccessful; for long before we are consulted, the usual effects of such derangements have become well marked upon the constitution; the system has become reduced and debilitated; and the ordinary features of chlorosis or hysteria have become well established. In such cases, our treatment, however judicious, often fails completely; and we not unfrequently meet with disappointment, from the difficulty of enforcing on our patients an exact observance of our directions as to medicines, exercise, diet, &c. In other cases, the constitutional symptoms have preceded the uterine derangements, but in both, it often happens, that the disease becomes more and more confirmed; and little or no benefit being derived from the advice of the regular practitioner, the patient resorts to quack medicines and nostrums; and, after a waste of time, of money, and of health, the physician is again applied to. It is under such circumstances, and in such cases, that electro-galvanism acts with the greatest success; inducing a return of the menses, when arrested, or producing an easy and abundant secretion of them, in those cases where this process has been inefficiently and painfully performed, perhaps, for many years previous; and this change is soon followed by an amelioration of all the distressing symptoms under which the patient has laboured.

The best and most convenient method of applying electro-galvanism, is by means of the machine sold at New York, under the name of the "Vibrating Magnetic Machine," or with the apparatus made by Clarke of London, for a similar purpose. Each of these machines is accompanied by a short treatise explanatory of the principles upon which it is constructed, and containing directions for its employment. The former instrument will be found cheaper, more convenient, and less likely to go out of repair than the latter.

Two insulated wires are connected to the apparatus, one being attached to each pole; at the free extremities of these wires are two platina buttons. When the machine is in operation, one of these buttons is applied by the physician to the lower portion of the spinal column, corresponding to the point of exit of the sacral

* See Segaud, Lafond, *De l'Electricité Medicale*, and Manjuy, *Traité de l'Electricité Medicale*.

nerves, and the other button is applied by the patient herself, or by a female attendant, immediately over the os pubis. In order to protect the patient from the inconvenience of getting the shock through the hand which holds the button, a thick glove should be worn.

Having desired that one button be kept firmly pressed upon the os pubis, the physician commences, by passing the other button along the spine, from the occiput to the os coccygis. After this has been done slowly four or five times, the button is then kept for five or six minutes immediately over the sacrum, and the electricity is thus passed in an uninterrupted current through the uterus. It is by no means necessary to put our patient to great torture by increasing the strength of the shocks; more benefit will be derived from an uninterrupted and steady transmission of a moderate quantity of electro-galvanism, than by occasional shocks of great intensity. The current may also be sent transversely through the pelvis, by placing a button on each hip, above the great trochanter.

Of course, it is needless to add that, with ordinary care, there need not be the least exposure of the person of the patient, for all the necessary manœuvres can be completed without removing the bed-clothes—the patient lying on her side, with her back turned to the physician. In some cases it may be useful to carry the current more directly through the uterus, which can be easily done by applying one button to the sacrum, and having the other wire attached to a curved brass rod, which can be introduced into the vagina so as to touch the cervix uteri. This is the plan I have advised a medical friend to adopt in the case of his wife, who has for years been a martyr to dysmenorrhœa.

It is not proposed to employ electro-galvanism to the exclusion of those remedies, whose efficacy in the condition of the system accompanying derangements of the uterus is so well established; on the contrary, they should always be employed during the intervals between the menstrual periods.

My usual practice is, to administer a combination of sulphate of quinine, and sulphate of iron, in small quantities—for in these cases both the stomach and head are easily deranged by large doses—and if (as often happens), the patient be subject to constipation, I combine with the foregoing, one ounce of the sulphate of magnesia to eight ounces of water. This I find to be the best and most pleasant way of combining, in the same mixture, the properties of a tonic, a chalybeate, and a mild aperient.*

* The following is the usual mode in which I exhibit these remedies:—

℞ Sulph: Quinina gr. iv. vel vi; Sulph: Ferri gr. viii.; Sulph: Magnesia ʒi; Acid Sulph: dil. m. x. Aquæ ʒ viii.; of this half an ounce four times a-day.

In Graves's "Clinical Medicine," the reader will find the history of some cases that fell under my own observation, of which I shall here introduce a few particulars:—

1. *Anne Cummins*, admitted with ptosis and amaurosis of right eye, and in a lesser degree of the left eye. She had also amenorrhœa of ten months' standing. Electro-magnetism was employed for the relief of the two former symptoms: and on the 20th, as well as for a short time on the 18th, I also applied electricity to the region of the uterus, and on that evening she menstruated. This action continued on the 21st and 22d, and the fluid was natural both as to colour and quality. In this case not a grain of medicine of an emmenagogue nature was administered.

2. At page 423, the details of Cunninghame's case, are given. She was of an extremely hysterical habit, and was frequently an inmate of the hospital during the period of my connexion with that institution—June 15, 1842. She laboured under an attack of hysterical paralysis of one leg, for which electricity was recommended. Whilst this remedy was being applied to the region of the sacrum, the catamenia returned, having been suppressed for the thirteen months previous.

3. *Carroll*—Suppression of menses for 18 months. From 16th to 19th August, applied secondary electricity according to my usual practice. 20th, Learned that she had menstruated on the previous evening after I had left her.

4. *Smith*—Irregularity of catamenia for many months. 23d August—Yesterday, the 22d, she had a trifling manifestation of the catamenia, being then six weeks since their last appearance. Electro-magnetism was applied; and for the next two days the discharge was more abundant than it had ever been.

Since my arrival in this city, I have employed this agent with the greatest success in numerous cases of amenorrhœa and dysmenorrhœa, the details of which would be highly instructive, did space allow of their insertion. The four following examples, however, so clearly and satisfactorily demonstrate the advantage to be derived from this remedy in these diseases, that I shall make no apology for laying them before the reader:—

I. An unmarried lady, aged 25, of dark complexion, and spare habit, consulted me, on March 20th, for severe palpitations, accompanied with pain in the cardiac region, headache, dyspnœa, and pains and weight in the lumbar region. She had commenced to menstruate at the age of seventeen; and, for two years afterwards, the catamenia were quite regular, and she enjoyed good health; but for the last five years they were very irregular in their returns, and were accompanied by excess-

sive pains in the uterine and lumbar regions. For the last ten months she had not observed any discharge. As the period when the menses ought to appear was expected in a few days, I immediately commenced the use of electricity; and, after two applications, the discharge returned, and continued to flow for four days. She has since menstruated regularly, and her general health is quite restored.

II. *A. B.*, a widow aged about 33, of sallow complexion, consulted me many months ago, for suppression of the menses. Since the death of her husband, which took place 7 years ago, the catamenia have been very irregular, always secreted with pain; and for many months past, has been completely absent. She had been at various times under medical treatment for these symptoms, but never obtained any relief. When she applied to me, I recommended electricity, but she begged of me to try something else first; and, should it not succeed, that she would then consent to this remedy being employed. She was placed under the ordinary treatment; but, as the discharge did not return at the end of three months, she again consulted me on the 1st of June, two days before the menstrual period was expected. After two applications of twenty minutes' duration each day, the catamenia came on profusely, and lasted for five days. I have no doubt whatever that henceforth they will appear spontaneously.

III. About the end of May, I was consulted by Dr. Evans of Richmond, C. W., concerning a young lady, in whose case he was deeply interested. It appeared that about seven months previously she had had a violent attack of pleuro-pneumonia, for which she was very actively treated, both by general and local blood-letting. After remaining in a precarious state for some time, she recovered; but continued in a very delicate condition, with all the symptoms of anæmia. The menses had not appeared for four or five months previous to my seeing her, although she had been most judiciously treated, with a view to their restoration. I advised electro-galvanism to be employed, to which both Dr Evans and his patient consented; and as the menstrual period was not expected for ten days, I recommended her to spend the interval with some friends in Quebec, and to return to Montreal on the 9th of June. Accordingly, on the 9th I applied electro-galvanism in the way I have already described. The menses almost immediately appeared, and continued to flow from the 9th to the 16th. She says she never had such an abundant discharge before, nor did she ever pass a menstrual period so free from pain and uneasiness. In fact, whilst menstruating, she took more exercise than she was ever before capable of doing. This case affords an apt illustration of what I have stated above—viz., that electro-galvanism

is equally suited to cases where amenorrhœa has followed as a consequence of severe derangements of the general health.

IV. The following case more convincingly exhibits the value of this remedy than any I have yet adduced, or, indeed, than any I have ever seen:—A lady, aged 32, of pale complexion, and delicate constitution, consulted me in the middle of March, for severe constitutional symptoms, produced by long-continued derangement of the uterine functions. She belongs to a family in whom this tendency is hereditary. Her mother suffered from dysmenorrhœa during the whole period of her menstruation; one sister died at the age of twenty, without having ever menstruated, although for four or five years she was under the able treatment of Dr. Robert Nelson, whose skill and talents were so well appreciated in this city, and she herself had, for six or seven years, the benefit of that gentleman's advice. At the age of fourteen the menses appeared, and continued regular in their returns for three years or so. She was usually unwell on the 20th of each month; but, from the first menstruation up to the present time, the discharge was so exceedingly scanty as never to have required the use of a napkin. After the first three years, up to the present, the discharge has hardly ever been so abundant as to leave the least trace on her linen; and were it not that at such periods pains of an agonizing character, referred to the lower portion of the abdomen, tormented her, she could not have known that the catamenia were being secreted. On these occasions she was always obliged to make strong pressure over the pubis, and frequently one of her female relatives has had to sit or lie over the lower portion of the stomach to mitigate her suffering. These attacks usually lasted for three days, during which she used to have almost incessant vomiting. For the last four years, intervals of five, seven, ten, and even thirteen weeks have occurred between each menstruation.

Having learned, from personal experience, that in cases exhibiting such irregularity as the foregoing one, it is useless to employ electricity, except as nearly as possible at that part of the month on which the discharge used to appear when the function was in a healthy state, I determined to give it a fair trial on the 18th of April. Accordingly, on that day, and for the next four, it was administered, but did not produce any discharge; the period, however, was passed over free from all uneasiness. She was then placed on the chalybeate and tonic mixture, above mentioned, and on the 19th of May I again submitted her to the influence of the remedy. For two days no discharge appeared, but on the third it came on most abundantly, and continued to flow for seven days, requiring the frequent change

of napkins each day; and during the whole of this time she enjoyed excellent health, was without the least pain, and was able to walk three or four miles daily, and to nurse-tend a sister, who was attacked with severe rheumatic fever. This change was followed by an almost instantaneous improvement in her general health, and she was soon after enabled to undergo the fatigues of a long journey, which she bore remarkably well.

The foregoing cases I have given in detail, because they exhibit, in a convincing manner, the great utility of electro-galvanism where all other remedies had failed; and (as in the last one) where the disease was of such duration as to afford *but little prospect of success from medical treatment*. I may be told that many measures, such as sea-bathing, horse exercise, travelling, residence in the country, change of scene, enjoyment of society, &c., have frequently produced as speedy cures in cases equally obstinate. True; but how many times are we consulted by females whose avocations and circumstances do not admit of such remedies being employed? In this country, as elsewhere, the object of the physician should be, to avail himself of all means which, without abandoning *in toto* the powerful adjuvants just mentioned, will render him and his patient, so to speak, independent of them; and; in the class of diseases now under consideration, electro-galvanism comes in opportunely to our assistance.

It must be borne in mind that electro-galvanism acts in these diseases, not specifically, but just as it does in all other cases where there is a manifest *depression* of nervous power and vascular action. Our general experience of the remedy clearly shews that it is not in instances of exalted nervous power, or of great arterial excitement, that it acts beneficially; but, on the contrary, its powers are best exemplified where these functions are, as it were, below the ordinary standard. Bearing this fact in mind, we are not likely to be misled as to the cases of amenorrhœa and dysmenorrhœa, in which it should be employed, with the greatest prospect of success; nor shall we recommend it in those instances where the above indications are absent, to the exclusion of active remedies, which both the pathology of the disease, and our own experience, point out as the most appropriate.

Montreal, June 20, 1846.

CASE OF CONCEALED DELIVERY.

By S. C. SEWELL, M. D., Lecturer on Materia Medica, University McGill College.

On the 16th November, 1845, at a quarter past one, p.m., I was called to Mr. K.'s, to see his servant, Bridget Cloone, ætat. 40, who was said to be suffering from colic and pain in the back. Laying my hand on the

abdomen, I perceived that she was about seven or eight months pregnant. On my charging her with the fact, she denied it stoutly, said she had menstruated two months before, and finally, finding that she made no impression on my opinion, she declared, in the most solemn manner, "that whatever was inside of her, it was no child." The reason for this statement will appear presently. I had her removed to the University Lying-in Hospital, whither I went in an hour after, and finding that the physician in ordinary had not arrived, at the matron's request I examined her, per vaginam, and found the os uteri dilated and the membranes protruding; presently I detected what appeared to be a cord, lying coiled in the upper part of the vagina, and on pulling at it, a free extremity came down, but not to the os externum. There were no clots of blood in the vagina. At half-past five I returned, and found Dr. McCulloch in attendance; the child just being born by the feet, and the woman still persisting that there was no child. The child was feeble, but not at all exsanguined. It survived a few hours. To the placenta was attached two cords; that of the first child had evidently been divided with scissors from the appearance of the cut surface. Information was given at the police-office, that a new born child had been concealed, for the woman denied that any previous birth had taken place. On searching the bed-room which she had occupied at her master's house, the bed bore evident marks of a delivery having taken place, and on searching her trunk, the body of a male child was found, underneath the clothes, which had been very carefully smoothed over it. Care was taken not to disturb the position of the limbs, and the body was removed to the Police Station House.

An inquest was held on the following day, when Dr. McCulloch and I were directed to perform the autopsy, of which the following is the result:—The body was fifteen inches long, and weighed two pounds fifteen ozs. avoirdupoise. The body was not exsanguined; there was no fracture of the skull; the conjunctiva was intensely injected; the cornea hazy, and pupil open.

The body was found on its right side in the box, and was deposited on the same side in the station house; in consequence, livid patches were observed on that side from the gravitation of the blood.

External Examination.—Several marks of injury were found as follows:—One from the right nipple to the point of the shoulder, half an inch broad; one from the right side of hyoid bone to mastoid process of right temporal bone; one a little lower, and to the outside, which terminated at the back of the neck; the fourth commencing to the outside again, went to the middle of the superior costa of the scapula, the hands were turned up to the head, the right one to the right ear. The nails

were formed. The umbilical cord had been divided nine inches from the body, evidently with scissors, and there was no ligature on it. Meconium was protruding from the anus; the testicles had descended; the thighs were flexed on the abdomen, and the legs on the thighs.

Internal Examination.—The marks of injury before referred to, were cut into, and the cellular tissue underneath was found to be red with extravasated blood. An incision was made through the lower lip, and down to the epigastrium, in the mesial line. On dividing the lower lip, the tongue was found protruded more than a line beyond the gums. On opening the thorax, the following observations were made:—The apex of the diaphragm was opposite the fifth rib; the lateral portions were well descended; the lungs were of a uniform bright scarlet color, occupying the lateral portions of the thorax, and touching the diaphragm below, but not filling the pleural cavities entirely. The heart and great vessels were nearly in the mesial line, and the cavity of the entire thorax was large for the size of the child; the lungs crepitated on pressure; the lungs, heart, and thymus gland were then removed, and, on being put into water, floated; crepitation occurred under the scalpel; a portion of lung was squeezed under water, and bubbles issued from every part of the cut surface; the same was observed on squeezing a portion in air; nearly half of each lung was removed, and the remainder, with the heart and thymus still attached, was cast into water, when the mass again floated; portions of lung floated in water; the cavities of the heart contained dark blood, slightly coagulated; foramen ovale was closed, but not obliterated.

INFERENCES.

- 1st.—The child had breathed freely.
- 2d.—The marks of injury on the right breast and neck were inflicted during life.
- 3d.—They were, in all probability, occasioned by the left hand of an adult grasping the neck of the infant.
- 4th.—The protrusion of the tongue, and position of the hands, are, probably, referrible to strangulation.
- 5th.—Death was not caused by hæmorrhage from the cord.
- 6th.—The child was between seven and eight months of utero-gestation.

The rest of the evidence went to show, that Bridget Cloone had been a widow for some years; that she had carefully concealed her pregnancy; that she had taken powerful emmenagogue medicines, prescribed by an irregular practitioner, up to the day of delivery, and that she was seen, half an hour before my arrival, to get out of bed, stand by its side, take a pair of scissors from under the pillow, and cut something under the bedclothes.

The coroner's jury brought in a verdict of "Wilful

Murder." The bill of indictment founded thereon was thrown out by the Grand Jury. She was then indicted for concealing the birth of an illegitimate child, convicted, and sentenced to six months' imprisonment.

The above is an exceedingly important case in the annals of Medical Jurisprudence; and cases of the kind are very rare. Under the hope of escaping from the consequences incident to an actual infanticide, of which there is the strongest probability, this woman *persisted to the last that she was not pregnant*, little anticipating that a second child was to furnish its quota of evidence of the birth of a former one a few hours previously. The case furnishes a striking proof of the fact, that a woman may be delivered of one child, of which she may criminally dispose, for the purpose of concealing its birth, and may be afterwards delivered of a second, the life of which may be preserved.

GEOLOGICAL SURVEY OF CANADA.

Report of Progress for the Year 1844. By William Logan, Provincial Geologist.—Lovell & Gibson, 1846.

The provincial geologist has prefaced his report of progress for the year 1844 with a descriptive geographical sketch of the field of his observations. This is a very acceptable contribution to our knowledge of that district, of which no previous topographical survey had been made. An examination of this nature was obviously necessary before any account could be given of the geological structure of the country; and this obstruction, if not to the prosecution, certainly to the expression and recording of his specific labours, Mr. Logan seems to have surmounted with equal ability and success. The coincidence of the result of his admeasurements, with that of Capt. Bayfield's, and Deputy-Surveyor-General Bouchette's, is a striking proof of the accuracy of his science in this department of his investigations.

From the mouth of the Chat to the Bay of Chaleur, the majestic mountains of Notre Dame required to be passed over; and these, as they formed the primary object of the geological survey, become, likewise, interesting parts of the physical geography of the country, when drawn with the accurate, and not ungraceful outline which we have in the following sketch:—

"From the highest summit we visited, the panorama displayed was of the grandest description. In the northern half of the circle, the waters of the St. Lawrence, dotted with its ships and fishing boats, spread out to the right and to the left as far as the eye could reach. On its northern shore, immediately in front, unaided vision could plainly distinguish the lighthouse of the Pointe des Monts, some fifty miles off, from which the granite hills, rising immediately behind it in the interior, gradually sunk below the horizon as they receded from us, following them down the expanding gulph, to a point where we thought we could discern the Island of Anticosti, one

hundred miles away in the mist of the distance, while at our feet were arranged in parallel lines the ridges and valleys of the lower land between us and the river. To the eastward, a confusion of mountains and ravines belonging to the Notre Dame Range, filled up several degrees of the circle, and one summit, which exhibited a patch of snow, we supposed might be higher than the point we stood upon. Many of the peaks were bare; and, as they retired one behind another, and occupied a smaller angle in the perspective, it became difficult to distinguish those of the Notre Dame from such as appertained to other ranges. Turning southward, a sea of parallel undulating ridges occupied the picture, the more distant of which we conceived might present a table land, with a few marked points rising in cones and domes; and through one gap, which probably was the valley of some south flowing river, we distinguished a faint blue horizontal line, which we fancied might be in New Brunswick. Prominent points became still fewer, veering westward, until the horizon was again interrupted in that direction by a well defined outline of a not very distant part of the range from which we looked.

“The highest summits within our view were generally bare rocks. Those next in the scale were crowned with sturdy dwarf spruce trees, many of them not five feet high, but springing up so close together that their branches interlocking, rendered it very difficult to make way among them. On those still lower, spruce became mingled with white birch, and the size of the trees gradually augmented in proportion to the decrease of elevation. One feature in the vegetation high up in the hills, that struck us forcibly, and gave us much satisfaction after confinement in the forest below, was the great extent of open glade that appeared on all sides but the north. Wide slopes on the east, the south, and the west, were carpeted with the most luxuriant growth and abundant specific diversity of ferns, from which clumps of spruce, or of white birch, or of both mingled, started up here and there, giving the hills occasionally almost the character of park scenery, as if art had arranged the distribution with a view to ornament, and often producing, in combination with peaks, ravines, and a distant horizon, landscapes of a very pleasing description.”

