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ORIGINAL COMMUNICATIONS.

ART. XIV.—*On the non-contagious nature of Scarlatina.* By LUCIUS O'BRIEN, M. D., M. R. C. S. E., Toronto.

[The following sketch was read at a meeting of the Toronto Medico-Chirurgical Society, in October, 1844, in consequence of my having, in conversation with some of the members, expressed my conviction of the non-contagious nature of Scarlet fever. At that time, I stood alone in Toronto in this opinion. Since then, I have seen nothing to induce any change in this opinion; and a careful examination of the several authors within my reach, has shewn me that their *belief* in this contagion is of a very dubious character, and not grounded on a similarly firm foundation as in regard to Small Pox, Measles, and Typhus; *e.g.*, Rayer says that it is contagious, "but in a less degree than measles."

In speaking of the fever which prevailed in Edinburgh in 1817 and seq: I have used the term "typhus" in accordance with the *then* prevalent opinion, although it was a very different form of fever, if not essentially different, from that which prevailed so fearfully in Ireland, and of which we had such melancholy illustrations amongst ourselves in 1847. The fever in Edinburgh alluded to has been lately happily described under the term "Relapsing fever," in consequence of one very distinctive character. It is to be hoped that the Editor of the *Journal* will notice this important subject.]

Toronto, August 18, 1852.

Perhaps there is no subject, which, apparently easy to investigate, has led to more extraordinary differences of opinion, than that of Contagion. The remarkable manner in which the *same series* of facts have been brought to support *opposite results*, should make us very cautious in drawing our references. The talented, clear-minded, and highly-educated Sir Gilbert Blane, affords a remarkable instance of how far even deep investigation and patient research, may be misled. He speaks of the "criminal folly" and wilful "self-deception" of those who believe that Yellow Fever is not contagious, whereas the united experience of *almost all* the numerous British military surgeons, of the highest class, under whose observation this disease has fallen within the last thirty years, as well as of every civil practitioner with whom I had the pleasure of being associated for several years in the West Indies, has proclaimed their conviction to the contrary.

I will not now enter into any distinction between contagious and infectious diseases; let it suffice that by either term I mean diseases which, being found in one human individual, may be communicated to another by the actual touch, or insertion of some peculiar fluid, or by exhalation from the lungs or surface.

In order to establish the fact of contagion or infection, it is necessary to observe, 1st, whether the exhalations or breath of the diseased person will (not invariably, but *commonly*) produce the same disease in others, whose exemption is not secured by previous circumstances. 2ndly, whether this *apparent* communication is or is not influenced by circumstances of location. 3rdly, whether the disease may not spread from some general cause (atmospheric constitution, as Sydenham calls it), independent of contagion or location.

Thus, in illustration, First, let us take Typhus Fever, Measles, or Small Pox. Exposure to the exhalations of an individual labouring under any of these, will produce the same disease in all individuals, except when these exhalations are much diluted, or when the constitution is originally or temporarily insusceptible, or rendered so by a former attack, as in the two last named cases. This reproduction of disease is irrespective of location; and such diseases, together with those in which inoculation is necessary to such reproduction, as Vaccina, Syphilis, Frambœsia, &c., are what we correctly term contagious or infectious.

2dly. In places very liable to the production of malarious fever and its various grades, including Remittent and Yellow Fever, we find whole families, and bodies of men (*e. g.*, military and naval), attacked sometimes successively, sometimes simultaneously—witness the intermittents in this country and Eng-

land, the Walcheren fever, the jungle or hill fever of the East Indies, and the yellow fever of the West Indies and this Continent. In none of these cases can we prove the existence of contagion, and one of the strongest proofs of their non-contagiousness is, that individuals labouring under any of them, on being removed to a *healthy* location, have NEVER been known to communicate the disease. One conclusion to be deduced from this class of diseases is, that the spread of a disease among the individuals of a family, or of any mass of persons, is *not to be taken*, per se, as a *conclusive proof of contagion.*

As examples of the third class of diseases, I shall mention Cholera and Influenza. I am aware that they have been considered contagious, but I believe that extensive and accurate investigation have sufficiently proved that they are not.

Now, in endeavouring to ascertain how far Scarlatina is contagious, let us compare it with the foregoing examples. It would be very easy to multiply instances of its apparent contagion; and one gentleman, for whose judgment and talent I entertain the highest respect, has stated that he has known it conveyed a long distance (I forget how many miles) by means of a letter sent through the Post Office'—we may exclaim with Dominie Sampson—Pro-di-gious! But in such a case the only fact arrived at is, that an attack of Scarlatina occurred in a certain family about the time of the arrival of a letter from a house where this disease existed. In Edinburgh, in the years 1817, 1818, 1819, 1820, typhus (I use the term generally) occurred to a fearful extent. During that time the Edinburgh Infirmary contained somewhere about two hundred patients. The fever patients were placed in wards appropriated to them, except in the clinical wards, where, to a certain extent, they were mixed with other diseases. Almost all the individuals whose duties were connected with the fever wards, were attacked with fever—the physicians' clerks, matron, apothecary and nurses were, without exception I believe, attacked; several of the clerks and nurses had fever a second time, and some few even a third time. (In some a fatal termination took place.) To the best of my recollection no case of fever occurred among the nurses belonging to the other wards, or among the other servants of the establishment, and certainly in two years no such circumstance occurred. In Queensberry House, which was open part of that time as a fever hospital, and contained from thirty to forty or more patients, several of the nurses and some of the clerks were attacked with fever. I have the best authority for stating that in the Toronto Hospital, patients who were admitted with other diseases, and who had been placed alongside typhus fever patients, become affected with that disease I have no doubt but that many of our members have observed similar facts.

During the period above named, Scarlatina was also prevalent in Edinburgh, and numerous cases, in all its various forms, were admitted into the fever and clinical wards. Yet *not a single instance* has ever come to my knowledge of this disease being supposed to have been propagated in an hospital. Many instances also occur where Scarlatina has attacked one or two members of a family, and where no other individual has been affected, although the intercourse has been unrestrained. In my own family, three children had it with some days interval between them. The servant and other individuals who attended on them, and were constantly about them, did not evince a single symptom of the disease, whilst another, who never was exposed to the *contagion*, had a sharp attack. I had lately two fatal cases of Scarlatina, children of one family, one of which was assiduously visited also by our excellent Secretary;* the first fatal by sudden congestion of the brain, with retrocession of the eruption, the other from acute hydrocephalic symptoms. Their parents, Mr. and Mrs. B——, who had also a young baby, with these two, took up their abode at a house (of a Mr. R——) in this City, just as the eldest began to complain. Mr. R.'s family consisted, besides, of four adults, if not five, and four children of different ages. During the illness of Mr. B.'s two children, the other children were certainly kept down stairs, but the other individuals were indefatigable about the two little sufferers by day and night; and up to this very day of writing this sketch, no one else of either family has had any trace of the disease.† Mr. R. had a slight attack of *cynanche tonsillaris*, with aching of his limbs, easily accounted for by his being exposed for a considerable time one night to damp and cold, without sufficient clothing, while trying to procure leeches. True it is that after the funeral, I recommended fumigation, cleansing and ventilation to the utmost; for while so many men of observation and talent maintain the contagiousness of this disease, I should not feel justified in not directing what is so desirable under any circumstances.

The rarity of a second attack of Scarlatina is no proof of either opinion, as second attacks of Measles and Small Pox occur occasionally, and are by no means unfrequent in typhus.

Scarlatina is known to have spread very extensively through this Province during the last three years, and yet this fact is far more explicable on the idea of its being an epidemic, arising from some general cause, than from contagion. The experience of Ireland and other places shews that typhus will invariably

* The late deeply lamented Dr. Grasett.