There are several things that serve to lighten the toils of the practical geologist, and to form a sort of compensation for them, abstracting altogether from their value as a source of national wealth, or a professional occupation. It is neither the hope of discoveries, nor the expectation of any direct utility, that inspires the enthusiasm which is prophetic, and, at the same time, productive of success. Sir Walter Raleigh certainly made mention only of a subordinate motive, when he said, “Neither am I so far in love with that lodging, watching, care, peril, diseases, bad fare, and many other mischiefs that accompany these voyages, as to woo myself again into any of them; were I not assured that the sun covereth not so much riches in any part of the earth.” He was, in truth, only thus betraying himself by “an imagination.” That instinctive love of the miraculous and the beautiful, that unconquerable delight which many persons have in

beholding the pictures, and dwelling in the palaces of nature—these, as is evident from his rapturous descriptions, were undoubtedly a main source of his activity and adventure. There are no pursuits that better gratify these tastes than that of geology. Besides, that they are health and strength to the geologist, he is led by them into the rarest scenes which the surface of the globe presents—scenes which no ordinary tourist ever thinks of visiting, and that usually lie beyond the province of human duties. Many a fairy spot in the bending arm of a stream—many a glorious panorama it is his happiness to witness, as

High o'er the hill, and low adown the dale,
He wanders many a wood, and measures many a vale.

What, indeed, constitutes the charm of the practical study of geology, and often inspires the students or cultivators of it with extraordinary enthusiasm, is the gratifying of the many sensibilities which have a necessary affinity with it—“*eternim omnes artes habent quoddam commune vinculum, et quasi cognatione quadam inter se continentur.*”

Conglomerate Limestone, Pillar Sandstones, and Gneissic Shales.

The whole of the deposits given amount to about 1140 feet in thickness, and the distinguishing features they present are the bands of conglomerate limestone, and the bituminous mineral so much resembling coal. This is found not only in the septa of the cherty nodules which have been mentioned, but also in many small cracks across the strata, and in more parts than one of the vertical thickness. A similar mineral, in an analogous position, is found in the rocks at Point Levi and Quebec, and in the museum of the Quebec Natural History Society a block of it, procured in the neighborhood, containing about a cubic foot, is preserved. Some have been inclined to suppose that it might indicate the proximity of workable coal, and indeed I have been asked whether a mine upon it, in a position which I have not yet seen, but where according to information received, a cart load of it has been obtained, would be likely to be successful. Now none of the material where it has come before me in situ, bears any analogy in the mode of its occurrence to workable coal. This is always found in extensive by continuous beds conformable with the stratification; whereas the mineral in question occurs in cracks cutting the strata across for greater or less distances. It is true that where faults or dislocations exist among coal seams, there is often met with running across the stratification what by Scotch miners is termed a *vise*, and by Welsh, a *leader* of coal, which in general is a thin, confused, irregular interrupted black more or less carbonaceous sheet, conducting up or down, as the case may be, in the plane of dislocation, from the termination of a coal-bed on one side to that on the other; and there is no doubt it is the result of the grinding of the terminal edges of the strata against one another, when the slip producing the dislocation occurred. Without a slip or displacement, therefore, no leader would be found, and none in any case would hold true coaly mat-

ter extending beyond the distance between the separated edges of the coal-bed. Now in the case of the bituminous mineral, the cracks in which it occurs are, in many instances, unaccompanied by any displacement of the strata, and in others where the extent of the dislocation (that is the upthrow or downthrow, as it is called) is visible, no layer holding any of it occurs among the beds. Independent of all this, the formation in which the mineral is found, is an inferior member of a group of rocks, whose place is in all probability a very considerable distance below the position of the true workable coal-bearing measures, and we are, therefore, not warranted in expecting coal seams to exist in it. The rock is supposed to be the equivalent of a part of the Hudson River Group of the New York geologists.

Although the chain of Notre Dame Mountains runs parallel with the general strike of the strata between them and the St. Lawrence, and the rocks composing them have not been classified separately, I do not recognize them as similar to any mass met with on the coast, notwithstanding the direction they take ought to bring them out obliquely upon it towards the top of the group. Should further investigation prove that they really belong to the group; it will result that their mineral condition must have been greatly changed; but a larger number of facts must be collected to authorize any conclusion respecting them. In all the parts visited they present a metamorphic aspect. Their general colour is pale green, very much resembling the green of epidote. They are hard, close textured and silicious, often presenting the appearance of a very fine-grained sandstone, in which the beds would be nearly obliterated were it not for fine lines of discoloration. Such was their general character on the summits of the highest peaks, and at six localities in our progress up the gorge of the Chat; but loose angular fragments in the stream shewed that the green is sometimes mottled with red jaspery patches, and that some masses display a fibrous or a-bestiform structure, breaking into hard long pointed splinters, while others, possessing something the character of mica schist, split into silicious plates, whose separation is facilitated by the existence of flakes of mica, running in parallel layers.

On the summit of the Old Man Mountain, which consists of the close grained quality of green silicious rock first mentioned, we found the needle of our compass deflected 4° from the ordinary magnetic meridian of the neighbourhood, which is $22^{\circ} 30'$ west of true north. The irregularity may have been occasioned by the vicinity of some vein of the magnetic oxide of iron, but so much of the surface was concealed by moss and trees that our search for it proved unavailing.

Gaspe Limestones and Calcareous Shales.

In the neighbourhood of Gaspe Bay, four mineral springs exist in the rocks of the formation under description. Two of them are bituminous, and two of them are sulphurous. One of the bituminous springs is situated on the south side of the St. John River, about a mile and a half above Douglstown. The liquid is petroleum, and it oozes from the mud and shingle of the beach. On digging small pits a black earthy deposit two or three inches thick is perceived lying on bluish gray clay, and it is from this black earthy deposit that the liquid

was seen to exude, and none of it from the clay; though there can be no doubt there must be some orifice in the clay to allow the communication with the deposit resting on it. The liquid collects in the pits in a thin film on the surface of the water entering with it; and from twelve pits, I with difficulty obtained half a pint in four hours. When the beach is covered with water, intermittent black blotches are seen to rise through it in several spots, which, floating on the surface, are carried by the wind or the tide to the first impediment offered by any stick or collection of seaweed rising above its level, and occasionally a small supply of it is found thus embayed. Localities yielding the liquid are said to exist at intervals all the way up to the lower extremity of the first marsh island, a distance of about three-quarters of a mile, but though I examined about half of it, I did not detect any of them.

The position of the other petroleum spring is about two hundred yards up a small fork of the Silver Brook, which is a tributary of the South-west Arm, falling into it about six or seven miles above Gaspe Basin. The exact orifice from which the bituminous liquid issues is not easily determined; but it collects on the surface of the water, wherever impediments cause a quiet pool, in the form of a thick green scum, which can be taken with a spoon. A copious spring of pure water rises up at the spot, and though none of the petroleum was visible on the surface of the spring at its issue, it is not improbable some connexion may exist between their sources, as no trace of it was found higher up the brook. About a pint of the bituminous liquid was collected in half an hour, but this was from an accumulation found covering a small pool, on reaching the place the odour of which could be perceived for one hundred yards around.

PRACTICE OF MEDICINE AND PATHOLOGY.

ON THE TREATMENT OF ACUTE RHEUMATISM.

By P. M. LATHER, M. D.

It needs little else than a perusal of the instructive volume from which the following observations are extracted, to convince us of the immense importance of a prompt and scientific treatment of acute rheumatism. On this point the remarks of the author are worthy of implicit confidence, being the result of labours conducted under circumstances the most favourable to the eliciting of truth; namely, those of extensive experience united to and guided by high scientific acquirements. In his tenth lecture, the author enters upon the consideration of the several indications towards which the curative endeavours of physicians are generally directed, and first of *blood-letting*. The power of this remedy, he observes, carried to its full extent, is in many cases undoubted, the entire disease being rapidly subdued by it, but in other cases it is far from being efficacious, or may even prove positively injurious. Upon the whole, therefore, he decides that the practice which proposes to cure rheumatism, at any cost of blood which may be needed, is an uncertain and a dangerous one.

Still, he observes, venesection is among the remedies of acute rheumatism, not needful in all cases, but expedient in many. "It is expedient to abate vascular action when it is excessive, when the patient is robust and young, and the disease has arisen accidentally in a healthy constitution. * * * But in the young, robust, and previously healthy, where vascular action is not excessive, and in the old, the feeble, and the previously valentudinary, even when it is, venesection is best omitted. There are other remedies which, without the help of bleeding, may be trusted to for its safe and effectual cure.

Summarily, then, I would venture to say of venesection em-

ployed under the most suitable conditions, and in the most suitable measure, that it is to be trusted, rather as preparatory and auxiliary to other remedies, than for its own exclusive power in acute rheumatism. It may often render the disease more curable by other means; but seldom cures the disease itself."

Of opium he says, that as the pain is often severe, "the dose must be large, and often repeated, which is to reach it and lessen it. In the severer cases, and when the whole treatment is left to the sole remedial power of opium, the measure and frequency of its dose must be enough to *subdue*, if it is to have a fair chance of tranquillizing."

Upon the whole he regards the indications of treatment founded upon the state of the nervous system as safer than that founded upon the state of the vascular, and he looks upon opium consequently as a better remedy than venesection, and to be preferred to it, if we are to follow one of the two indications singly, and to use one of the two remedies only.

The third or derivative plan of treatment which seeks to obtain from the abdominal viscera a large evacuation of their secretions is thus spoken of. "The mode of proceeding is thus: ten grains of calomel are given at night, and a draught of salts and senna in the morning; and the same are repeated night and morning, as long as they can be well borne, and continue to produce their effect. The evidence that they are well borne, is that they occasion no distress, and the desired effect is that they bring away dark or bilious evacuations. If they induce tenesmus, and stools of pure bile or mucus and blood, it is a sign that they are doing injury, and should be withheld." This practice, which is due to Dr. Chambers, is strongly commended by the author. "In three days there is often a signal mitigation of the symptoms; and in a week I have seen patients who have been carried helpless into the hospitals, and shrieking at the least touch or movement of their limbs, risen from their beds, and walking about the ward quite free from pain." And again, "if in the treatment of acute rheumatism you were to choose one indication, and abide by it, and to trust to one class of remedies and to one only, you would find more cases that admit of a ready cure by the method now described, than by either of the two former. You would find the aggregate of morbid actions and sufferings which constitute the disease, more surely reached and counteracted, and more quickly abolished by medicines operating upon the abdominal viscera only, than by those which influence either the bloodvessels only, or the nerves only. * * * It has appeared to me not only to bring the disease to a speedier conclusion, but to prepare the way for a more rapid convalescence than the other methods."

Having thus spoken separately of the three methods of treating acute rheumatism, by taking the state of the vascular system as the main indication, by regarding chiefly the condition of the nervous system, and thirdly, by producing a derivative effect upon the intestinal canal, the author proceeds to remark that although each may, in certain cases, be successful singly employed, that the most successful mode of treatment is a compound of the three: "For," he observes, "I believe that by the judicious use of opium you may spare blood, and by the judicious use of bleeding you may spare opium; that by calomel and purgatives properly administered you may make bleeding and opium less needful, and that by bleeding and opium discreetly employed you may leave less to be effected by calomel and purgatives."

Of colchicum, Dr. Latham remarks, that "single-handed it cannot be trusted for the cure of the severer cases, but it can in the milder, and I have so trusted it, but I do not recommend the practice. Colchicum given alone, has been slow, even in these milder cases, of making its curative impression. Many days have generally elapsed before it has produced any abatement of swelling and pain, of vascular action and fever; and then, not until it has begun to purge smartly and even painfully. Finding then that in the milder cases I had no fair chance of obtaining from it the virtue of a remedy without running some hazard of its acting as a poison, I considered it much too hazardous an experiment to commit the treatment of acute rheumatism to it, mainly or entirely, in the severer cases."

The customary modes of exhibiting colchicum, is to combine it as an auxiliary to other medicines. Dr. Latham objects to its being so employed, but reserves it for special emergencies, when he trusts to it with great confidence. His words are these: "When by venesection and by opium, and by calomel with purgatives, excess of vascular action, and fever and pain are abated but not entirely abolished; or when pain and swelling do not subside in proportion to the abatement of the vascular action, then I invoke

the aid of colchicum, and give twenty or five-and-twenty minims of the wine of the seeds of the root, twice or thrice a day, and I often find the disease proceed uninterruptedly to a cure."

The author likewise trusts to colchicum in cases of relapse. As he observes, he gives it without combination, so that there can be no hesitation in allowing that the beneficial effects are essentially due to its specific virtues, for the cure has occurred prior to the production of any irritation of the bowels.—*Ranking's Abstract.*

OBSTRUCTION OF THE BOWELS.

The object of the following case is to show the effects, apparently and probably really produced by electro-magnetism. The patient, a widow lady of about fifty-five years of age was taken, ten days previous to my seeing her, with bilious vomiting and intense pain in the bowels, accompanied by constipation. The pain ceased in three or four days, but the vomiting continued, and was now sero-raceous. She had been bled to faintness, had taken calomel until the gums were sore, had taken one drop of croton oil every hour, for several doses, besides various other cathartics, and repeated ordinary injections. I found her on the eleventh day; without fever, bowels somewhat tender, and vomiting fecal matter every hour or two. The injections before used had been given with a common syringe and allowed to come away immediately. I used the rectum tube and force pump, but could not throw up more than a quart at once, which, however, was compelled to be retained for half an hour, when it was suffered to pass, and after a little repeated. This was continued for six times, until the injection, which at first returned mixed with fecal matter, came away, after being kept up an hour, almost as clear as when given. The symptoms nearly the same; there was evident obstruction, and, probably, invagination of the small intestines. I now used Mr. Pike's electro-galvanic apparatus, applying a pole on either side of the abdomen; this was continued for twenty minutes, when there was evident commotion in the bowels, and in ten minutes more there was a regular evacuation, thin and small in quantity, but distinctly of a fecal character and the first in eleven days. The vomiting ceased entirely; two hours after, the battery was again used, and in twenty minutes produced a free operation. A dose of oil operated in the usual time, and the patient rapidly recovered. There was no doubt in my mind but the favourable change was produced by the electro-magnetism, though more probably by the successive shocks producing muscular action, rather than by any electro or magnetic agency.—*New York Journal of Medicine.*

[In the above instance we have another illustration of the value of Electro-galvanism as a remedial agent. A case of obstinate constipation, is at present in the Montreal General Hospital, in which I have frequently recommended a trial of electro-galvanism, and at one time, whilst the patient was under my care, I was going to employ this remedy, when, to the surprise of every one, the bowels became spontaneously relaxed, and five or six motions took place daily for at least a fortnight. Soon after, constipation became again established. I know of two cases where this dangerous malady yielded to the remedy; and I can also bear testimony to the statement of the author, that it is useful in stimulating the uterus to contraction in cases of powerful labour.

Shortly before my departure from Dublin, a case occurred

* I would remark that I always avoid the use of this machine during pregnancy. In one case, however, where I used it for chronic rheumatism, without being aware of the pregnancy, it apparently caused a miscarriage, which followed after about twelve or fifteen applications.

in the Lying-in-Hospital, in which the ordinary symptoms which call for the use of ergot, presented themselves. It was, however, determined to try electro-galvanism. One button was applied to the sacrum and a curved brass rod connected with the other wire was introduced into the vagina, so as to come in contact with the os uteri; the circle being thus completed the apparatus commenced to work, the pains which had long been suspended, were renewed, and in less than fifteen minutes a living child was expelled. The placenta, too, was in this case retained, and after the period allowed for its separation had elapsed, electro-galvanism was again applied, and with the happiest results; one or two contractions immediately followed, and the detachment of the afterbirth quickly ensued. The great importance of this fact must be at once apparent, not only as regards cases of tedious labour, but in those instances where the accoucheur is obliged to induce premature labour. Nor should we lose sight of its probable application for criminal purposes in producing abortion. Dr. Radford of Manchester, (England), and others, have recommended electro-galvanism for the arrest of uterine hæmorrhage after delivery, and its success has been fully established; but in these cases, it is not to be expected that it will ever be extensively employed, for the absolute necessity for immediate and prompt treatment, and the delay which must always occur before the apparatus can be set to work, are in themselves sufficient objections against substituting this for the ordinary method in such cases.—R. L. M.D.]

OBSERVANCE OF METHOD IN CONDUCTING POST-MORTEM EXAMINATIONS.

ESPECIALLY WHEN THEY ARE INTENDED FOR LEGAL PURPOSES.

By H. LETHBRIDGE, M.B., Lecturer on Chemistry at the Medical School of the London Hospital.

A very slight acquaintance with the practice of our criminal courts is sufficient to point out this very prominent fact, that of all classes of persons who may be called upon to give evidence, medical men generally cut the worst figure. They are so undecided in their manner; have always omitted so many important points of the inquiry; are accustomed to rely so thoroughly on the opinions of others, and, in short, give their testimony with so much qualification and confusion, that they are looked upon as a prominent and an easy mark for the very worst advocates. Now, the secret of all this appears to lay in one or other of three great deficiencies, as—1st, a want of a good medical knowledge; 2nd, a want of method or system in conducting the inquiry; or 3rd, a want of caution in forming an opinion, and of steadiness in asserting it. Omitting the first of these, which is, unfortunately, a far too frequent cause, but which cannot be discussed here, then the second assumes the greater importance, for it is out of this want of method that the third is sure to flow; it begets incaution and uncertainty in the manner of the witness, and is suggestive of all the subtlety and misconstruction with which the case is sure to be surrounded: and if it does not beget, it will favour, the development of the other bad but plausible elements of jurisprudence.

I have more than once seen a good and a clear evidence broken into pieces, and made altogether worthless, because the observance of some little point, remote enough in the inquiry, had been omitted. Let me take an instance:—A man dies suddenly, and circumstances seem to show that he had been poisoned; the medical attendant does not en-

ertain a doubt upon the question; he had, in fact, made up his mind before the body was looked at, and, to his thinking, there was no necessity for examining the head, or the spinal cord, or even the heart, beyond taking a glance at its position. When, therefore, he gets to be questioned in the witness-box, he is compelled to admit that he does not know anything about the state of the nervous centres, and is quite unable to say whether the coronary arteries were normal; whether the valves of the heart were healthy: whether there was any communication between its right and left sides; and he is, in fact, altogether unprepared to negative a supposition, that death might have been occasioned by any one of half-a-dozen natural causes which the sophistry of an advocate could easily invent, and as easily give probability to; and then comes the consequence,—that his evidence, though good in the main, and absolutely correct in its import, is nevertheless set down as nought, and he himself abused and grievously discarded.

Now this is not an imaginary or an overdrawn instance for the daily and almost hourly practice of our courts is giving the reality to similar ones; and we may learn from them a world of profitable experience—as, not to be guided by premature opinion, nor to take anything for granted, and above all, to observe a method in conducting our inquiries.

Again, the law of evidence imposes upon every medical man the necessity of conducting his operations in such a manner as will enable him to take cognizance of every fact connected with the inquiry; and these, moreover, should be recorded exactly in the order in which they occur. In this manner, he will possess himself of all the information relating to the case; he will be able to meet the sophistry of an opposing counsel—to give his testimony clearly and distinctly—and to say with much confidence what has, or what has not, been the immediate cause of death.

A little attention to duties of this kind has led me to think, that almost every medico-legal inquiry may be profitably conducted, if it be followed out somewhat after this manner:—

1. Record the name, age, and sex, of the deceased.
2. Note the day and the time at which the examination is made, and set down the period which has intervened since death.
3. Direct attention, as early as possible, to surrounding circumstances, as to whether there is any weapon or trace of blood near, any cup or bottle likely to have contained a poison, or any evidence whatever in the position of things about the body to indicate a struggle or the cause of death.
4. Observe the position of the body. Is it in bed or on the ground; and how does it lie?—or is it suspended?—and so on.
5. Is the body naked?—or are the clothes on? and are these disturbed or torn?
6. Notice the position of the limbs. Are they or the fingers bent, as if convulsions had preceded death? Open the hands, and search for anything which may have been torn from an opponent during the death struggle.
7. Observe the appearance of the countenance, as regards its colour and expression. Are the eyes open or prominent? What is the state of the conjunctiva and pupil? Is the mouth open, or is there any foam about it? Does the tongue protrude, or is it bitten? Smell the mouth, and notice the colour of the gums.

Connected with this part of the inquiry it may be said that the countenance will often give an important indication of the cause of death, and of some of the circumstances which immediately preceded it. Thus, as regards its colour, it is mostly livid after apoplexy and death from hanging or stran-

Swelling, and pale from hydrocyanic acid, the mineral acids, and the alkalies, as well as from most of the vegetable poisons, from blows upon the epigastrium, or injury to some vital organ. On the other hand, it may be either pale or red after poisoning by carbonic acid, alcohol, or opium, and some other poisons.

The expression of the face, too, is generally convulsive when there has been much struggle immediately before death, as during hanging, drowning, or poisoning by prussic acid, strychnia, and nuxvomica: and it is often convulsed in cases of death accompanied by great hæmorrhage. On the contrary, there is more frequently a calmness in the expression after death from apoplexy, opium, carbonic acid, &c., while the features are pinched, and there is an anxiety, in the look after the action of the metallic and most vegetable poisons.

The eye also is generally open, prominent, and glazed, after the effects of hydrocyanic acid, and prominent and injected after death from apoplexy and strangulation. Some authors, moreover, have described a suffused or even ecchymosed condition of the conjunctiva after death from arsenic.

The foam about the mouth is very characteristic of prussic acid, while the bitten tongue would indicate a great struggle or convulsive action immediately before death.

The gums put on a blue appearance after poisoning by lead; and the red or spongy gum, or even an ulcerative condition of the mouth, is often indicative of the action of mercury. It must be remembered, however, that the same appearances, together with extreme salivation, have been produced by other substances, as iodide of potassium, colchicum, fox-glove, croton oil, and some other metallic and vegetable substances.

8. Proceed to remove all clothes from the body, and observe if there has been any evacuation just before death, either from the stomach or rectum, or bladder, or if there has been any emission of seminal fluid. All of these appearances indicate convulsive action, and the latter, which is the common accompaniment of death from hanging, points to an irritation of the upper part of the spinal cord, and may serve to establish the fact of suspension before death, supposing that an opposite question were to arise.

9. Note the appearance of the body, whether deceased was fat or thin. Observe the colour of the skin, whether it is livid in any part, or if it presents any marks of violence, and if putrefaction has commenced. The colour of the skin is exceedingly pale when there has been much hæmorrhage before death, or after starvation, &c., and it acquires a yellow tint after poisoning by lead and copper: it is often ecchymosed or covered with purple petechiæ after arsenic, and some wasting diseases. The dependent parts are generally very livid soon after death by hydrocyanic and carbonic acids; and when there is any mark of violence, this should be accurately described, considering by what means it was likely to have been occasioned: whether it could have happened by accident, or by the hand of deceased, or by that of another person. In examining wounds about the throat, it is of great importance to notice on which side of the neck the wound is deepest, for this may indicate where the cut was commenced, and whether it was made from right to left, or from left to right: dissect them also to learn what parts have been involved in the division. If there is any mark of a cord around the neck, observe where the knot pressed, or where the cord was crossed; for one person would, in all probability, strangle another by crossing it behind, while in a case of self-murder it would most likely be crossed in front.

Lastly, it is of great consequence to know whether the wounds or bruises were produced before or after death, and although this cannot always be confidently stated, yet there are circumstances which will often furnish very important indication—as the absence or presence of coagula in the

wound, or of ecchymoses around it, or of infiltration of blood, fibrin, or serum, into the surrounding tissue; and I have noticed that most post-mortem wounds soon dry and discolour upon the edges, acquiring a brownish parchment-like appearance. With respect to evidence furnished by putrefaction, I shall have occasion to refer to this more particularly under the next head; but it may here be stated that most poisoned and plethoric bodies putrefy quickly, and especially those of persons who have died soon after a full meal; while, in the reverse cases, and after poisoning by arsenic, there is generally a delay in this process.

11. Can any opinion be formed as to the time which has elapsed since death took place?

This is often a very important question, and may involve an answer having a certain day, or even hour, for its limit. We shall find, however, that the post-mortem signs are not by any means so constant in their occurrence, or so conclusive in their import, as to warrant us in making, on all occasions, such a positive reply. Our evidence upon the subject may be collected somewhat after the following manner:—

(a). Is there any warmth in the body?

I have not been able to get together very many facts connected with this as a sign, and my observations have been made chiefly upon subjects removed directly after death from the wards of an hospital into a dead-house, the temperature of which was about 50 deg. Fah. I have found that adults cooled pretty constantly after this manner:—The extremities lost their heat very rapidly, sinking to the temperature of the room in less than three hours, while the surface of the trunk has felt warm, even up to the twenty-fourth hour; and at this time, a thermometer, placed either in the axilla or rectum, has generally stood at somewhere about 70 deg. Fah. In fact, these parts have hardly ever lost the whole of their heat until after the lapse of thirty-six, or sometimes forty-eight hours.

There are many circumstances, however, which may modify this order of things, as, for instance, the body would have cooled faster had it been exposed to a current of cold air, or had it been immersed in cold water; also children and very thin subjects will cool sooner than adults, or corpulent ones. Again, when the body has remained in bed, or been well clothed, and surrounded by bad conductors, the temperature is retained for a somewhat longer time.

(b) Has the rigor mortis set in?

It most commonly happens that the limbs begin to stiffen in about two or three hours after death, and the rigor is generally firm and complete after the lapse of seven or eight hours. To this, however, there are occasional exceptions, as for example, it is nearly always accelerated when the fatal event has been sudden, and when, immediately before death, there has been violent convulsive action, or a prolonged muscular exertion. In such cases the living spasm appears to pass at once into the dead rigor. Look, for instance, at the suicide, who is often found with the weapon firm in his grasp; and so with the murdered and the drowned, who frequently retain their hold of objects which had been clutched during the death struggle. Cases, on the contrary, will now and then occur, in which the rigor mortis appears to have been delayed for a very considerable period. Such, however, is rarely the sequence of sudden death, but it is, as far as my experience goes, indicative of some inflammatory action immediately before death. It has also been said that there is no cadaveric rigidity when life has been destroyed by lightning or by electricity; but I am not disposed to put faith in this assertion, for experiments on animals seem to show that a fatal shock will throw them at once into a state of extreme rigidity, out of which they pass in an unusually short time.

(c). What is the condition of the cornea?

In general the cornea becomes slightly clouded after the lapse of nine or ten hours. In about sixteen this condition

is still more evident; it then gets somewhat lax, is easily indented on pressure, and when this is made upon the side of the eyeball, the cornea becomes still more opaque. In about twenty-four hours it commonly acquires perceptibly greater laxity, and in forty-eight it may become quite flat, and so opaque, that the pupil can hardly be defined through it.

(d). Has putrefaction commenced?

This is rather an inconstant event, and is liable to be interfered with by many modifying circumstances, such as the temperature of the room, the time of the year, the condition of the body, the cause of death, and whether it occurred soon after a meal or not, and so on. Nevertheless, we must attempt to set down something like an order for the time and succession of its several steps. In about eight or ten hours after death, the surface of the body, especially over the chest, and on the inside of the arms and thighs, puts on a marbled appearance, due to a turgescence of the superficial veins. In about sixteen hours, the dependent parts become livid or reddish-purple, and after the lapse of twenty-four hours, this lividity is generally very marked, and the marbling on the chest and arms begins to acquire a purplish tint. About the second day it assumes a brownish hue, and at this time the abdomen and groin show more evident marks of the putrefactive process, by acquiring a green colour. From this period it advances with more or less rapidity, according to attendant circumstances. In five or six days, the entire surface is ordinarily very green, and the venous marbling still strongly marked. About this time, in warm weather, the epidermis begins to loosen, and the fluids acquire great liquidity, and gravitate to the dependent parts, through which they readily escape: beyond this, the track of decomposition can scarcely be followed with any certainty.

While we are occupied in discussing the question which refers to the time of death, it may not be altogether out of place to refer to those evidences which prove that death has actually taken place, for there are several morbid agents which have the power of producing a state exactly simulating death, and it will devolve on the medical man to pronounce whether that state is apparent or real. The records of this country, but more especially those of France, where the burials are more hastily hurried over, show that numbers are annually consigned to a premature grave.

12. What therefore are the evidences which show that death has really taken place?

(a). An absence of cardiac pulsation, and of the respiratory movements.

(b). A loss of sensibility in the excito-motory system, as when the eyelids cease to wink on being touched, or the limbs and muscles to move on being pinched or pricked; or one of the most powerful agents as a test of this function is galvanism, and I have found that the muscles lose their faculty of contracting under its influence in about three hours after death. Nysten has given results which appear to indicate a much longer persistence of this the last act of vitality; but as far as my experiments have gone, I am led to think that three hours is about the mean time of its duration.

(c). The appearance of the rigor mortis, which generally sets in after about two or three hours.

(f). The loss of temperature in the body.

(g). The opacity or cloudiness of the cornea.

(h). The lividity of the dependent parts, and the mottling on the arms and chest.

(i). The setting in of putrefaction.

Of all these signs of death, the second merits the greatest consideration, because of the certainty of its import and of the early period at which it generally manifests itself.

13. Examine the head, and note if there is any bruise on the scalp; does the latter bleed freely as if its vessels had been congested?

14. Are the membranes of the brain natural, and is there any fluid upon or beneath the dura mater; note its quality

and quantity: and when there is any blood, observe if it is coagulated or not. Are the vessels on the surface of the brain gorged or not; and is the blood liquid or dark?

All the considerations which arise from these inquiries will be well enough understood, but it is to be remembered, that apoplexy, and, according to Dr. Conolly, epilepsy complicated with mania, will nearly always produce an extreme congestion of the vessels of the brain, and even in some instances, effusion of blood between the dura mater and arachnoid.

15. Remove the brain, and examine it by making a series of thin horizontal slices, until it reaches quite to the base; observe if there has been any softening, or are there any bloody points indicative of congestion; has any blood or serum been effused into its substance, or into the ventricles, and what is the condition of the choroid plexus.

If the brain has not been examined in this careful manner, difficulties may arise in after stages of the inquiry, for no person can pretend to say whether it was diseased or not.

16. Examine the calvarium and the base of the skull for any fracture.—*Lancet*.

DISSECTING ANEURISM.

The infrequency of this disease is perhaps sufficient reason for publishing the following case.

The patient was a strong, fine looking negro, a cook on board a Liverpool packet, aged about fifty, and except an occasional uneasiness at the epigastrium, evidently caused by indigestion, had enjoyed uniform good health.

Aug. 12th, 1844. He was attacked with severe colic, accompanied with hot skin, soft, full pulse, and previous constipation. Forty grs. of calomel, and ʒ. of oil, accompanied with copious injections, cooled the skin and relieved the pain, but did not move the bowels; the loss of twelve or fourteen ounces of blood, about six hours after the calomel and oil were taken, produced slight faintness, and was soon followed by free evacuation of the bowels, and entire subsidence of the pain. He continued to improve, and by the 17th, was nearly recovered, complaining of nothing except weakness. On the 17th, after lying down for an hour, after dinner, he attempted to rise, but immediately fell back, and died instantaneously without a word. On examination, twenty-four hours after death, all the organs of the chest and abdomen were found healthy, excepting the heart. The pericardium contained about a pint of fluid blood, and the same of coagulated. The cavity of the left ventricle was dilated, being increased about one-fourth, and the walls proportionally thickened. The blood had escaped into the pericardium through an opening in the aorta, just above the semi lunar valves. On opening the aorta, the serous coat was of a dusky-red appearance, and somewhat thickened, but uniform and smooth. Half an inch above the orifice of the aorta was a Z shaped opening, about one-fourth of an inch long, through the lining of the vessel, and the inner lamina of the muscular coat; a passage for the blood was now formed between the two lamina, embracing from half to three-fourths of the entire circumference of the artery, and extending as far as we could examine (we were obliged to remove the heart by stealth, and examine at home, lest, by attracting the attention of two or three friends present, we should lose the specimen), which was about six inches. The origin of the innominate, and the carotid and subclavian, were plainly included in the disease, though to what extent we could not determine; and if any inference could be drawn from the part of the aorta preserved, and still in my possession, the dissection must have continued nearly, if not quite, to the iliac arteries. The rupture into the pericardium was circular and ragged, and situated a very little below the internal opening, which was smooth and even. The laminated structure of the middle coat was clearly apparent, and left no doubt that the fictitious canal was formed by a separa-

tion of the inner and outer layers of this tunic. The specimen is a fine one, and it is greatly to be regretted that we were unable to trace it out as far as it extended.

MEDICAL JURISPRUDENCE.

DAMAGES RECOVERED BY A NURSE DISEASED BY A SYPHILITIC INFANT.

ASSISTANT BARRISTER'S COURT, CORK.—APRIL 3.

Before H. BALDWIN, Esq., Q. C.

A case of considerable interest to the medical profession, involving this disputed question, came on in this court this day, and was listened to with marked attention by a crowded auditory. It had been previously before the same barrister, but was "dismissed without prejudice," a medical gentleman deposing that the disease was not contagious. It was now brought forward, strengthened by testimony of high medical authority, for the purpose of showing that the opinion of that medical witness was fallacious.

The case was stated by Mr. Bryan Galway, who said it was a special action for a sum of £94s. 7d. damages by the plaintiff against the defendant, under the following circumstances:—In the month of September, 1844, the plaintiff's wife received a child from the defendant to nurse, at a yearly sum of £4. The child had a sore on its mouth, but no particular attention was paid to it. In a few days the plaintiff's wife became diseased, and she diseased her husband; in fact, all the family became ill. The plaintiff's wife, ascertaining the nature of the disease, returned the child to its parents. The nurse, who was pregnant, was subsequently delivered, and the child then born was also diseased, and in some time after died of it. The disease appeared to fasten on the plaintiff's wife, and to get cured of it she had to go into the hospital of the Cork Union Workhouse. Everything that medical skill could suggest to effect her cure was tried, but ineffectually, for the poor woman is still labouring under the disease. For this injury the action was brought to recover damages.

Evidence was given to prove the contract, and a quack doctor was called to show that he was to get 30s. from the father of the diseased child for the purpose of curing it of the corruptive sores on the body. This fellow called himself a "herbalist"—a cognomen which appeared to sound not altogether agreeable to the barrister, who remarked that it he caught one of them "herbalists" in his criminal court, he would know how to deal with him.

The brother of the plaintiff deposed to the good health of the plaintiff and wife previous to getting the child to nurse, and that the child had sores on the mouth, body, and penis.

Dr. O'Connor, physician to the Cork Union Workhouse, was sworn—He said that the plaintiff's wife was six weeks under his care, labouring under syphilis in a secondary degree; a child would convey the disease to the nurse; there was no doubt of that; a child might be born apparently free from that disease, and subsequently break out and communicate it.

Court—Is that opinion general?

Witness—That is the opinion of Sir Astley Cooper, Mr. Colles of Dublin, and the most eminent of the faculty.

Court—Is there any dispute about that now?

Witness—None whatever.

Court—Have you read the treatise of Ricord, the eminent French physician, on that subject?

Witness—I have not. I have read a review of the work, and though Ricord laid it down that secondary symptoms could not communicate the disease in a primary form, he never held that the secondary disease was not contagious in a similar degree.

Court—Have you seen the child?

Witness—No; the mother only, and she is not well yet. Dr. Christopher Bull, surgeon to the Cork South Infirmary, deposed that he had seen the plaintiff's wife about a week since; she was then labouring under the disease in a secondary form; he had met numerous cases where the disease had been communicated by children to nurses.

Dr. T. M. Ahern sworn—He had attended plaintiff and wife previous to September, 1844; they were in good health and free from disease; saw the child alluded to; it was diseased; plaintiff and wife became diseased, and are so still.

To the Court—Had no hesitation in stating that it was the child which communicated the disease to plaintiff's wife, for this reason, that her breasts were affected while every other portion of her body indicated no disease.

Mr. Scannell then raised a legal question as to the guilty knowledge of the defendant in giving the diseased child to nurse, contending it was necessary to prove that before the action could be sustained.

To meet this, the court inquired of the medical gentlemen whether it were possible that the mother of the child was not aware of the disease?

Dr. Bull replied that it might, but the chances were that she knew it. He himself had observed the disease one hundred times in nurses who got foundlings to nurse, and they were astonished when they ascertained the nature of the disease. It was of an insidious character, and lurking in the system. He had also seen cases of secondary where there had been no primary disease observed.

Court—How was the child affected?

Dr. Ahern—Both secondary and primary.

Court—How was the plaintiff's wife affected?

Dr. Ahern—In a secondary form.

Dr. Bull—That is invariably the case when the disease is communicated by a child to a nurse.

Court—As there is some difficulty in the case, I shall take time to consider it. If I grant a decree it shall be for the whole amount, as I look on these poor people to have been injured for life.

MORTALITY OF ILLEGITIMATE CHILDREN.

The frequent occurrence of illegitimate births in the Prussian province of Posen, has induced Dr. Cohen v. Baren to institute some investigations as to the injury resulting in them to the children, from the mother being placed in an improper position at the time of birth, as compared with injuries from the same cause in married women. Of fifty cases, thirty were born while the mothers were standing, seventeen while stooping or sitting, and two while kneeling. Of the fifty women, thirty-two were primiparæ. Of the children, forty were at the full time, and ten premature; of these latter, seven were above thirty weeks of uterogestation. Of the nineteen which were born while the mother was stooping, sitting, or kneeling, one had a fracture of the skull; it was probable, however, that this was caused by laying a heavy stone on the child's head, for it was dropped on soft turf; in ten of these not the slightest contusion or ecchymosis could be discovered; in one, probably from dragging the cord, which was much shortened from being several times twisted round the fœtus, there was rupture of the liver. In twenty-five cases the umbilical cord was torn; in seven the placenta came away along with the fœtus, the cord being untorn; in fifteen the cord remained uninjured; and in three this point could not be determined. In the twenty-five cases, where the cord was torn through, eleven children presented ecchymosis, five fractures or fissures of the cranial bones, and one rupture of the liver. The conclusions from these investigations, compared with those which Henke gave in his critique on Klein's cases, are as follows:—1st. The proposition that the fall of children on the ground can cause dangerous injuries, and through these death, is proved; and although it must be regarded in general, as only an occasional cause of death, still cases are not wanting where injuries received in this way have been the sole and only cause. In illegitimate children, too, a trifling injury is of greater importance than in children born in wedlock, and may be the cause of their death. 2nd. It

is proved that the fall is not invariably followed by death, as many children have fallen without receiving the slightest injury. 3d. That if unexpected protrusion of the child is frequent in persons who do not conceal their pregnancy, it is much more frequent in those who do. 4th. In unmarried females, it occurs chiefly in primiparæ. 5th. The assumption, that unmarried females being generally long in labour, the injuries observed on the foetal head are to be attributed to its long detention in the pelvis, is correct in a very few instances. 6th. The unusual conditions in which women who bear illegitimate children bring forth, show that very slight contusions, concussions, and extravasations, arising from the parturient process, may be followed by death, and therefore the medical jurist ought to be very careful in attributing such traces of injury (even though very considerable), to violence intentionally applied. 7th. Of four children born in an unusual position, in three it can be affirmed that the cord was broken by the act of parturition itself. 8th. Injuries of the head are to be ascribed to the fall, more especially where the ground is hard, rather than where it is soft. 9th. The integrity of the cord is an obvious prevention to the production of injuries of the head; and where injuries are met with under such circumstances, we must rather suspect that they were induced by violence applied in some other way. 10th. In delivery, in an unusual position, the cord is generally torn; it is seldom that the fœtus remains in connection with the placenta in the uterus, and still more seldom that both come away together with the cord entire. 11th. Illegitimate children show a less degree of physical development.—*Preussische Verein Zeitung, in Northern Journal of Medicine.*

SURGERY.

LECTURES ON DISEASES OF THE KNEE-JOINT.

By Sir B. C. BRODIE, Bart.

LECTURE III.

SCROFULOUS DISEASES OF THE KNEE-JOINT.

Delivered in the Theatre of St. George's Hospital, Jan. 21.

(Continued.)

Having described in the last lecture the symptoms of scrofulous diseases of the knee-joint, I have now to speak of its treatment.

In former times this disease was confounded with a number of others, under the general appellation of *white swelling*, and they were all treated very much in the same way. Blisters, setons, issues, and other kinds of counter-irritants, were had recourse to. Such was the common practice when I was a student in this hospital, and for a long time afterwards. It was a most unskilful procedure, and in a large proportion of the cases, if not in the majority, the loss of the limb was the consequence. Other modes of treatment, however, were occasionally had recourse to. One person recommended one thing, another person recommended another. Some of the remedies employed were innocent enough (poultices of sea-weed, for example), while others, such as friction and chamopping, were as mischievous as possible.