† One little boy was affected twenty days afterwards.

run through every family where poverty, confinement, and dirt are the inmates, and that on the contrary, in the houses of the affluent, where cleanliness, separation and ventilation are thoroughly observed, typhus is very rarely found to spread, and if it should, it is almost invariably confined to those who have been in direct and constant attendance on them. The prevalence of Scarlet Fever has been wholly at variance with this rule. It has prevailed equally among the affluent and the poor, in the well-ventilated and well-cleansed house as in the confined, and I question whether it has not been as fatal among the former as the latter, except where proper medical means have been wanting, or improper treatment adopted at first.

In the summer of 1842, Mr. B., a young man, an inmate of my house, was attacked with this disease while on a visit to a friend a few miles out of town. He was brought home in about 24 hours, just before the eruption appeared, and had a tolerably sharp attack. Although my three eldest boys alone of all the family, had ever had Scarlatina, no restriction was put on the attendance of the others, and no one either of my family, or of that in which he was attacked, had any symptoms of it. Mrs. O'B. attended him assiduously by day, and I by night, and his washing was all done at home. Last spring Mr. E., another young gentleman also residing with me, was similarly attacked. As in the former case no restrictions were employed, and again no one else was attacked, although five adults, and one infant about a year and a half old, in the house, never had had this disease.

I could adduce many other instances of a similar nature, but I fear the subject must have become tedious by this time to my hearers, and shall therefore hasten to a close.

Dewees of Philadelphia, no mean authority, says, "the evidence" (of its being a contagious disease) "to say the least, is equivocal. The facts connected with" its "spreading seem to be perfectly explicable, on the ground of its being epidemic and not contagious." "I have never seen so far, any decided proof that it has communicated itself in any one instance; on the contrary, I am strongly disposed to doubt its contagious quality."—Pract. of Med., page 184.

In conclusion, when a student and for some years afterwards, I was strongly impressed with the contagious nature of Scarlatina. I had been so taught, and in spite of the evidence to the contrary, I so believed. More experience has gradually changed that belief, and notwithstanding the difficulty of the subject, and the weight of authority against me, I now unhesitatingly declare that I believe SCARLATINA IS NOT A CONTAGIOUS DISEASE.

Toronto, October 1st, 1844.

ART. XV.—*Apparatus for making Extension in Fractures of the Lower Extremity of the Radius.* By PROF. BEAUMONT, F. R.C.S., Eng., &c. &c. &c.

Having introduced into the practice of the Toronto Hospital an apparatus for making extension in fractures of the lower extremity of radius, and finding its success commensurate with my expectations of its usefulness, I was induced to publish an account of it. I have now the pleasure of adding Dr. Warren's opinion of its value :—

Dec. 8.—Fracture of the Lower End of the Radius, with other Fractures—Description of Professor Beaumont's Apparatus, &c.—Dr. J. M. WARREN presented the specimen, which was quite interesting from the fact of the opportunities being rare for observing this fracture in a recent state.

The patient was a man thirty years old, and was brought into the Hospital, having fallen a distance of forty feet through a scuttle to the floor. The following is the Hospital Report :—

"There is now, at six P. M., fracture of the right radius, apparently just above the joint. There is great deformity, simulating dislocation of the wrist backwards. Crepitus distinct.

"The right leg is shortened, by measurement, one and a quarter inches. It is everted, with edge of foot lying flat upon the table. There is distinct crepitus at or near the cervix femorix. When pressing the two iliac crests, they yield sensibly, and give a *feeling* of indistinct crepitus." The patient died at ten P. M.

The pathological appearances of the parts exhibited were as follows: The right radius was fractured transversely half an inch above the joint, with a comminuted fracture extending into the joint. The internal lateral ligament was torn away from its attachments to the ulma, carrying a bit of the bone with it.

The right femur was the seat of a comminuted fracture through the trochanter, and a longitudinal fracture of the shaft of the bone extended from its cervix downwards for four inches. Neither of these fractures communicated with the capsular ligament.