I need not occupy your time by describing the other methods of treatment which were formerly proposed, or which are, to a certain extent, now in use. It will be better that I should at once explain the results of my own experience on the subject. First, let me say a few words as to what ought not to be done; and a very important consideration this is on many occasions. Blood ought not to be taken away from the joint, either by leeches or by cupping, and far less should it be taken from the general system. The disease depends on a weak state of the constitution, and the abstraction of blood will only tend to its aggravation. Neither blisters, setons, issues, tartar emetic ointment, nor any other kind of counter-irritant, ought to be employed. Such remedies torment the patient, they make him ill from the general disturbance of the system which they produce, they lower him by the continued discharge of matter, and do no good whatever to the malady.

I express this opinion in the strongest and most confident manner, having seen this kind of treatment extensively practised formerly, and being able to compare the results with those arising from the treatment which I have since employed.

There is one great principle to be attended to in the treatment of all diseases of joints—namely, that the joint should be kept in a state of perfect repose. If there were an inflammation or a sore in the leg, and it were rubbed all day long, would that inflammation ever subside, or that sore ever heal? If a diseased joint be subjected to friction, as it must be when it is kept in motion, is it likely that a cure can be effected? There is no mode of surgical practice more important than this, that a diseased joint should be kept in a state of perfect immobility. In the early stage of the disease that is the only local treatment that is required, and, indeed, it is the chief thing to be attended to even in its most advanced stages.

There are different methods of keeping the knee in a state of immobility. The simplest method is that of employing the leathern splints which I formerly mentioned, one on each side of the joint. It has the advantage of giving very complete support; at the same time that the splints are easy to be worn, because they exactly fit the parts to which they are applied, and that, there is the further advantage of the patient being able to take them off and put them on for himself. An apparatus that is not easily removed is liable to great objection. There may be an attack of inflammation, causing a sudden increase of the swelling, and requiring the splints and bandages to be removed and readjusted; and if these be of such a kind that the patient cannot readily do what is wanted himself, he may suffer torture until it is done for him by his surgeon. I need not occupy your time by explaining that a continuance of pressure on the joint when it is inflamed and trying to swell, cannot be otherwise than productive of great mischief.

But while the joint is kept in a state of perfect repose, attention must be paid to the general health; for the disease having its origin in a morbid condition of the constitution, it is needless to attempt to cure it by mere local treatment. It must be plain to you that no general rule can be laid down on this subject. It may be that the evacuations are of a white colour, the bowels confined, the tongue furred, and it may be requisite in the first instance to have recourse to a course of blue pill or gray powder, and occasional purgatives. But as soon as the digestive organs are brought into a proper state, the patient will generally be benefited by the exhibition of what are called *tonics*, but especially of some preparations of iron. To children I give the *vinum ferri* of the old pharmacopœia, proportioning the dose to the age of the patient. This generally agrees with him very well; and if to this be added the occasional exhibition of purgatives, other medical treatment is seldom required. The patient may take this or some other preparation of iron for three weeks, omit it for ten days, then resume it for three weeks, and so on for an indefinite period: by which I mean to express as long a period as two or three, or even four or five years; our object being not so much to cure a particular disease as to mend a weak constitution. Diseases may often be cured in a short space of time, but a weak constitution cannot be rendered a strong one until after the lapse of some years. The patient generally requires to be watched whilst he is using these remedies. They may over-stimulate him and make him feverish, and then the dose requires to be diminished, or the medicine must be omitted for a time, to be resumed afterwards. I have mentioned the *vinum ferri*, but other preparations—for example, the *tinctura ferri murialis*—may be exhibited, or the syrup of iodine of iron. I sometimes give the latter and the *vinum ferri* alternately. It appears to me that in these cases

large doses of iron are not required; small doses introduced into the system, off and on, for a great length of time, are what you are to rely upon. Five patients out of six will be benefited by taking iron. Every now and then, however, a patient is met with, with whom no form of iron agrees, and recourse may then be had to quinine, bark, or the alkaline solution of sarsaparilla. The latter combination agrees very well with delicate children, and may often be given with advantage in alternation with preparations of iron.

But the object being to improve the constitution, much will depend on other things besides the exhibition of medicine. A scrofulous child will not prosper in the air of a crowded city, though he may do very well in the country. In general, patients are benefited by a residence at the sea-side—that is, by having the opportunity of breathing the sea-air during a considerable portion of the year. Sea-bathing may be serviceable under certain circumstances, but I have no doubt that the advantage of it is very much over-rated. It does good, because the patient must go to the sea-side to obtain it, but I question whether sea-water baths are in themselves better than any other baths. A long while ago, a person, being encouraged to do so by the medical practitioners of that day, established some sea-water baths near the Strand. A vessel went to the Nore three times in a week, and was so constructed that it filled itself with a supply of sea-water. There was a large plunging-bath, and some excellent warm sea-baths, quite as good as can be procured at Dover or Brighton. At first they were popular; patients went their either of their own accord or because they were sent by their medical advisers: but every year there was a little falling off, and in the course of a few years the proprietor, finding that he had scarcely any customers, was compelled to give them up. The fact was, the public found out by degrees that sea-baths without sea-air were of little avail. The same observation may be made respecting other methods of using sea-water. A child goes to the sea-side, towels wet with sea-water are put on, or a great quantity of sea-weed is tied round the knee; and this is supposed to do good, but it is a poor substitute for the leathern splint. The fact is, the child's health is improved because he is breathing the sea-air, and the good thus obtained is attributed to other causes. It is also well for the patient to live on a plain but nourishing diet, and to be as much as possible in the fresh air; care, however, being taken that he should not be exposed to cold; and with this view, whenever he leaves the house, or lies at an open window, he should be well wrapped up, and defended from changes of temperature by warm clothing.

This is the whole of the treatment that is required in ordinary cases, and it is for the most part eminently successful. Indeed, when you are called to a young person with scrofulous disease of the knee in good time, before matter is formed, and in whom there is no visceral affection, it will scarcely ever happen that the case does not end well.

But let it be observed, that no good can be obtained without perseverance. It may be necessary for the patient to wear the splints sometimes for three months, sometimes for six months, and sometimes even for a year; while the constitutional treatment may be required for a much longer period.

But perhaps the joint is contracted, the leg has been kept bent upon the thigh, and at the end of a certain time it becomes a question whether it can be made straight. You may effect this object by applying the instrument with a screw described in a former lecture. Violent extension on this, as on other occasions, is to be avoided. By little and little, and by very slowly turning the screw, the leg

is to be brought into the straight position. Then no harm will happen, and not only the usefulness of the limb will be saved, but actually the mobility of the joint in many instances.

In the more advanced stage of the disease, when abscess is formed, the cure is much more doubtful, and at any rate is much more tedious; but the principle of treatment is exactly the same. If there be abscess, there is still greater reason for keeping the joint in a state of repose than if there be not. When the abscess presents itself externally, and the skin over it is tender, the splint which presses on that part must be left off, but the other may be allowed to remain. Sometimes, however, the abscess is so situated that both splints must be left off for a time, and the joint fomented and poulticed, as in other cases of suppuration. As soon as the skin becomes thin, open the abscess; you will save time by doing so. When the abscess has been opened three or four days, re-apply the splints, placing some simple dressing over the orifice of the abscess. A little change is now required in the splints, inasmuch as they must be lined with oil-silk, in order to prevent them from being spoiled by the purulent discharge.

When abscess has formed, you must generally be well satisfied to save the limb, without expecting to save the mobility of the joint. I do not say that the latter object will never be attained, but in nine cases out of ten, after the formation of abscess, there will be an ankylosed joint. This is not a great inconvenience provided the limb be in a proper position, but it is a very great one indeed if it be ankylosed at a right angle. To prevent this, notwithstanding the abscess, the machine with the screw may be had recourse to, so as to straighten the leg. Sometimes, in the case of abscess, when it is closing, a small piece of bone exfoliates, and it may be that several pieces may exfoliate afterwards. When exfoliation occurs, there is always ankylosis: the process of cure is very tedious, and in fact, many years often elapse before it is so complete that no more separation of dead bone takes place. All that I have just now stated shows how important it is that the case should come under judicious treatment before abscess is formed. It makes, indeed, all the difference as to the period of cure. If matter be not yet formed, it may almost to a certainty be prevented, and a few months may be sufficient; but if there be a single drop of matter, it becomes the nucleus of a large abscess; and then, not only many months, but some years, may elapse before the cure is complete.

I have said, that under proper treatment, especially if consulted in the early stage of the disease, you will seldom fail in preserving the limb; still you will not do so in all cases; and sometimes, from the bad state of the constitution, and in spite of the best treatment, the disease will go on. If a child be brought into the hospital in whom the disease has been neglected for years, so that the joint is entirely destroyed, nothing better can be done than to amputate the limb: but is the operation to be performed in all these cases indiscriminately? Indeed it often requires a great deal of judgment as to whether you should amputate or not. I am always unwilling to resort to amputation, because I think a limb so valuable that it is worth the patients while to run some risk as to life, if there be a chance of preserving it. But there are other reasons for hesitating about amputation. I have frequently known cases in which, immediately after a scrofulous knee has been amputated, symptoms of disease in the chest have become developed, and the patient has died of tubercles and vomica in the lungs; and I have known other cases in which, under the same circumstances, he has sunk under disease, of the mesenteric glands, or hydrocephalus.

Before a limb is condemned, careful inquiry should be made as to the state of the general health, and pains taken to ascertain that there is no disease going on in other organs. If one knee be amputated, and disease has begun in the other knee, or in the lungs or mesentery, the probability is that this other disease will go on with increased rapidity. But, on the other hand, where disease has occasioned great mischief in the knee, the limb ought to be amputated rather than that the patient's constitution should be worn out by it; and it should be always borne in mind, that although disease in a joint does not directly destroy life, it may do so indirectly, by producing such a state of exhaustion as is favourable to the development of disease in the lungs and other viscera. If by examination with a probe, it be ascertained that there is a piece of dead bone connected with the interior of the joint, and so situated that it cannot exfoliate, the limb may be given up at once; there being no chance of a cure, and no remedy but amputation.

I have stated that where an abscess is formed, if the patient recovers, it will be generally, with an ankylosed limb. Bony ankylosis will take years for its completion. A knowledge of this fact is of importance, as it shows that if the limb be bent, there will be plenty of time to get it into its proper place.

The disease of which I have now spoken is one of the most common in surgery, especially in children; and there is no disease the result of which is more affected by surgical treatment than this. Under wrong treatment it will generally go on from bad to worse, until the joint is destroyed and the limb is lost; whereas, under good treatment, in nine cases out of ten the patient will recover. It is therefore a subject especially worthy your attention as practical surgeons.

Primary ulceration of cartilages of the knee.—In the last and preceding lectures I have alluded to the ulceration of cartilages. In the cases which I have hitherto mentioned they become ulcerated as a consequence of disease in other textures; but sometimes this process takes place apparently from disease originating either in the cartilage itself or in the surface of the bone to which it is connected. It has been said that cartilage does not possess bloodvessels, and therefore that it cannot ulcerate from an action originating in its own structure, but only from the influence exercised upon it by the vessels of the parts which are in contact with it. But in growing persons it is evident that cartilages are sufficiently endowed with vascularity. On making a section of the articular cartilages of a child, large vessels are perceived in it injected with blood. Nor can it be doubted that in adults the cartilage possesses vessels, or some structure that answers the same purpose. Are not cartilages constantly exposed to friction; and if they have not the power of self-repair, what is to hinder their being worn away in consequence? A man walks or labours all day long, rubbing the cartilages of all the joints against each other; nevertheless they remain uninjured through the whole of a long life. Then they are subject to a particular alteration of structure, indicated by a fibrous degeneration, of which this preparation affords a good specimen. Such an organic change may occur at any period of life, but it is of very frequent occurrence in old persons; and this cannot be explained except we suppose that they possess either blood-vessels, or something equivalent to them. I mentioned in the last lecture, that in cases of scrofulous disease of the joint I have seen bloodvessels injected with red blood extending from the diseased bone into the cartilage connected with it, and ramifying on its substance. The same appearances have been observed by Mr. Mayo and Mr. Liston. Here is a preparation taken from a boy who had met with a compound fracture of the thigh. The femur was separated at the epiphysis, and some time afterwards it was necessary to amputate the limb. There was no matter in the the joint, no fluid, and no

inflammation of the synovial membrane; the surface of the cartilage in contact with the bone was entire, but towards the articular cavity it had in many places disappeared, as if a portion of it had been removed by a chisel. There are several preparations in the museum, showing the same kind of absorption of the articular cartilages; and if you will take the pains to study them, you will be satisfied that what I have now stated is correct.

In adducing these facts, however, I do not mean to say that in what I call primary ulceration of the articular cartilages, the ulceration always begins on the surface which is towards the articular cavity. There is reason to believe that, in some instances, the morbid action originates in the surface of the bone with which the cartilage is connected, extending from thence to the cartilage itself; and in practice I do not pretend to distinguish these two orders of cases from each other. The disease may, for the most part, be traced to rheumatic inflammation, having this peculiarity, that it is confined, in the first instance, to the harder textures. There are, perhaps, wandering pains, first in one shoulder, then in the other, then becoming, as it were, concentrated in the knee; the cartilages of which soon afterwards begin to ulcerate.

On examining the joint in an early stage of the disease, the cartilage is found to be absorbed at one point, and the surface of the bone exposed and carious. Probably there is no effusion of any kind in the joint, neither serum nor pus; but the exposed surface of bone is more vascular than under ordinary circumstances. As the disease advances, the ulceration of the cartilage becomes more extensive, and when it has attained a certain point, pus is formed in the joint. As in some cases there is suppuration without ulceration, so in these there is ulceration without suppuration. As I have observed in a former lecture, the two processes are generally combined, but there is no necessary connection between them. The cartilages at last become destroyed throughout the knee—on the femur, the patella and the tibia. Sometimes, when abscess forms, it is limited by adhesion to one part of the joint, and then perhaps suppuration takes place in another part of it. In other cases the abscess is not so limited; the whole joint is distended with matter, so as to form one large abscess; and in this stage of the disease the bones in the neighbourhood of the joint become inflamed and dark coloured; the matter lodging in the cancelli becomes putrid, probably a portion of the bone loses its vitality and exfoliates into the articular cavity, while the abscess finds its way out in various directions, making numerous sinuses under the fascia and among the tendons, before it presents itself externally.

While these changes take place in the affected joint, they are indicated by the following symptoms. Generally, as I have already mentioned, there are rheumatic pains in other joints in the first instance; by and by the pains are, as it were, concentrated in the knee. The pain is very severe, and yet the joint is scarcely at all swollen, or rather I should say that there is no swelling in the first instance. After a time there is a slight general enlargement of the joint, the consequence of a deposit of lymph or serum outside of the synovial membrane. The swelling assumes the shape of the articulating ends of bones, and appears greater than it really is; because the muscles of the thigh are wasted above, as those of the leg are below. The pain is aggravated by motion, and there is a painful starting of the limb at night. The pain is especially aggravated by pressure on the patella, and whenever, in this or any other case of disease of the knee, this symptom exists, you may suspect that the cartilages of the joint are beginning to ulcerate. The disease may go on not only for weeks but for many months, the patient's health suffering all the time, from disturbed rest at night and constant pain in the day, and yet without suppuration taking place. By and by matter forms, and there is

then an aggravation of all the symptoms. The matter, as in all other cases of abscess connected with the knee-joint, burrows in various directions among the muscles and tendons, making numerous and circuitous sinuses;—but it would be needless for me to repeat what I said on the subject of abscesses of the knee in the two preceding lectures.

With regard to the treatment of this disease, it is important, in the first place, that the joint should be kept in a state of the most perfect repose; and splints are required here as in other cases of chronic affection of this joint. Although I believe that issues, setons, blisters, and counter-irritants, actually do harm rather than good in some other cases of diseased joints, yet I believe them to be beneficial here. A caustic issue inserted on each side the patella will very often stop the pain and the starting of the limb at night, when other means have failed. Nevertheless issues are not always required, and in fact in the majority of cases they may be dispensed with. What, then, is the chief remedy to be employed? That which is used for chronic rheumatic inflammation elsewhere. Mercury given as for irritis or chronic inflammation of the testicle, is here productive of the greatest benefit. Two grains of calomel and one-third of a grain of opium may be exhibited three times daily till the gums are affected. The influence of this agent in stopping rheumatic ulceration of the cartilage is remarkable. In fact it very seldom fails, if given before suppuration is established.

But some patients cannot take mercury; either it disagrees with them, or for some other reason you do not like to give it; and then sarsaparilla or the iodide of potassium may be substituted with great advantage. Mercury, however, is on the whole the most efficient of the three remedies, and next to that, sarsaparilla. In many cases the best mode of proceeding is to exhibit mercury in the first instance, until the disease is arrested, and then follow it up by a course of some good preparation of sarsaparilla. The mercury should be exhibited till the gums are somewhat sore. If it disagrees in one way, try it in another. Mercurial ointment may be rubbed into the thigh in the usual manner, when the patient is unable to take it internally.

In the great majority of cases, as I have already stated, no other treatment is required than splints, to keep the joint quiet, and the putting the system under the influence of mercury first, and of sarsaparilla afterwards; and it is only if these remedies prove to be not sufficient that recourse need be had to caustic issues. I formerly used to make them in the first instance, and undoubtedly with benefit; but I do not recommend them in the first instance now, because I find that much more dependence can be placed on other treatment, and that it is well to dispense with a troublesome and painful mode of treatment if you can. Getting rid of their use in cases of inflammation of the synovial membrane, and where there is scrofulous disease of the joint, and employing them only now and then in this particular disease, you see how seldom it is necessary to have recourse to them.

If abscess be formed, it will require to be treated as in cases of scrofulous disease. Make a free opening, do not squeeze or compress the part, but let the contents of the abscess flow spontaneously. I have explained to you that the scrofulous disease of the joint occurs usually in children. This affection rarely occurs except in the adult. It is owing to this that when matter once forms the limb will seldom be preserved, there not being the same powers of repair in the adult as in a child.

If matter be not already formed, the limb may, I believe, always be preserved; and in the majority of cases the mobility of the joint also. Of course where the cartilage is extensively destroyed, this last good result must not be expected; and the patient must be well satisfied if he re-

covers with ankylosis. In severe cases of the disease, the progress of it towards ankylosis is sometimes very rapid. In one case, in which the symptoms were of a more than usually urgent character, the patient recovered under the mercurial treatment; but when, at the end of about three weeks, the symptoms had subsided, the knee was found to be completely fixed, and the mobility of it was never restored.—*Medical Gazette.*

OBSERVATIONS ON LIGATURES AND ANEURISMS.

By T. WILKINSON KING,

Lecturer on Pathology at Guy's Hospital.

[Continued from Page 45.]

SECTION III.—*The Hunterian method of tying vessels was not by cutting ligatures, and not the less secure. Great vessels closing spontaneously. Scarpa, C. Bell, and P. Crampton, for gentler ligatures.*

Mr. Hunter observed, that "in dogs the mere exposure of the tibial artery to the air for about an hour excited such a degree of inflammation and thickening of its coats as completely to obstruct its canal"* (On the Blood). Mr. Frere gives a very pretty drawing of the tibial of a horse, thickened and narrowed by recent active inflammation in consequence of pressure used experimentally. We see inflammations, granulations, and contractions, closing up wounded arteries of limited size, and shutting up the vessels in vomical cavities. Other facts of this kind will yet be adduced of even greater importance.

It is remarkable with what negligence authors in general have regarded solid principles, with respect to a fair and broad theory of closing arteries.

Considering all that yields to the blood pressure in arteries; how tegument, muscle, nerve, ligament, and bone, are eaten away under the pressure of an aneurism, or a new vascular growth; where shall we look for organic forces capable of resisting such tensions? The answer is not doubtful nor scanty.

A certain hypernutrient action, or a subsequent contraction, may, in spite of aortic distension, close up the orifice of the coronary artery, the carotid or intercostals, and narrow the aorta itself as well as all its main branches.†

The pulmonary artery being absent or imperfect, the bronchial branches of the aorta become extremely enlarged in consequence of the great facility with which the blood is transmitted, but this change is not extended equally to the orifices of the bronchial arteries, because the tensions of the aortic tube keep up that measure of nourishment in its coats which is common to the vessel in its normal state, and these tensions have more influence around the mouths of vessels than the forces which dilate the vessels themselves.

Such facts are neither solitary nor devoid of practical indications.

It is a similar effect when the tissues of organs become condensed almost to the exclusion of blood, and it is a momentous reflection, that very little more than granulations about an artery may contract to the blocking up of the channel.

The old mode of tying vessels gave 75 per cent. of successful cases. It does not appear that the new or tight ligature has had quite as great success when applied to the same vessel, namely, the femoral.

On the other hand, it may be said that, according to record, the tight ligature of still larger arteries has met with success nearly equal to that recorded for the Hunterian method applied in the thigh. Yet the records may deceive us here. In former times it seems almost to have been credit enough to record an operation, while in latter times, as to ligature at least, the operations may be said to be common, and the dignity of success more needful for an attractive narrative, unless the accounts involve rare particulars.

Modern successes, too, have depended something on less advanced stages of the disease.

Those who desire to have the clearest idea of the Hunterian ligature, must divest themselves of all modern notions, and read

* Crampton, Med. Ch. Tr. vol. vii. p. 345.

† There is a curious result of tension in the mouths of some arteries when unnatural changes occur in the direction of the circulating blood.

Hunter for themselves. Of course the surgeons of old knew something of the closure of minor vessels when cut, and of the surgery of amputations, and of laying open aneurisms.

I think it is abundantly evident, and hardly less ridiculous, that in spite of the distinct expressions of Hunter and his followers, Dr. Jones starts with the assumption that, if an artery be well tied, the inner tunics are divided.

I should say, judging from a series of human specimens, the repair internal to the vessel is nothing in any case. Dr. Jones made quite enough of external effusions to become organised [in which I have no present trust.] Mance seems to omit all idea of salutary external changes. Wishing to be brief even as to what is real and positive, I cannot dwell on these discrepancies.

A measure of unhealthy inflammation diffusing may disorganise some extent of arterial tunics to induration, contraction, and internal effusions, &c. Experiments on young animals may cause great effusions around the vessel; in other cases, human, &c., the inflammation or repair is most limited, as it should be. In a healthy body, parts alone should undergo changes as they have been injured, or are subjected to new physical or other influences.

In Mr. Hunter's third attempt to secure the femoral artery for popliteal aneurism, in a man *æt.* 35, a single ligature [including also the vein] was applied "so slightly as only to compress the sides of the artery together, sufficiently tight to prevent the pulse in the sac, without injuring the coats of the vessel." The separation took place on the 14th day, and on the 28th the patient seemed well. The sac suppurated, and the cure was complete by the 12th week. In his fourth case the vein was excluded; the thread separated on the 29th day. Suppurations and ague delayed the cure for three months. The fifth patient was a man, *æt.* 42; the ligature came off on the 11th day; he was well by the fifth week.

Mr. Lynn's patient was aged 25; a broad ligature was tied so as to cut off all communication with the tumor in the ham; it came off on the 13th day, and he was well in a month.

Mr. J. Earle, on a man *æt.* 50, employed a simple ligature, which separated on the 15th day, with the best success.

Sir E. Home's second or supplementary paper contains similar cases. He operated on a man, *æt.* 32: the thread came off on the 11th day, and the cure was complete in five weeks; and on a man, *æt.* 36, the separation was on the 12th day, and on the 28th he was well. Sir E. put a ligature on the femoral of a man, *æt.* 33; it came off well on the 12th day. On the 32d, the like was done to the opposite vessel, which, after thirteen days, gave rise to repeated bleedings. These were finally subdued by pressure, &c. Mr. Knight tied the vessel for a man, *æt.* 35. There was a slight hæmorrhage from the wound on the 23d day; the separation was on the 31st, and the cure speedy.

All these ten successes I must connect with the gentler ligature, although I may be wrong. It seems quite certain that they long preceded the odd discovery,† of what happens on tying an artery with force enough to cause that sudden giving way of almost every thing, which is felt almost unpleasantly even post-mortem.

It may be inquired what were the unsuccessful cases of the first Hunterian experiments? They are few and instructive, but by no means prejudicial to the main point of our present consideration. Sir E. Home's concluding case was that of a gentleman, who probably drank to the last. Active inflammation and bleedings on the 11th and 12th days were fatal. Hunter's first attempt is no slight lesson. A man, *æt.* 40, had four ligatures put on the femoral artery and veins at once, and tied so slightly as only to compress the sides of the artery together. On the ninth day there was bleeding; on the fifteenth a thread came off, recovery was only retarded by abscesses, discharging ligatures, &c. His second experiment was to use one ligature to both artery and vein to stop the current; it came off on the 14th, and on the 19th there was hæmorrhage. A fresh ligature led to fatal bleeding a few days afterwards. I conceive that if the wound had not been forcibly kept open the bleeding would not have occurred, or would have been trivial.

History must tell all that succeeded the true Hunterian operation. Ligatures thick and thin, round and flat, long and short, soft and hard, double or sixfold; ligatures of reserve and temporary, upon cork or plaster, or even with a bougie to plug the artery,

† Dessault in France, and J. Thompson in Scotland, share this honour. It was dividing arteries that introduced very tight liga-

tures, the presse artere, serre artere, &c. &c.; these were some of the modifications that had their day. England adheres to tight ligatures, and calls them Hunterian, without a shadow of authority. All Europe besides is at least less confident.

There is yet a good variety of experience in favour of gentle ligatures. There are abundant scattered cases of Hunter's immediate followers at home and abroad, and I have already shewn by the calculations of Mr. Phillips that the success was good.

Early in the seventeenth century M. A. Severinus once tied the femoral with success. In 1688, we learn that Bontentuit succeeded with a simple ligature, and before 1772 Guttani exposed the inguinal, and with graduated compress or roller closed it speedily. All this I attach to loose ligatures rather than cutting ligatures: always remembering, however, that inflammation of the arterial tunic may casually aid the final contraction of the orifices, while the indispensable sealing of the divided vessel in the absence of such casual contraction depends essentially on external fibrous tissue, as it were two little acorn-cups united by their stalks, new growth fashioned by definite tractions and tensions. The products of diffused inflammation around may be useless or mischievous, but the *shipping threads and bands stretched over and between the two compressed ends of the artery (as they gradually recede from each other), close over the separating ligature, and are invaluable and alone indispensable.* Violent inflammations may leave unequal consolidation and contractors even to obliteration of vessel for inches.

Scarpa's cases afford some remarkable evidence in favour of slow ligatures, and even of late hæmorrhage. (See his Appendix.)

For a man, *æt.* 23, he cut open a femoral aneurism, and applied two moderate ligatures. There was bleeding on the 11th day, stayed by his reserve ligature with compression. The separation was on the 20th, and the cure good [1794.]

A man, *æt.* 33, had a sixfold thread put on the femoral with one of reserve. They came away on the 16th: after the 20th there were bleedings, but the case did well. [Morigi, 1796.]

Scarpa's full plan was put in force on a man, *æt.* 42. Two sixfold ligatures with compresses were put on the femoral. They came away on the 18th. A little blood oozed from exertion, but the cure was speedy, [1800.]

The same is related of a man of 46, except that there was no bleeding, and that after the 36th day fatal sloughing supervened in the sac, [1799.]

And again, the same as the last, but the casting was on the 21st, and abscess only retarded recovery, [1804.]

One of his cases is harder to dispose of. The full treatment, if I may so speak, was applied near the profunda of a man *æt.* 34. The separations seem to have been near upon the 8th day, and without any mischief, [1803] What would the cutting ligatures have done?

His full plan was thrice employed with success on the brachial, the six ligatures separating all between the tenth and thirteenth days. Latterly Scarpa had again and again fair success by applying a loose ligature over plaster on the femoral artery only for four days. Just so likewise on the brachial artery.

Mr. P. Crampton applied a tape $\frac{1}{2}$ inch wide by means of a presse artere on the superficial femoral, and the tightening was stayed just as the tumor ceased to beat. After two hours the tape was somewhat loosened, and at the end of 24 hours completely relaxed. In due time the aneurism seemed to be fairly stopped. Mr. Dease did the like operation, continuing the pressure for about 24 hours. The cure seemed complete in a month. [M. Ch. Trans. vol. vii. 369.]

Sir C. Bell left a thread quite loose around the artery of an animal; the vessel became quite filled with clot, shewing that the coats inflamed.

Sir C. Bell [Institutes of Surgery,] distinctly observes, "it is not necessary to draw the ligature so as to cut the inner coats of the artery, and it is not safe, unless in young and healthy subjects, and in amputations. You will observe that the important consideration is, the mode in which a ligature should be employed in old arteries subject to aneurism. It is very dangerous to apply the experience acquired in operating upon the healthy subject to the subject of aneurisms—far less is it safe to draw conclusions from experiments on brutes! Any mode of securing the artery will do in the young and healthy individual, and in amputation."

The latitude which Sir Charles gave his pupils, seeing he was almost singular in his doctrine, may seem natural; yet, I must think it beyond the point of safety in any case. It would have been well to explain fairly what he deemed "folly." One mo-

dem declares for tight ligatures, even exaggerated with an adjective oath.

Sir Philip Crampton certainly ranks with Sir C. Bell as an opponent of the over-daring followers of Jones. We have seen that one-seventh of the successful ligatures have caused some hæmorrhage; and we have seen that the event is least serious when late; and we may at least contrast with the violent practice, the suggestion that, in a given proportion of cases, the most proper mode for the particular constitution to separate a considerable ligature, is with some bleeding; that is, that the surgeon should anticipate slow and scanty repair, and that all his views should be directed to such a mode of cure. At all events, it is most certain that both local and general circumstances are now and then imminently dangerous, in proportion as the ligature is made to destroy the scanty films which temporarily restrain the efforts made by the blood to burst out; and safe in proportion as the granulating repairs [or the like] are matured to shut the door after bleeding has diminished the irresistible tension within. Yet, no doubt, the fear of erosion, or wasting, may be greatest after depletions.

I imagine the successes of Assalini's forceps depend mostly on repair external at least to the lining of the artery, and certainly not on division of this membrane; and some decided evidence for gentle ligatures and correlative principles is found in the history of the serre-artere animal ligatures, and the practice of some able American surgeons, [Vide Reese's American edition of Cooper's Dictionary, and H. G. Jameson, in American Med. Recorder, 1837.]

It will be seen that I regard cure after hæmorrhage as a proof that the main sealing is external; and it will be understood that what I describe as the sole essential repair is consistent with the least of what is called inflammation, and the scantiest material re-formations. It is certain that the least diffused inflammation consistent with the injury done to a healthy body, is that on which we ought to reason when considering healthy repair; the more complicated case follows—thickening and contraction of vessel, in the second place; inflammatory changes within, in the third place.

I regard as quite insignificant the common, narrow, conical clot, small and loose, except that its base or nucleus is attached to an inorganic effusion the size of a pin's head, where the vessel is gathered up to a mere point.

MIDWIFERY.

ON THE CONTAGIOUSNESS OF PUERPERAL FEVER.

By SAMUEL KNEELAND, jun., M. D., of Boston.

In most contagious diseases, occurring in large cities, it is difficult to trace the communication from one individual to another, from the continual intercourse carried on; and therefore it is chiefly the physicians of such large communities who are the strongest opponents of contagion—but in puerperal fever the circumstance of a large population has no relation to the question of contagion, owing to the peculiar state of those exposed to its attacks. When, therefore, we see this disease occurring solely, or chiefly in the practice of a single physician, in a large town, to use the expression of Dr. Blundell, "stalking behind him, wherever he goes, like his evil genius," we are surely justified in attaching some importance to such facts.

This coincidence has been mentioned by most writers on puerperal fever, the opinions of some of whom we shall now mention.

Dr. Gordon, in his account of the epidemic which raged at Aberdeen in the years 1789 to '93, says, that he could foretell what woman would be attacked upon hearing by what midwife they were to be delivered; and that in almost every case his prediction was verified.

Mr. White of Manchester, observes, that he is acquainted with two physicians who have the whole business of midwifery in a large town divided between them; one loses several patients every year by the disease, while the other has not a single case.

Dr. Armstrong, in his essay on the Sunderland epidemic, mentions several instances of a similar confinement of the disease to one man's practice.

Drs. Ramsbotham, Lee, Gooch, Robertson, Hutchinson, Blundell, and a host of others, mention the fact of the greater number of observed cases occurring in the practice of single individuals, while their medical brethren had no cases.

Dr. Holmes (in the *N. E. Quarterly Journal of Med. and Surg.*) speaks of a series of cases in an American journal, observed by Mr. Davies, who says, "in the autumn (1822) he met with twelve cases, while his medical friends in the neighbourhood did not meet with any, or at least very few." He also mentions nine cases occurring in the practice of Dr. Pierson of Salem, in 1829. "Up to this period," says the latter, "I am not informed that a single case had occurred in the practice of any other physician," admitting, however, that his information may have been defective. In a letter addressed to Dr. Storer, it is stated: "about three years since, a gentleman in extensive midwifery business in a neighbouring state, lost, in the course of a few weeks, eight patients in child-bed, seven of them being undoubted cases of puerperal fever. No other physician of the town lost a single patient by this disease during the same period."

In the *American Journal of the Medical Science* (for Oct., 1842, p. 410), is the quarterly summary of the Transactions of the College of Physicians of Philadelphia, from which we extract the following: Dr. Condie, having alluded to the prevailing puerperal fever of a peculiarly malignant character, observes that, "in the practice of one gentleman, extensively engaged as an obstetrician, nearly every female he has attended in confinement, during several weeks past, within the above limits, had been attacked by the fever." If it be not contagious, "how otherwise can be explained the very curious circumstance of the disease in one district being exclusively confined to the practice of a single physician, a fellow of this college, extensively engaged in obstetrical practice—while no instance of the disease has occurred in the patients under the care of any other accoucheur practising within the same district. Scarcely a female that has been delivered by this gentleman for weeks past has escaped an attack."

These and many other authorities which might be adduced, prove beyond doubt the singular confinement of the disease to one man's practice. But how can this fact be explained? We should not wonder at it if occurring in the pestilential wards of an hospital; but in private practice it is certainly very strange, that the disease should follow precisely the steps of one unlucky individual, tracing him far and wide with the certainty of a blood-hound. Considering the exceeding rareness of the disease (comparatively) even in private practice, it certainly seems impossible to attribute to mere chance the numerous well-authenticated instances of the above fact; they who can swallow the immense improbability of the doctrine of chance in this matter need hardly strain so much at the infinitely less improbability of contagion.

Clearly connected with the above fact is the question of its propagation by being carried by physicians and nurses. The whole question turns upon this; for although the succession of effects to causes does not absolutely prove the dependence of the former on the latter, if it can be shown that the instances of contagion (supposed) occurred very much more frequently (and in many instances solely) where there was communication between a physician and those affected, and where there was no such communication, are we not justified in a measure in establishing the relation of causation? The question of the contagious nature of a disease, according to Dr. Alison, (and the same reasoning may be applied to all sciences, as well as medicine,) always ultimately turns on a calculation of chances. "The question always comes to this—is the circumstance of intercourse with the sick followed by the appearance of the disease in a proportion of cases so much greater than any other circumstance common to any portion of the inhabitants of the place under observation, as to make it inconceivable that the succession of cases occurring in persons having that intercourse should have been the result of chance? If so, the inference is unavoidable that that intercourse must have acted as a cause of the disease. All observations which do not bear strictly on that point are irrelevant, and in the case of an epidemic first appearing in a town or district, a succession of two cases is sometimes sufficient to furnish evidence, which, on the principle I have stated, is nearly irresistible."

Let us submit this question to the test of Dr. Alison, by quoting the opinions of some of the best authors.

Dr. Gordon says, "the disease seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients affected with the disease. I had evident proofs that every person who had been with a patient in the puerperal fever became charged with an atmosphere of infection, which was communicated to every pregnant woman who happened to come within its sphere. It is a disagreeable

declaration for me to mention, that I myself was the means of carrying the infection to a great number of women."

Dr. Gooch mentions the case of a physician who had several deaths in his practice from this disease in quick succession; when thinking he might have carried the contagion in his clothes, he changed them and had no more cases.

Dr. Ingleby, in the *Edinburgh Medical and Surgical Journal*, (vol. 49, p. 415,) observes, that Dr. Campbell thinks that, "unless the practitioner has been engaged in the dissection of the bodies of those who have fallen victims; the disease cannot be conveyed by him from females labouring under it to others recently delivered." "But the fact of the extension of the disease in this manner from the living subject has been most satisfactorily shown by Mr. Robertson. I have repeatedly observed the same myself, and have submitted a number of cases in illustration. I have also adduced the strongest ground for believing that the effluvia derived from the body of a woman who died from puerperal fever were conveyed in this manner by two practitioners, and rapidly produced the same disease in two other females."

Drs. Blundell, Abercrombie, Ramsbotham, and many others, assert that the contagion is often carried about by physicians and nurses, and many of them accuse themselves of having been the vehicles of its conveyance. When to this testimony is added the hundred fold more which is buried with the hapless victims, we have an amount of facts which it is obstinacy or willing blindness not to perceive the force of.

But, unfortunately, we have no need of crossing the Atlantic to search for evidence of this fearful truth.

In the *American Journal of Medical Sciences*, (Oct. 1842,) Dr. West stated some facts communicated to him by Dr. Jackson of Philadelphia, who, when practising in Northumberland County, had seven cases of delivery in rapid succession, in all of which puerperal fever supervened; of which five proved fatal. "Women," said he, "who had expected me to attend upon them, now becoming alarmed, removed out of my reach, and others sent for a physician residing several miles distant. These women, as well as those attended by midwives, all did well; nor did we hear of any deaths in child-bed within a radius of fifty miles, excepting two, and those I afterwards ascertained to have been caused by other diseases. I now began to be seriously alarmed on the score of contagion. Although I had used some personal precautions before, I now feared that they had not been sufficient."

Dr. Holmes, in the journal before alluded to, gives a series of cases occurring in the practice of a physician of a town at some distance from Boston. "There were seven cases, which happened between the 20th of March and the 8th of May; the first five proved fatal in from four to seven days after delivery; the other two recovered. These were the only cases attended by this physician during the above period; and no other cases of a similar character with those of Dr. C. occurred in the practice of any of the physicians in the town or vicinity at the time."

In the first letter to Dr. Storer, (in the same article,) is found the following statement: Between the 10th and 28th of February, 1830, "I attended six women in labour, all of whom did well, except the last, as also two who were confined March 1st and 5th. Mrs. E., confined Feb. 28th, sickened and died March 8th. The next day, the 9th, I inspected the body, and the night after attended a lady, who sickened and died on the 16th. The 10th, I attended another, Mrs. G., who sickened but recovered. March 16th, I went from Mrs. G.'s room to attend a Mrs. H., who sickened and died on the 21st. The 17th, I inspected Mrs. B. On the 19th, I went directly from Mrs. H.'s room to attend another lady, who also sickened and died on the 22nd. "Up to the 20th of this month I wore the same clothes. I now refused to attend any labour, and did not till April 21st, when having thoroughly cleansed myself, I resumed my practice, and had no more puerperal fever. These cases were not confined to a narrow space. The two nearest were half a mile from each other, and half that distance from my residence. There were no other cases in their immediate vicinity which came to my knowledge. Of the six cases you perceive only one recovered."

The successive communication of the disease by single physicians and nurses to their patients cannot always be thus satisfactorily traced; and hence the non-contagionists maintain that the doctrine of exclusive, if not of occasional contagion, must fall to the ground. Without again showing that this negative kind of evidence is of no weight against the more positive kind which has been now brought forward, or that we do not argue for the exclusive contagion of puerperal fever, we shall merely state for the

careful study of those who consider this argument as unanswerable, that it is quite as strong for small-pox and other contagious diseases, in which this communication cannot always be satisfactorily traced. Of a like unsatisfactory and negative character is such reasoning as the following: in the Philadelphia epidemic of 1842, the physician in whose practice most of the cases occurred, says that he cannot easily believe in the transmission of the disease from female to female by a contagion conveyed in the person or clothes of a physician, because having absented himself from the city after the occurrence of the disease in his practice for a week, and on returning, having entirely changed his clothes, his first case of labour was followed by a fatal attack of the fever.

It becomes an interesting question for the conscientious physician how the contagion is conveyed; as by the knowledge of this he is enabled to regulate the conditions of his intercourse with his patients. Is this disease transmitted by direct inoculation, by the atmosphere carried about by the physician, from patients before death? and is it not also conveyed from the examination of the bodies of the deceased? As to the channel by which the poison enters the system, it is probably both by the vascular and respiratory system in the majority of cases; by the uterine surface and by the lungs. Many have limited its introduction to the "wounded surface" of the uterus—but that the blood is frequently poisoned from its entrance by the lungs, is shown by the fact, that puerperal fever (though the term be misapplied,) may seize a woman before delivery; or that the poison may be introduced, and produce the symptoms of this disease, before the local action in the uterus has taken place. Mr. Ingleby says the attack may commence before delivery, and that he has seen a single case. We think we have seen another—a woman entered the Hospital de la Faculte, in the service of M. Paul Dubois, with all the symptoms of puerperal fever of the low type which then existed in the city; she was in the sixth month of her pregnancy, and had never carried a child to the full term; she had the "facies of puerperal fever," with the abdomen moderately swelled and painful, though not exquisitely so; weak pulse, and great prostration. On examination the os uteri was found dilated, and delivery imminent; a few hours after she was delivered of a child, dead, though not at all putrefied; the symptoms were aggravated after delivery, and death took place in a few hours. At the autopsy, the peritoneum presented evident traces of inflammation, its cavity containing the milky fluid, with membranous flocculi found in the form of the disease, we shall hereafter mention as the erysipelatos; the uterine veins contained pus, and the placental insertion presented that softened, semi-putrid aspect, which the Germans have called "putrescentia uteri."

It probably principally affects the blood, though its first violence may often fall upon the nervous system. It is impossible to determine this point, which is fortunately of secondary importance.

It is, doubtless, very often propagated by direct inoculation from the living subject. Dr. Rigby observes, in his *System of Midwifery*, "the discharges from a patient under puerperal fever are in the highest degree contagious." "The puerperal abscesses are also contagious, and may be communicated to healthy lying-in women by washing with the same sponge; this fact has been repeatedly proved in the Vienna Hospital." He also observes, that they are also communicable to unpregnant women; and that frequently abscesses and diffuse inflammation attacked those who washed the bed-linen soiled by the discharges. Of the acrid nature of these, the following case from the *Dublin Journal of Medical Science*, (Nov., 1844,) will afford sufficient illustration.—It was here necessary, for the removal of the retained placenta, to pass each arm in succession into the uterine cavity, where they were tightly grasped by the neck. Two days after pustules appeared on the arms, one of them being surrounded by a livid base, the part of the arm near it becoming hard and swollen. The writer hence concludes that a morbid poison was generated epidemically in the blood, contact with which communicated the taint, and converted a common furuncle into a malignant pustule; and this altered condition of the blood was sufficient to produce all the phenomena of puerperal fever in the patient who was, however, free from the common symptoms of uterine inflammation.

Such being the malignant character of these discharges, that even those who have washed the linen soiled by them have communicated the disease to others, and that even the simple operation of passing a catheter has been the cause of propagating it, we should naturally expect to find the fluids after death of a peculiarly virulent nature. Many cases are on record of the most serious accidents from wounds received in post-mortem examina-

tions of puerperal fever; these cannot be accounted for on the supposition of a common poison acting on an enfeebled constitution, as several cases happen in the course of every epidemic; whereas such cases are comparatively rare in diseases infinitely more common, and in which the same enfeebled condition of the system is always a predisposing cause. From the comparatively greater frequency, then, of serious and even fatal symptoms in autopsies of this affection, we are compelled to recognize the existence of a most deadly and peculiar poison. This is so deadly that no wound is necessary, in order that the fatal effect may take place. Dr. Duncan, in the Transactions of the Medico-Chirurgical Society of Edinburgh, mentions an instance, where Dr. Cumming was present at the dissection of one who died of puerperal fever. "He took no share in the dissection, excepting introducing a fresh thread into the needle which was employed in sewing up the body, and was not aware of any abrasion, or of having punctured himself in the act of threading." In about a week after, he experienced an uneasy sensation in the middle finger of the left hand, where was discovered "an angry pimple." Death took place on the 11th day, with evident signs of a profound alteration of the blood.

With these startling facts before us, we are prepared for the long list of victims to the physician's ignorance and negligence; a list which we fear is yet to be swelled before the fearful truth we have endeavoured to bring forward shall be graven on the tablets of medical science.

From the mass of authority showing that the contagion of puerperal fever is liable to be conveyed by physicians to their patients, from their being present at, or taking part in the post-mortem examinations in this disease, we shall select enough, we trust, to convince the most sceptical.

In 1821, Dr. Campbell of Edinburgh, attended the autopsy of a married woman, who died of puerperal fever, after an early abortion; he removed the pelvic viscera and external parts, and carried the whole in his coat pocket to his class room; the next morning, having on the same clothes, he assisted, with some of his pupils, at an instrumental delivery at Bridewell; this woman was attacked with puerperal fever, and died (in the autumn). The same night, he went with another physician to deliver a woman, who also died; three others shared the same fate. Similar instances occurred in his practice in the summer of 1823; assisting at a dissection of this disease, (at the time having no cases of it,) from the poverty of the people, he could not properly wash his hands; without any farther attention, he went, when he returned home, to two cases of labour; both were seized with the disease, and died. Other physicians of the city, who had similar misfortunes, convinced of the contagious nature of the disease, gave up for a time the practice of midwifery.

Dr. Rigby, in the Library of Medicine, (vol. 6.) says that it is highly unsafe for one to attend a case of midwifery after a post-mortem of puerperal fever; and that it is impossible to remove the smell from the hands for several hours, even by frequently repeated washing.

In the *British and Foreign Medical Review*, (Jan. 1842,) the same author states in a review of Dr. Kiwisch's work, on the diseases of child-bed women, (who does not believe in the contagion of this disease,) that a young physician, contrary to advice, examined the body of a woman who had died from puerperal fever; there was at the time no epidemic; the case seemed to be purely sporadic; three other women were soon after delivered by him, all of whom died of this affection, the symptoms of which broke out soon after delivery. "The patients of his colleague did well, except one, where he assisted to remove some coagula from the uterus; she was attacked in the same manner as those whom he had attended, and died also; we trust that this fact alone will forever silence such doubts, and stamp the well-merited epithet of "criminal," as above quoted, upon such attempts."

Dr. Gooch remarks: "A practitioner opened the body of a woman who had died of puerperal fever, and continued to wear the same clothes. A lady whom he delivered a few days afterwards was attacked with, and died of a similar disease; two more of his lying-in patients, in rapid succession, met with the same fate."

The following is a still more striking example: Dr. Merriman, as quoted by Dr. Holmes, "related an instance occurring in his own practice, which excites a reasonable suspicion that two lives were sacrificed to a still less dangerous experiment. He was at the examination of a case of puerperal fever at two o'clock in the afternoon. He took care not to touch the body." At nine o'clock the same evening he attended a woman in labour; she was so

nearly delivered, that he had scarcely anything to do." She died in forty-eight hours; and the child also died of *erysipelas* two days afterwards.

In the *London Cyclopædia of Practical Medicine* are several cases of physicians who had examined the bodies of those affected with this disease, all of whose patients, in quick succession, fell victims to a similar affection.

In the *American Journal* (above quoted) it is stated that Dr. Warrington examined the body of a woman who died of this disease, and laded out the contents of the abdominal cavity with his hands. A few days after he was called upon to deliver three women in rapid succession. One was attacked with metritis, another with partial peritonitis; both were very sick, but recovered: in the third case, the patient was seized with peritonitis, and died on the fifth day. Two other women in his practice were also attacked with it, and both died. He would not be present at the autopsies of these cases for fear of communicating more readily the disease.

From the above series of facts, carefully collected and fairly stated, let every one reason for himself, and we think he will come to the same conclusions.

We think we may deduce the following propositions, from a careful examination:—

1. From the confinement of cases to the practice of single physicians and nurses in populous cities; from the fatal results attending post-mortem examinations; from its ravages in hospitals; that puerperal fever is contagious; that it may have other modes of propagation, in certain states of the atmosphere, and among strongly predisposed individuals; but that the fact of its conveyance by practitioners attests its contagiousness.

2. That it may be propagated by direct inoculation with the fluids of the living and the dead; by the effluvia arising from the bodies of the sick, inhaled in the very chamber of death, (as in the wards of an hospital,) or carried about by the person of the physician; by clothes, bedding, (fomites,) which have been in contact with a diseased individual.

3. That the order of propagation from the physician to the patient, and the regular succession of cases, show that the epidemics of puerperal fever are, in almost all cases, the effects and not the causes of the contagion.

4. The contagion acts according to the frequency of communication between the physician or nurse, (in whose practice are cases,) and lying in women, independently of insalubrity of places, wretchedness of patients, or the neighbourhood of dwellings—for although poverty and misery seem to predispose to it, communication is none the less fatal to the higher classes.

5. A case, to all appearance sporadic, may communicate the disease; a mild case may communicate a severe disease; and *vice versa*.

6. Immunity proves nothing against contagion; it may be the effect of an acquired or temporary inaptitude—it is equally inextinguishable in all contagious diseases.

7. The rapidity of its propagation shows that it is contagious at the commencement; the fatal results of attending autopsies indicate this character after death.

8. That a physician should not make, or be present at an autopsy of this disease; or, if he does, should take proper measures to cleanse himself and dress, for the safety of his next patient—that if a case (or several cases) occur in his practice, he should consider himself, in the language of Dr. Holmes, "a private pestilence," and regulate his conduct accordingly—that persons who have washed, or have otherwise handled the clothes or bedding soiled by the discharges of this disease, should not approach, much less nurse a woman after delivery.

9. That when the disease is prevalent, a prompt removal from possible intercourse with a "pestilential" physician, and a strict attention to ventilation, cleanliness, quiet, proper food, &c., are the dictates of a reasonable fear.—*American Journal of the Medical Sciences*.

CASE OF RACEMIFEROUS HYDATIDS OF THE UTERUS.

The following case, reported in a recent No. of the *Philadelphia Medical Examiner*, by Dr. J. K. MITCHELL, presents some points of interest. We give it slightly abbreviated, but pretty nearly in the authors words:—

"On the 10th of July I was called to the case of Mrs. T—, who had returned a few days before from a visit to the South

She complained of nausea, such as usually affects females during inter-gestation, but of greater intensity and prolongation. There was also an unusual degree of tenderness to the touch in the hypogastric region, extending to the right iliac fossa. A careful examination of the part by palpitation presented no unusual conformation; induration or tumefaction. The history of the case led to the supposition of the existence of a pregnancy of about a month's duration, as, previously to that period, her catamenial regularity and perfect health left no doubt of an unimpregnated condition.

"Aperient medicines, to regulate a costive state of the bowels, and antacids, for an acid condition of the stomach, with sinapiens as revellents, relieved the more pressing symptoms. On the 18th of July my attention was called to a small tumour on the right side, about half way from the *symphysis pubis* to the anterior superior spinous process of the *os ilii*, in a right line. It was then about the size of a turkey's egg. The part was painful to the touch, ached when at rest, and suffered from attempts to alter the position in bed. There was a remarkable frequency (120) of the pulse, some heat of surface, and an anxious expression of countenance. The tongue was dry, but clean, the thirst moderate, the nausea, irrepressible; and slight mental incoherency, with restless movements of the head and hands, indicated much disturbance of the innervation.

"The application of leeches and a poultice relieved in some measure the local suffering, and an antispasmodic prescription abated the restlessness.

"On the 22nd of July, the *uterus* was perceptibly enlarged, occupying a position entirely to the right of the median line, and extending from the place of the tumour first discovered to the *symphysis pubis*.

"On the 28th, it was found that the rapid increase in the size of the *uterus* had obliterated the exterior vestiges of the lesser tumour, and that the former occupied the whole of the right hypogastric region, and rising above the umbilicus, extended a little way to the left of the *linea alba*.

"Irritation, and probably pressure suddenly produced, interfered with the power of micturition, and a catheter was used to withdraw the urine, of which the quantity was scanty, and the quality offensive.

"The uterus had by this time acquired such a size as to fill nearly the whole abdominal cavity on the right side, while it extended about two inches to the left of the *linea alba*, without any obliquity in the position of the *os tinca*, to explain the presence of the body of the *uterus* on the right side above.

"The history of the case, the short period of time since the cessation of the *menses*, the singular tumour on the right side, and the preternatural rapidity of the development of the uterus, rendered the *diagnosis* obscure; but on the whole, we were disposed to believe that a dropsy of the right ovary had extended to the uterus, or that there was a rapid production of a mole in *utero*. The absence of any *fremitus* on percussion, and the escape of a little mixed blood, misled us to hydatids; and the rapidity of development, and failure to excite motion, left no doubt as to the absence of a *fœtus*.

"On the 7th of August contractions of the *uterus*, with the usual pains, announced expulsive efforts, and in the course of the night an immense body of hydatids were expelled. There were many thousands of these vesicles attached to each other, or to a common membrane, so as to appear like bunches of grapes. They varied in size from almost imperceptible globules to the dimensions of large grapes. A few had acquired the volume of a pigeon's egg, while one or two were as large as a hen's egg. They were transparent, uniform, and without nucleoli or apparent organs, and might be properly termed *racemosa acephalocysts*.

"Hæmorrhage and after pains, as in ordinary cases of labour, followed the expulsion of the hydatids, without causing any abatement of the abdominal tenderness or frequency of pulse. On the following day signs of puerperal peritonitis became obvious.

"On the 9th the case ended in death, and in thirty two hours thereafter an autopsy took place.

"On opening the cavity of the peritoneum it was found to contain about ten ounces of turbid serum, mixed with pus, of which latter a less diluted portion was found in the pelvic cavity. The right ovary was completely disorganized, nothing having been left of it but the exterior membrane, which was found ruptured, and appeared to have been filled with pus, of which a part still remained. The left ovary was enlarged and softened. It presented, when cut into, a very beautiful, perfectly developed, *corpus luteum*.

"The uterus was about the size of that organ as it is usually

found a day or two after delivery. The interior presented a rough surface at the fundus, as if their had been an attachment of the membrane or of some of the hydatids to it, and that part was partially covered with coagulated blood. The cervix was of an unusually dark hue, but not softer than usual.

"This case is interesting for several reasons—

"1st. Because it gave no signification of its character by the discharge, from time to time, of single vesicles, or by intermittent gushes of water, produced by their accidental rupture, an event not unusual in such cases.

"2nd. Because it was obviously a consequence of impregnation; a blighted ovum having given origin to the disease, as evinced by the presence of the membranes, to which the vesicles were attached, and by the perfect development of a *corpus luteum*.

"3rd. Because of the very rapid development, first of an ovary, then of the uterus.

"4th. Because of the severe constitutional disturbance, which, as proved by the history of other cases, marks the presence of hydatids *in utero*, and is not commonly found either in uterine dropsy or pregnancy.

"5th. Because there remained *no traces* of a *fœtus*, and no vestiges of an ovum, except the transparent membrane to which the vesicles were attached; the most careful examination of which could not, *per se*, have given evidence of an ovarian origin."

CHEMISTRY, MATERIA MEDICA AND PHARMACY.

ON THE BEST MEANS OF DISGUIISING THE TASTE OF NAUSEOUS MEDICINES.

By Wm. Acton, Esq.

Surgeon to the Islington Dispensary, and formerly Externe to the Veneral Hospital of Paris.

As I am in the daily habit of prescribing those peculiarly nauseous substances copaiba and cubeba, my attention has been constantly directed to render these medicines as palatable as possible, without in the least interfering with their efficacy; and in doing so I have tried most of the plans recommended by different practitioners. In the belief that my experience might interest the society, I have been requested to write a short paper embodying my views on the subject, as they are applicable to a large number of nauseous or bulky substances; and with this view I venture to call the attention of the meeting, first, to a few of the old-fashioned methods of prescribing these remedies.

An amusing collection of looches, electuaries, and mixtures might be made from the old dispensatories. Fashionable physicians of the last century appear to have thought of only one way of disguising the taste or odour of nauseous medicines, by prescribing largely aromatic water, essential oils, honey or syrups. Need I say this plan has now been given up, or only followed by the remains of the gold-headed cane school, who still seem to adhere to the motto,

"Omne tulit punctum, qui miscuit utile dulci."

The Young England school of pharmacy began their improvements by introducing certain *culinary innovations*, among others stands prominently forward the

COPAIBA CUSTARD,

which we are told should be made in the following manner:—

℞ Vitelli ovi no. j. Bals. copaib. ʒiv.

Aq. flor. aurant. ʒij.

Syr. tolu. ʒss.

Aquæ distil. ʒij.

• Ess. limon. gtt. iij.

It is to be regretted that this Esculapian Ude has not furnished posterity with his name; were he alive, however, I fear his former patients would recommend him to confine his practice to the servants' hall, as his *extremets* are not adapted to the present fashionable style of cookery.

Other *pharmaceutical artistes* have turned their attention to concentrate the powers of nauseous medicines in extracts and essential oils, thinking thereby to do away with bulk and flavour, but they soon found that when they destroy the flavour, they are obliged to increase the quantity. In illustration I beg to give the prescription of a physician, brought to me a few weeks ago by a patient who had been under his care for four months:—

℞ Ext. copaibæ resinosa, ʒij. Ext. cubebæ, ʒj.
Ol. essent. cubebæ, ʒij. Pulv. glycyrrhizæ, gr. xij.
Mucilag. q. suf. M. ft. mass. et in pil xxxvj. divid.
Sumat. iv. ter die.

Twelve pills a day!! Why, surely a statute should be raised to the martyr who thus supports the pill trade! Staticians would tell you that this individual took 84 a week; 336 a month, or 1344 during the four months, and, as the poor fellow told me with a sigh, all to no purpose. When he related his tale, I was disposed to ask him, as Mr. Adolphus, the barrister, did a witness (who came forward on a trial to speak of the efficacy of large numbers of Morison's pills,) how he managed to swallow them: was it by the aid of a shovel, or a coal scuttle? for without such aid he (Mr. Adolphus) was unable to conceive it possible to bolt these "monster" doses; but, perhaps, this is the new plan of giving physic to the "million," and not adapted for private practice.

But to be serious; these means are now seldom resorted to, and modern surgeons in private practice find it only necessary to resort to some expedients for bulky or nauseous solids, and one of two others for liquids. The best plan of giving solids is by means of

WAFER-PAPER.

This paper, according to Dr. Ure, is made in the following manner:—"A certain quantity of fine flour is to be diffused through pure water, and so mixed as to leave no clotty particles. The pap is not allowed to ferment, but must be employed immediately it is mixed. For this purpose a tool is employed, consisting of two plates of iron, which come together like pincers, or a pair of tongs, leaving a small definite space betwixt them. These plates are first slightly heated, greased with butter, filled with the pap, closed, and then exposed to the heat of a charcoal fire. The iron plates being allowed to cool, on opening them the thin cake appears dry, solid, brittle, and about as thick as a playing card." We meet with it in small sheets, of a light colour, breaking easily when dry, but tenacious and moulding itself easily to the substance it covers when wet, increasing but slightly its bulk. When any powder is to be taken, it must be mixed with syrup or other tenacious substance to the consistence of a bolus, and the patient be desired to break off as much of the paper as may be necessary to envelope the substance, dip it (the paper) in water, lay it on a plate or clean surface, and then place the electuary in its centre, fold the corners carefully over it, and swallow it by drinking a little water. Some persons have suggested putting the powder on the paper, and folding it without wetting the powder. This I should not, however, recommend, or an explosive mixture might result, much to the disgust of the patient and to the injury of the method. Those who are unable to swallow pills can manage to bolt these boluses covered with wafer-paper; they slip down the throat easily, as would an oyster, and do not produce that convulsive action of the muscles of the larynx and pharynx which frequently attend the effort of swallowing pills. I would strongly recommend the use of the wafer-paper as an envelope for scammony, when prescribed for children, a medicine so frequently producing nausea. It is equally applicable for taking the pulv. jalap comp., or any other substance prescribed in ʒ or ʒss doses.

The chemist must take care not to make the electuary too soft, or the object would not be attained.

The best modern method of giving nauseous liquids is in the form of

CAPSULES.

Of these I find no end of varieties; but I fear the majority of the makers of such useful articles have not a very clear idea of the objects sought to be attained. Need I say, that it is of the greatest importance to employ genuine copaiba? The next important point is to obtain a capsule of a certain definite size, so that we may know what dose the patient is taking, and which the surgeon is generally unable to do. Another circumstance to which the manufacturer gives but little attention, is the thickness of the capsule. I would recommend the chemist to reject all samples that are not an eight of an inch thick. In many instances I have known the capsule burst in the effort of swallowing, or dissolve as soon as it is in the stomach. I have called the attention of the profession to the subject in my work on Venereal Diseases, and must refer those curious on the subject to p. 61. An improvement has lately been introduced by enclosing copaiba in membranes, thus obviating many of the objections to all gelatine capsules. I am told that these membranous capsules are in the hands of respectable parties, who make a point of filling them

with genuine copaiba. I would suggest, however, to the patentees to increase the size, and make them uniform, or the surgeon will return to the gelatine capsules, which, when properly manufactured, answer the purpose.

The chemist should, in the selection of his capsules, take particular care that no one of them leaks, or the odour of the oil will be rapidly communicated to the others, and our object in giving copaiba in this way frustrated. The patient should be told likewise to take his capsules after meals. By this means the gelatine will not immediately be acted on by the gastric juice, and those unpleasant adjuncts to copaiba, eructations, will not be experienced. Many persons will tell you they are unable to take pills, and feel convinced they will be unable to swallow capsules; recommend such sceptics to take about a dessert spoonful of water in their mouth, and then place the capsule on the tongue, when the whole will be swallowed without difficulty, whereas if the capsule be placed on the tongue and water be drunk, the patient will often swallow the water, but the capsule will remain and produce convulsive action of the pharynx. Given in this way, it is singular how soon the medicine will act and effect the purpose we have in view; and it is no less remarkable that the stomach becomes tolerant of the medicine—a patient has not that tell-tale face so often characteristic of one taking nauseous medicines. I shall not venture to describe the gilding of pills, or the introduction of fluids into the back part of the throat by means of glass tubes, but may refer to an excellent plan of covering pills and boluses with gelatine, as mentioned in a former number of the *Pharmaceutical Journal*. In this last way, however, the pills or boluses are not able to mould themselves to the form of the throat, and the plan is far inferior to the wafer-paper, which I hope to see more usually introduced than at present, when bulky or nauseous medicines are to be given.—*Pharm. Jour.*

PETRIFIED FOREST NEAR CAIRO.

The following particulars are from an account given by Dr. Buist, of Bombay, in explanation of some specimens of silicified wood presented by him to the Literary Society of St. Andrews—"The specimens consisted of about forty-five pieces of wood; trunks, roots, knots and branches, from three inches to three feet in length; some were exhibited sliced and transparent, showing the sap vessels and the medullary rays; some cut into bracelets and brooches. In explaining the peculiarities of these, Dr. Buist stated that few things were more remarkable—few less noticed, [considering how worthy it was of examination] than the petrified forest near Cairo. From the city you proceeded, by the Caliphs' Tombs, to the southeast. Passing for five miles through an arid valley, through which a river torrent appeared to have flowed, skirted on both sides by low, brown, rocky ridges, the traveller turns suddenly off to the right, and beyond the first range of sand hills, finds, spreading far as the eye can reach, a vast expanse of rolling hillocks, covered with prostrate trees. At first sight, these wear exactly the aspect of rotten wood dug out from a Scottish or Irish peat-bog. The color and the amount of decay seem the same. They are lying in all positions and directions on the surface of the burning sand—some forty or fifty feet in length, and one or two feet in thickness; not continuous or entire, but in a line broken across, left in their places like sawn trunks. On touching them, instead of proving mouldering and decayed, they turn out to be hard and sharp as flints. They ring like cast-iron, strike fire with steel, and scratch glass. The sap-vessels and medullary rays—the very bark and marks of worms and insects, and even the spiral vessels, remain entire; the minutest fibres of the vegetable structure are discernible by the microscope. Here you have the carbon—the most indestructible matter known to us—entirely withdrawn, and substituted in its place a mass of silica—a matter insoluble by any ordinary agent, and at any common heat. Yet so tranquilly has the exchange been accomplished, that not one atom has been disturbed; the finest tissues remain entire—the most delicate arrangements uninterfered with. The limits of the petrified forest are unknown: it probably extends over an area of many hundreds, perhaps thousands of miles. It has never been described with any care, and, extraordinary as it is, has excited very little attention. The trees are scattered loosely and at intervals over the desert, all the way from Cairo to Suez, a distance of 86 miles. No theory of their silicification or their appearance where they are found, has ever been attempted. The late Dr. Malcolmson found fragments of the wood imbedded in the conglomerate which contains the Egyptian jaspers, and

hrew it out as possible that they and the gravel of the Desert, consisting almost entirely of jaspers, might possibly be the result of abrasion or denudation. This throws the difficulty only one step further back; besides this, that the appearance of the forest is at variance with the theory. No agates or gravel appeared around: the trees seemed to have been petrified as they lay; they looked 'like a forest felled by mighty winds.' A further mystery was this: they lay on the surface of bare drift sand and gravel, and reposing on limestone rocks of the most recent tertiary formation—the texture and color of the imbedded oyster shells were as fresh and pure as if brought not six weeks from the sea."—*Athenaeum*, Jan. 1846, p. 130.

CHEMICAL CHANGES PRODUCED BY THE ACTION OF THE SOLAR RAYS, OR ACTINO-CHEMISTRY.

By ROBERT HUNT.

At a meeting of the British Association at York, it was proposed by Sir John Herschel, that all those phenomena, which exhibit change of condition under the influence of the solar rays, should be distinguished as forming a peculiar province of chemistry, and be designated by the term Actino-chemistry; this was generally approved by the chemical section.

Accordingly, the sun's rays are divided into those producing light, those producing heat, and those producing an actinic influence.

Mr. Hunt, in his experiments, confirms a fact first pointed out by Sir John Herschel, that the rays of the sun facilitate precipitation.

A solution of manganate of potash having been made in the dark, was placed in two glasses and set aside. After having been kept in darkness for two hours, the solutions remained as clear as at first. One of the vessels with its contents was then removed into the sunshine, when the solution immediately became cloudy, and was very speedily decomposed, the precipitate falling heavily. By experiments with the spectrum, the author found that the precipitation was due almost entirely to the most refrangible rays.

A few grains of sulphate of the protoxide of iron were dissolved in rain water; if kept in perfect darkness, the solution remained clear for a long time; it became, however, eventually cloudy and colored from the formation of some peroxide of iron, even in tubes hermetically sealed. A few minutes' exposure to the sunshine is sufficient to produce this change, and the oxide formed, instead of floating in the liquid, and as in the former case rendering it opaque, falls speedily to the bottom.

Mr. Hunt made some experiments, [particularly one with a mixture of the bichromate of potash and the sulphate of copper,] in which precipitation appears retarded by solar agency, and he is inclined to think that it will eventually be proved that the electric energy of the different bodies in relation to each other, will greatly modify the results obtained in these experiments.

The action of the sun's rays appears also to affect the color of the precipitates. If a solution of bichromate of potash is exposed to sunshine, it acquires a property of precipitating several metals as chromates, differing many shades in color from the colors produced by a solution similarly prepared and kept in the dark. If the actinized solution (solution exposed to sunshine) be poured into a solution of nitrate of silver, the chromate of silver formed is of a much more beautiful color than that given by a solution which has not been exposed to the sun's rays. The same is true when the salts of mercury are used.

Solutions of sulphate of iron exposed to sunshine, yield a Prussian blue, with the ferrocyanide of potassium, of a far more beautiful color than that produced by a solution which has not been so exposed.

Among other curious actions that the sun's rays exert, is the one by which it prevents electro-metallic precipitation. Place in a test tube a strong solution of nitrate of silver; in another tube, closed at one end by a thin piece of bladder, place a solution of iodide of potassium; this is supported in the solution of nitrate of silver by being fixed in a cork, and a piece of platinum wire is carried from one solution into the other. An arrangement of this kind being kept in the dark, iodine is soon liberated in the inner tube, and a crystalline arrangement of metallic silver is formed around the platinum one, in the outer. Another being placed in the sunshine, iodine will be liberated, but no silver deposited.

Mr. Hunt has examined at length the action of the sun's rays upon some photographic preparations, (the salts of silver,) with many curious and interesting results, especially concerning the

chloride of silver. Five grains of pure chloride of silver were put into a long test tube full of distilled water, and placed in the sunshine to darken, the powder being frequently moved, so that every part might be acted upon by the sun's rays. It was found, even after an exposure of a few minutes, that the water contained chlorine; (it became opaque on the addition of nitrate of silver;) and this was gradually increased as the chloride darkened. The darkening was continued for several hours, after which the solution was filtered to free it from chloride of silver, and nitrate of silver added to the filtered liquid; the chloride of silver precipitated, when collected and dried, weighed 1.4 grains on one occasion, 1 grain on another, and 1.5 grains on a third trial. From several other experiments on the chloride of silver, the author is inclined to believe that the first action of the solar rays is to liberate one half of the chlorine, which, moisture being present, a very readily replaced by oxygen. By the continued action of the exciting cause, the other proportional of the combined gaseous element is in like manner set free and replaced, and we now have oxide of silver, which in a short time is decomposed under the so called actinic power of the solar rays, and hence we have eventually nearly pure metallic silver in a state of extremely fine division.—*Lon. and Ed. Phil. Mag.*, July, 1845, p. 25, and *October*, 1845, p. 216.—*In American Journal of Science and Arts*.

FREEZING OF WATER BY THE AIR PUMP, WITHOUT THE AID OF SULPHURIC ACID OR ANY OTHER DESICCATING AGENT.

By J. LAWRENCE SMITH.

In attempting to freeze water under the air-pump, without the aid of a desiccating agent, the cooling of the water to the point of congelation is prevented by the heat received from the containing vessel. I have lately found that by obviating this difficulty, water may be readily frozen by its own evaporation.

It was first shown by Count Rumford, that water does not wet a sooted surface, but forms in globules, like quicksilver. Three drops of water were placed in a sooted watch-glass; the spheroidal globule lay on the soot, exposing a large surface for evaporation, at the same time that the water was insulated from any source of heat. Arranged in this manner and placed under an air-pump, two or three minutes were sufficient to freeze the water. The glass was sooted over an oil lamp with great care; the experiment fails if the globule of water touches the glass even by a small point.

In place of the sooted watch-glass, make a shallow cavity in the end of a large cork, and over a lamp, burn it, sooting it at the same time. By putting three drops of water into the cavity thus prepared, and subjecting it to the action of the air-pump under a pint receiver, the water froze solid in a minute and a half; and in two and three-fourths minutes, 20 grains of water congealed, though at 73° Fahr. when introduced. Under a receiver of three quarts capacity, 20 grains of water froze in four minutes. I could not succeed in freezing the same amount in the sooted watch-glass.

By placing corks, prepared as above, over a saucer of sulphuric acid, the same results are obtained more rapidly. I put half a drachm of water, at 65° Fahr., in each cavity, and exhausted the receiver till the mercurial gauge reached 4.10ths of an inch, which was effected in one minute. In a minute and a half, the water on one cork began to freeze, and in five minutes they were all frozen. An ounce of water, in a large flat cavity, froze in 3½ minutes.

A flat-bottom porcelain capsule was prepared for an experiment on a large scale, by sooting it in the following manner. After coating it with soot over a lamp, and allowing it to cool a little, a small quantity of oil of turpentine was carefully poured upon the edge and passed over the entire surface; the vessel was then warmed to drive off the redundant turpentine. The surface was again coated with soot, and again with turpentine, and this process was repeated a third time; finally, another coating of soot was placed, when it was ready for use. Two ounces of water were placed in this capsule under a receiver, and the air-pump worked for one minute. After standing six minutes, the surface was frozen.

This experiment, as well as similar ones, was attended with violent ebullition on the part of the liquid, throwing the water against the sides of the receiver, which was owing to the rapid formation of vapor on the under surface of the liquid.—*American Journal of Science and Arts*.

THE

British American Journal.

MONTREAL, JULY 1, 1846.

HYGIENIC MEASURES FOR THE CITY.

Salus populi, suprema lex, is an ancient and a wise maxim. It lays at the foundation of all social happiness, and, by consequence, materially affects national prosperity. One of the most important concerns which could engage the serious attention of all civic corporations is the preservation of the health, as far as they have the means of doing it, of those whose interests have been entrusted to their keeping. A matter of this kind ought to be their chief care, their most anxious solicitude, and should be paramount to every other consideration. What signifies the embellishment of a city, so long as its environs, nay, even its very centre, abound with fertile sources of disease, which require but the warmth of a summer's sun for the production and the elimination of those miasmatic emanations which scatter death around? Beautiful, indeed, to the eye may such a city appear, but it is all external show; mark its mortality, and say whether the thousands spent in ornament would not have been much better employed in a complete and thorough system of drainage, ventilation, and cleanliness, the effects of which, though less visible, would be more lasting, and would ensure more certainly the happiness of the inhabitants, and their consequent prosperity, by the ablation of obvious causes of disease, or at least establishing a greater immunity from them.

In the way of drainage, ventilation, and general cleanliness, our civic authorities, since the incorporation of the city, have done much, and are therefore entitled to much praise; but much still requires to be done, and the sooner this is done the better. We are not alarmists. Far from it; but it would ill comport with our duty did we not express our apprehension, that the cholera, which appears again to have commenced its pestilential progress over the continent of Europe, may revisit us, and we know of no means more likely to moderate its desolating agency, than general hygienic measures. This city has already had a bitter experience of it in two former visitations, having been decimated in 1832; and it is well known, that in no districts of the city was the mortality from it greater than in the low, ill drained, ill ventilated parts, of which the St. Ann Suburbs then ranked pre-eminent. Should this scourge again appear among us, it requires not the spirit of prophecy to predict, that it will prevail chiefly in localities similarly circumstanced, and

these will be the St. Ann and St. Joseph Suburbs, the valleys along Buonaventure Street, behind the Champ de Mars, and behind St. Mary Street in the Quebec Suburbs. These different places are notoriously badly drained. They are even, in times of the existence of ordinary epidemics, from this very circumstance, the most unhealthy districts of the city, and furnish cases of sickness in greater abundance relatively than any other. How great, then, the necessity of immediately putting into execution such measures as will remove this obvious cause of disease, will, we think, abundantly appear from the few facts which we have given, and which will equally apply to every city in the Province. Unfounded reports of the existence of Asiatic cholera at Quebec, prevailed in this city a week or two ago. They have made us reflect seriously on the general preparation of the city for a third visitation, should such arrive. We are of those who consider a danger conquered that is boldly met; and in view of the importance, the extreme importance, of the end, we consider that no more fitting and appropriate subject could occupy the attention of our civic authorities, and we hope that an action, speedy and energetic, will be taken upon it.

Progress of the Asiatic Cholera.—We extract the following piece of intelligence, as a matter of considerable interest, from one of our journals, received about a fortnight ago:—

“We have already stated that the cholera had made its appearance in some of the provinces of Persia, carrying death into the principal towns. It has spread from Bokhara to Herat and Meshio, and has now taken the direction from the Caspian Sea to Teheran and Isphahan. Late accounts from Odessa state that it had crossed the Russian territory and appeared suddenly at Tiflis, taking a northerly direction between the Caspian and the Black Seas. On the other side, the cholera broke out unexpectedly at Orenbourg, in the mines of the Ural mountains; it crossed the Volga, and set its foot in Europe, at Casan, only 2,000 kilometers from St Petersburg. If the accounts we have received are exact, it has taken a most irregular direction. It has advanced from west to north, and does not seem to have followed the banks of the rivers, as in 1828 and 1832. The cholera which devastated France in 1831 and 1832, had been raging in Persia for seven years, 1823 to 1830. It first appeared in 1823 at Orenburgh, and shed death around that town for five years. It reappeared at Orenburgh in 1829, and one-tenth of the population fell a victim. It broke out at St Petersburg in July, 1831, and in France in the October of the same year.”

More lately we perceive that it was advancing with rapid strides towards St Petersburg, from whence we doubt not it will penetrate into western Europe.

CORRESPONDENCE.

Letter II.

COLLEGE OF PHYSICIANS AND SURGEONS OF UPPER CANADA.

To the Editors of the British American Journal.

GENTLEMEN,—In the letter which you did me the honor to publish in the last number of your journal, headed “The present condition of the Profession of Medicine compared with that of the Law;” I endeavoured to show, that the latter has been raised to the enviable rank it occupies at present in Upper Canada, by the unity of purpose displayed by its members in matters affecting the general good of their class; and this fact was made use of to support the opinion advanced by me in relation to the causes of the degraded state of our own profession. Dropping, for the present, the comparative part of the argument, I shall confine myself, on this occasion, to the consideration of the sins of omission and commission, justly chargeable against us individually and collectively; and if, in the pursuit of this inquiry, it shall afterwards appear that I have inflicted unmerited reproach upon any section of the profession, or any individual member of it, the injury shall be fully and promptly redressed. In my former communication I ventured to animadvert with some freedom upon the conduct of a small party of gentlemen here, with reference to the College Bill; since then, other facts connected with this subject have transpired, which would impart to their proceedings a character even more objectionable than the one already found for them, were it not for the charitable supposition that they might have acted upon erroneous information, though such a supposition, I am sorry to say, would rest upon nothing more than a bare possibility. These facts afford an example of a very common fault, if a fault, remarkably prevalent among the easy in circumstances—the successful portion of the profession; I mean the fault, or rather the *sin*, of selfishness—and in this particular instance, of a degree of selfishness so inordinate, so absorbing, and, at the same time, so blind, as to shut out from the mental vision of the actors, the light of reason altogether. It is necessary, as well for the general purpose of these letters as an act of common justice, that the “sayings and doings,” of the gentlemen referred to should receive some further notice at my hands, and the statement which follows is intended to accomplish both these objects.

It is already known to your readers that the bill to incorporate a College of Physicians and Surgeons in Upper Canada, was presented in the House of Assembly in compliance with the prayer of a petition addressed to the Legislature and the Government. This petition was framed and transmitted to Montreal in March last, and printed copies of the draft were received by several members of the profession in this city about the middle of May. One or two of the provisions of this bill have been seized on by the party of gentlemen above mentioned, as a ground of complaint against certain members of the Medico-Chirurgical Society, and as charges are involved in that complaint of a character at once disgraceful and untrue, it becomes the duty of the accused to repel the calumny as promptly and effectively as they can. I shall endeavour, therefore, (as one of the supposed offenders) to perform that duty on the pre-

sent occasion, and in the manner following.—It is necessary to premise that the business of the society has been conducted during the last six months by a few individuals, never more than eight or ten in number. These are the men against whom the following charges have been brought:—1st, Of having caused their petition, the purport of which was kept secret, to be embodied in the College Bill; 2nd, of a secret design to erect the society into the first body of fellows for the new college; 3rd, of having caused the bill to be *smuggled* into the House of Assembly.

The last mentioned charge deserves no commentary; it is an idle waste of time to bestow even a passing word upon it. In reply to the first charge, I have to say that I was one of the committee appointed to draft the petition alluded to—that no part of that petition was inserted in the bill, nor was it at all adapted to such a purpose: that the burden of its prayer was simply this,—that an act similar to the act passed in the third year of the present reign to incorporate a College of Physicians and Surgeons in Upper Canada, but so modified as to obviate the objections raised by the College of Surgeons in London might be passed in the then present session of the Provincial Parliament. It is true that this petition was got up in the belief that the Hon. Solicitor General was prepared to bring forward some such measure if requested to do so by the society, or the profession generally. But it is equally true that the details of such a bill were never canvassed at any meeting of the society, nor has the charter of the old College, or the draft of the new bill ever been seen on the table or among the papers of that body. Although the foregoing declaration embraces a reply to the second charge, it shall have a separate and more pointed contradiction; and I now deny most emphatically that such a proposal was ever broached by the society at any of their meetings during the last six months, nor do I believe that an arrangement of this kind was ever contemplated by any individual among them. Whether the adoption of the Society as a nucleus for the new College was a judicious measure or not is a question that ought to be decided by the Profession at large. (I must avail myself of this opportunity to direct the attention of the Profession to one serious fault in the petition, I allude to the concession made in favour of the London College of Surgeons. As that concession has not been granted in the present bill, so it is to be hoped it will not be allowed in any future one.) Thus much for the *sayings* of these malcontents, now for a glance at their *doings*, as counter petitioners.

Verily this, (the counter petition), is one of the most remarkable productions that ever emanated from the pen of a jealous, disappointed, and indignant gentleman. What an extraordinary development of the organ of self esteem does every page bespeak—what superb displeasure in every line! That the four and thirty Tyro’s who compose the Medico-Chirurgical Society, when no longer graced by his companionship, should dare to aspire to the dignity of fellowship of a Chartered College is an act of presumption quite beyond the reach of his conceptive faculties. Such is the spirit that breathes through the whole of this singular document. A few quotations from its voluminous pages will serve to amuse and, (I hope, also), to instruct your readers.

The petitioners begin by telling us that the bill is “crude

and undigested in principle and detail, its provisions would be *inoperative and impracticable*, and *subversive of the vested rights of the best informed and most experienced practitioners* in Upper Canada, and ruinous to the interests of the public.”

Remarks, “crude and undigested, inoperative and impracticable, and yet *subversive of the vested rights of the best informed, &c., &c., &c.*” Well done ye best informed, this is a *telling* paragraph, it must have cost you a world of labour, “mais c’est toujours le premier pas qui coute.”

2. “That your petitioners observe that, whereas, the preamble of said bill professes chiefly to provide, against an alleged defect in the laws now in force, for the prevention of persons practising without license, in its enactments it affects the fundamental principle upon which the constitution of the Medical Profession is at present based, and repeals an act establishing a *Medical Board* in Upper Canada which has been in operation during the last 28 years.”

Remarks.—Repeals an act establishing a Medical Board, ah, “this is the unkindest cut of all.” The chief petitioner and prime agitator is a member of the Medical Board, but not a member of the Medico-Chirurgical Society. It is possible that the patronage of the members of the Medical Board might cease with their existence as a board. True the establishment of the College would affect the fundamental principles upon which the constitution of the Medical Profession is at present based in Canada, but it would affect them most beneficially, it would give to that Profession a natural and solid foundation in exchange for an artificial and uncertain one.

3. “The bill did not emanate from the Medical Board and Profession at large, there is no overruling public necessity proved by the circumstance of its being brought forward by an individual member and not by the Government upon the petition of a few members of the Medico-Chirurgical Society, &c.”

Remarks.—Here we have the Profession at large introduced, and were it not that the whole tenor of their petition forbids the belief that they have been actuated by a sincere desire to promote the interests of the Profession at large, I should thank the petitioners for this show of liberality; but the animus of the author is too openly displayed, even in this paragraph, for that decoy to take.

4th. “That your petitioners are informed, and believe, that only eight medical practitioners, members of the Toronto Medico-Chirurgical Society, were present at the meeting of the Society when the resolution to petition for the *incorporation of the Society as a College* was adopted, some of whom, impressed with the propriety of well considering the details of a measure having the tendency of the bill referred to, have already in their capacity of members of the Medical Board petitioned your Hon. House, that no bill affecting the medical profession should be passed into a law without giving the board and profession &c. &c.”

Remarks.—Here we have a bold and positive assertion directly at variance with the truth! the petitioners did not ask “for the incorporation of the Society as a College,”—what they did ask for was an act to incorporate the Profession under the title of the College of

Physicians and Surgeons. The resolution, upon which the Society’s petition was based, was passed unanimously, but the stimulus of an *arrière pensée*, does sometimes make people act very foolishly. Still, I can almost venture to assert that there is not the least foundation for the statement contained in the latter part of this paragraph. Although the only fault that could be found in such a procedure, would be that of weakness, yet that fault, so excusable under ordinary circumstances, would be rightly regarded as a very grave one in this case, because the society were engaged in a laudable undertaking, in which the good of the whole profession was deeply concerned, and their efforts might have been seriously embarrassed by such conduct; but I repeat that I do not believe the statement; there is probably some miserable subterfuge at the bottom of it.

5th. “That ——— clauses give a power of supervision to the *minutest* portion of the profession.”

Remarks. The bill would have given power of supervision to the society composed, at present, of 34 or 35 members, among whom, there are six or seven members of the Medical Board, many graduates of “the best colleges in the empire,” besides members of royal colleges of surgeons, military men on full and half pay, and gentlemen of merit, educated wholly in this country.

6th. That in the opinion of your petitioners, these clauses affect the vested rights of all persons now licensed to practice physic, surgery, and midwifery, in U. C., not members of the Medico Chirurgical Society, or who *may not desire to become members of the college by the said bill proposed to be incorporated*; and the said Bill, if passed into an Act, would not only deprive gentlemen who have been educated in the best colleges in the empire, and who are entitled to all due privileges of the chartered colleges of which they are members of their right, to practice within the Province, unless they shall comply with the By-laws, &c. &c.

Remarks. I cannot see the force of these objections: the object of an act of incorporation would be to place the profession under a different form of government, from that which now prevails, but does it follow that their privileges would be abridged thereby? The effect would, undoubtedly, be the very reverse of that.

It is admitted on all hands that the Bill in question has some faults, but only in one instance does it exhibit a manifest inconsistency; it is not the opposition, but the character of the opposition that I complain of. If these disclosures shall serve to impress upon the minds of the impartial and independent portion of the profession the necessity of union and organization among themselves, by which means alone they can hope even to obtain a satisfactory legal recognition of their rights, then the object the writer has had in view, while thus engaged, will be accomplished.

You are at liberty to give up my name, if requested to do so.

I am, gentlemen,

Yours, &c.

Toronto, 26th June, 1846.

M. D. &c.

[To be Continued.]

BILL OF MORTALITY for the CITY of MONTREAL, for the month ending MAY 31, 1846.

DISEASES	Male.	Female.	Total.	AGE														
				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.....	Measles,	2	10	12	4	4	.	4
	Scarlatina,	1	.	1	1
	Hooping Cough,	4	4	1	3
	Fever,	16	17	33	15	14	2	1	1
	Small Pox,	1	.	1
DISEASES OF BRAIN AND NERVOUS SYSTEM.....	Apoplexy,	3	1	4	1	.	2	.	.	.
	Convulsions,	3	.	3	2	1
	Dentition,	3	3	2	1
	Hydrocephalus,	1	2	3	2	1
DISEASES OF THE THORACIC VISCERA.....	Consumption,	19	20	39	9	7	.	3	1	2	3	9	2	3	.	.	.	
	Croup,	4	1	5	2	1	2	
DISEASES OF ABDOMINAL VISCERA.....	Dropsy,	4	.	4	2	.	1	.	.	
	Childbirth,	1	1	
	Worms,	1	.	1	.	1	
	Jaundice,	1	1	
	Cholera,	1	.	1	.	.	1	1	
	Inflammation,	9	9	18	10	4	1	1	2	.	.	.	
OTHER DISEASES, AND DISEASES NOT SPECIALLY DESIGNATED.....	Abscess,	2	2	1	
	Unknown,	1	1	2	
	Still-born,	3	6	9	9	1	
	Debility,	4	4	8	
	Sudden Death,	3	3	6	1	2	1	2	1	
	Purpura,	1	.	1	
	Suicide,	1	.	1	
	Total,	78	85	163	58	37	5	9	1	6	11	16	7	9	3	.	.	

MONTHLY METEOROLOGICAL REGISTER AT MONTREAL FOR MAY, 1846.

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,	+53	+69	+54	+51.	29.74	29.74	29.70	29.73	N. W.	S. W.	N. E.	Fair	Fair
2,	" 47	" 56	" 48	" 51.5	29.69	29.71	29.74	29.71	N. E.	N.E. by N.	N. E.	Rain	Rain	Fair
3,	" 46	" 70	" 54	" 58.	29.90	29.90	29.94	29.91	N. E.	E.	E.	Fair	Fair	Fair
4,	" 52	" 72	" 56	" 57.	30.03	30.00	29.96	29.99	N. E.	N. W.	W.	Fair	Fair	Fair
5,	" 57	" 79	" 52	" 68.	29.90	29.92	29.95	29.92	W. by S.	W. by S.	W. by S.	Fair	Fair	Fair
6,	" 44	" 68	" 46	" 56.	30.07	30.06	30.05	30.06	E. N. E.	N. E.	N. E.	Fair	Fair	Fair
7,	" 45	" 71	" 53	" 58.	30.08	30.05	29.97	30.03	N. E.	N.E. by E	N.E. by E.	Fair	Fair	Fair
8,	" 51	" 75	" 59	" 67.5	30.00	29.98	29.93	29.97	E.	S.	S.	Fair	Fair	Fair
9,	" 58	" 78	" 60	" 64.5	29.91	29.83	29.67	29.80	S.	S. by E.	S. by E.	Fair	Rain	Rain
10,	" 49	" 50	" 42	" 49.5	29.50	29.45	29.46	29.47	E. N. E.	E. N. E.	N. E.	Rain	Rain	Rain
11,	" 35	" 45	" 40	" 40.	29.50	29.50	29.48	29.46	N. by W.	W. N. W.	N.W. by W.	Rain	Rain	Fair
12,	" 44	" 61	" 42	" 52.5	29.53	29.65	29.90	29.69	N.W. by N.	N.W. by N.	N. by W.	Fair	Fair	Fair
13,	" 44	" 56	" 54	" 49.5	29.85	29.82	29.76	29.81	W. by N.	S. W.	S. W.	Fair	Rain	Rain
14,	" 55	" 78	" 65	" 66.5	29.84	29.80	29.75	29.80	W. N. W.	S. W.	S. W.	Rain	Rain	Fair
15,	" 63	" 66	" 56	" 65.5	29.78	29.90	30.14	29.94	S. by W.	S.	S.	Rain	Rain	Fair
16,	" 61	" 68	" 56	" 64.5	30.20	30.16	30.13	30.16	N. W.	N. W.	N. W.	Cloudy	Fair	Fair
17,	" 62	" 80	" 64	" 71.	30.14	30.05	29.93	30.04	N. W.	N. W.	N. W.	Fair	Fair	Fair
18,	" 52	" 66	" 37	" 59.	29.84	29.92	30.05	29.94	N. W.	N. N. W.	N. by W.	Rain	Fair	Fair
19,	" 33	" 57	" 43	" 45.	30.07	29.96	29.85	29.96	N. by W.	N. W.	N. W.	Fair	Fair	Fair
20,	" 44	" 60	" 36	" 52.	29.72	29.77	29.88	29.79	W. by N.	W. by N.	W.	Fair	Showr.	Fair
21,	" 37	" 64	" 48	" 50.5	30.04	30.06	30.10	30.07	N. W.	N. W.	W.	Fair	Fair	Fair
22,	" 47	" 74	" 55	" 60.5	30.23	30.17	30.08	30.16	W.	W.	W. by S.	Fair	Fair	Fair
23,	" 53	" 65	" 54	" 61.5	30.04	30.01	29.95	30.00	W. by S.	W. by S.	S.W. by W.	Fair	Showr.	Fair
24,	" 56	" 80	" 63	" 68.	30.03	30.00	29.96	30.00	S. W.	S. W.	S. W.	Fair	Fair	Fair
25,	" 66	" 87	" 69	" 76.5	29.92	29.86	29.84	29.87	S. W.	S. W.	W. S. W.	Cloudy	Fair	Rain
26,	" 64	" 88	" 61	" 76.	29.88	29.76	29.79	29.81	W. by N.	W.	W.	Fair	Fair	Fair
27,	" 63	" 68	" 58	" 65.5	29.73	29.63	29.62	29.63	S. W.	S. by E.	S. by E.	Rain	Rain	Rn&th
28,	" 61	" 84	" 65	" 72.5	29.66	29.75	29.70	29.70	S. S. W.	S. S. W.	S.	Fair	Fair	Fair
29,	" 62	" 81	" 63	" 71.5	29.65	29.66	29.74	29.68	S. S.	S.	F.	Fair	Fair	Rain
30,	" 64	" 70	" 61	" 67.	29.76	29.77	29.79	29.77	S.E. by E.	S. E. by S.	S. E. by S.	Fair	Rain	Rain
31,	" 61	" 81	" 65	" 71.	29.88	29.86	29.89	29.88	S. S. E.	S. S. E.	S.	Fair	Fair	Fair

THERM. } Max. Temp., +88° on the 26th.
 } Min. " +33° " 19th.
 Mean of the Month, +59° 9.

BAROMETER, } Maximum, 30.23 Inches on the 22d.
 } Minimum, 29.45 " " 10th.
 Mean of Month, 29.80 Inches.

MONTHLY METEOROLOGICAL REGISTER AT H. M. MAGNETICAL OBSERVATORY, TORONTO, C. W.—May, 1846.
 Latitude 43°. 39' 4. N. Longitude 79°. 21' 5. W. Elevation above Lake Ontario, 108 Feet.

DAY.	Barometer at Temp. of 32°.			Temperature of the Air.			Tension of Vapour.			Humidity of the Air.			Wind.		Rain inch on surf.	WEATHER.
	7 A.M.	3 P.M.	10 P.M.	7 A.M.	3 P.M.	10 P.M.	7 A.M.	3 P.M.	10 P.M.	7 A.M.	3 P.M.	10 P.M.	7 A.M.	3 P.M.		
1,	29.314	29.286	29.326	54.3°	55.5°	54.8°	51.09	357	393	360	86	77	94	88	—	Overcast. B'ng st'ly fr. noon to midn't
2,	29.348	29.381	29.482	54.3	55.2	47.4	52.51	333	401	325	81	84	92	83	0.115	Overcast. B'ng st'ly fr. 3 am. Partially cl. in the aft
3,	29.614	—	—	62.7	60.8	—	—	330	379	—	60	73	—	68	—	Unclear. Hazy round horizon. Fine.
4,	29.720	29.659	29.611	59.632	65.1	50.1	51.92	294	337	254	73	56	71	80	—	M'ly cl'd. Mir. ann. Aurora from 11 to 11 pm
5,	29.653	29.589	29.530	59.630	61.8	52.1	55.77	204	423	340	78	72	89	80	—	clearly cl'd. Stud. sq. wd at 10 h 5 m am
6,	29.594	29.524	29.630	59.619	51.6	46.3	50.37	281	433	314	63	53	69	62	—	Cloudy to 10 a.m. Clear and fine p.m.
7,	29.624	29.583	29.589	59.593	55.4	49.4	52.97	185	330	302	46	76	87	74	—	Clear to 6 a.m. Rempler of day overcast
8,	29.620	29.552	29.534	59.551	55.3	50.0	57.61	369	343	393	86	66	88	82	—	Over. Halo rd. sun fr. 9 am to n. Str'ra
9,	29.429	29.252	29.189	59.308	55.2	56.0	51.99	413	438	472	97	97	97	87	0.630	Raining moderately on slightly all day—
10,	29.172	29.231	—	—	57.1	54.1	—	376	281	—	83	68	66	57	0.950	Drizzly clouded all day.
11,	29.433	29.407	29.393	59.407	56.6	46.4	41.70	111	157	166	51	53	66	70	—	Cl. to 6 a.m. Rem. of day m'ly clouded
12,	29.614	29.400	29.451	59.418	47.2	61.6	49.93	191	310	226	59	58	80	78	—	Mostly clear. Fine.
13,	29.353	29.264	29.371	59.353	46.3	67.8	59.76	275	403	378	88	61	67	78	0.175	Over. cl. haze. Str'ra from 10 h 50 m pm
14,	29.406	29.323	29.332	59.351	58.3	61.6	62.33	452	411	392	94	70	73	80	—	Stowery. M'ly cl'd. Cl's electrical
15,	29.383	29.680	28.744	59.668	51.1	59.8	52.49	350	464	294	95	92	85	86	1.900	Mostly overcast to 6 p.m. Rem. clear—
16,	29.825	29.794	29.805	59.753	49.4	60.8	49.3	315	407	315	91	78	91	84	—	Quiet clear. Very fine.
17,	29.813	29.735	29.735	59.735	62.3	66.3	47.29	319	178	142	88	80	55	—	not ap	Very sl. pd to 3 am. Cl'd to n. Rem. of
18,	29.602	29.742	29.818	59.751	54.1	53.4	41.2	38.7	45.13	226	61	62	86	68	—	Very sl. pd to 3 am. Cl'd to n. Rem. of
19,	29.853	29.687	29.556	59.678	42.5	55.1	45.13	171	262	201	63	32	56	73	—	Mostly clear. Hazy round hor. Fine.
20,	29.491	29.401	29.524	59.502	48.9	66.5	51.23	217	300	210	63	32	56	73	—	Mostly clear. Hazy round hor. Fine.
21,	29.762	29.759	29.778	59.778	44.8	51.8	42.09	158	226	194	51	59	85	85	—	Cloudy. Clear spaces. Rem from 9 p.m.
22,	29.844	29.757	29.655	59.723	47.6	56.5	52.09	221	214	223	69	47	59	68	0.350	Cloudy. Clear spaces. Rem from 9 p.m.
23,	29.553	29.561	29.644	59.586	54.4	68.4	59.45	376	484	405	90	72	85	86	—	Th. & p'ning. & p'n to 4 am. Rem. cl'd
24,	29.701	29.616	29.616	59.616	64.2	53.0	49.1	372	522	—	85	88	85	86	—	Th. & p'ning. & p'n to 4 am. Rem. cl'd
25,	29.645	29.645	29.578	59.601	65.2	70.4	66.35	552	661	560	96	92	96	93	0.140	Cloudy. Th. & p'ning. & p'n most of aft.
26,	29.554	29.439	29.344	59.419	66.0	73.8	67.22	580	660	497	93	81	92	89	—	Th. & p'ning. & p'n most of aft.
27,	29.982	29.984	29.306	59.267	72.2	76.6	68.06	625	503	441	81	57	78	76	—	Th. & p'ning. & p'n most of aft.
28,	29.391	29.225	29.275	59.265	63.9	69.0	63.54	482	536	412	84	77	88	83	—	Mostly clear. Clouds dispersed. Fair.
29,	29.325	29.271	29.240	59.268	62.9	70.0	61.77	450	514	414	80	72	77	79	—	Cloudy. Clear spaces. Th. 10 & 11 am.
30,	29.213	29.195	29.309	59.309	58.8	72.5	457	625	522	470	94	81	87	88	0.115	Th. & p'ning. and rain to 5 am. Cloudy.
31,	29.482	29.482	—	—	74.3	—	—	606	617	—	80	75	81	81	—	Th. & p'ning. and rain to 11 pm.
Mean	29.520	29.492	29.501	59.506	54.00	62.16	52.92	341	387	336	79	68	81	77	4.375	First th. st. of the season at Toronto.

Under the head of Tension of Vapour, is given the elastic force of the Aquæous Vapour in the Atmosphere at each Observation, in decimals of an inch of Mercury, or the Proportion of the Barometrical pressure due to its presence. Under the head of Humidity of the Air, is given the proportion of the Aquæous Vapour bears to the quantity the air is capable of sustaining at the existing temperature, saturation being represented by 100. The Instruments are Standard Instruments. The Rain Gauge is 21 feet above the soil. The Means entered are the Means by 24 hourly Observations, from 9 a. m., to 9 a. m. The Observations entered in the column for 7, 9 a.m., on Sundays, is actually taken at 9 a. m. The two Observations taken on Sundays are not included in any of the means.

Proportion of Wind from each Quarter—
 N.W. .. 113
 N.E. .. 83
 S.W. .. 71
 S.E. .. 71
 N.E. .. 86
 N.W. .. 86
 S.W. .. 271
 S.E. .. 271
 N.E. .. 271

Proportion of Calm,

Extreme Daily Range,

Mean Daily Range,

22° . 2 from 20th pm. to 21st a. m.

Range 46.6

29.862 at 8 a. m. on 19th.
 29.172 at 9 a. m. on 10th.
 79 - 7 on 27th. P. m.
 33 - 1 on 22th. a. m.
 17 - 97

1840 .. 94.2
 1841 .. 76.4
 1842 .. 81.2
 1843 .. 26.3
 1844 .. 49.5
 1845 .. 47.3
 1846 .. 79.5
 1847 .. 78.4
 1848 .. 52.2
 1849 .. 60.2
 1850 .. 71.8
 1851 .. 55.7
 1852 .. 79.7
 1853 .. 32.1

Range, No. Days, Inches.
 46.2° .. 9 .. 2.850
 45.2° .. 11 .. 1.975
 46.6 .. 6 .. 1.570
 49.7 .. 14 .. 6.670
 49.7 .. 8 .. 2.309
 46.6 .. 10 .. 4.375