The right sacro-iliac synchondrosis was torn asunder, and the bones forming it, fractured. The ramus of the ischium and pubis was fractured. The lower half of the sacrum and os coccygis were comminuted.

In connection with the specimen of fracture of the lower extremity of the radius, Dr. W. made some remarks on the interesting nature of this accident to surgeons, from the liability to deformity so likely to occur in spite of the best-directed treatment. The various apparatus invented by so many distinguished surgeons, with the object of preventing this deformity, show

the importance attached to it. From simply regarding the external appearances presented by this fracture, it was formerly supposed that the bones yielded in an oblique direction; but observation of pathological specimens has shown that it is, on the contrary, almost always transverse, the peculiar deformity arising not so much from the overlapping of the fragments, as from the direction of the displacement by muscular action. Dr. Smith, of Dublin, in twenty specimens which he examined, found the fracture to have a transverse direction in eighteen. In the present specimen it is transverse.

Dr. W. said he would avail himself of this opportunity to show a very efficient apparatus for making extension in fractures of the lower extremity of the radius, contrived by Professor Beaumont, of Toronto, to whom the profession is indebted for the invention of many ingenious surgical instruments, some of which have been for a long time in use at our Hospital.

This apparatus consists of an angular splint, made of gutta-percha, adapted to the bend of the elbow. To this is attached a bar of iron, which extends beyond the hand, and is then bent to a right angle. This latter portion has attached to it two axles, with ratchet wheels, for the purpose of making extension by means of cords attached to a leather cap laced to the wrist just above the joint. In addition, there are two small splints adapted to the anterior and posterior part of the forearm.

The following extract from the letter of Dr. Beaumont to Dr. W., describes the method of application :—

" The patient's arm and forearm, having been bent at a right angle, should be placed in the angular splint, and there fixed by a bandage. A piece of gutta-percha, of the shape of the leather cap, may then be softened and wrapped round the carpus and metacarpus, in order to protect the skin from any painful pressure; and when the gutta-percha has become hard, the cap is to be laced tightly over it, and in such a manner that one loop of the cap shall be on the radial border of the metacarpus, and the other loop on the ulnar border. The strings from these loops may, by turning the angles, be more or less tightened, so as to keep up permanent extension, which will be as nearly as possible in the axis of the broken radius, and the distal fragment will thereby be drawn very nearly into its normal relative position with the proximal fragment. The extension should be so gradually made as to remove the displacement with little or no pain to the patient: but should the extension become painful, it may be lessened by throwing the catch out of the teeth of the ratchet-wheel, and allowing the angle to revolve backwards. The anterior and posterior splints need not be applied for the first week, especially if the distal end of the forearm should be much swollen and inflamed, and as this part may be left exposed in it

whole circumference, we can very effectually apply cold evaporating lotions, and can see and feel that the fragments are in their normal relative positions before applying the anterior and posterior splints. These splints are made to reach very nearly to the metacarpus, and, as you see, are so made as to press most against the interossous space (so as to preserve its width), and also to prevent pressure in a direction from radius to ulna."

Dr. Beaumont also thinks this apparatus might be found useful in fractures at other parts of the radius, also in fractures of the coronoid process of the ulna, with dislocation backwards, and in fractures of the humerus just above the condyles, when the latter and the bones of the forearm are drawn backwards.

Art. XVI.—*On the White Globules in Disease.* By James Bovell, M. D., Toronto.

From the numerous observations that have been lately made by different eminent Pathologists, we seem to be in the possession of those facts which render the nature of some of the ultimate changes of the White globules much more intelligible than formerly.

Are we yet in possession of a sufficient number of facts to lead to the conclusion that the Pus corpuscle is nothing more than a fatty degeneration of the White? Impressed myself strongly in favour of such a change, I nevertheless feel much diffidence in enunciating it, being well aware that a mere Provincialist has not the same opportunities for investigation and experiment enjoyed by Physician or Surgeon to a large hospital.

Before, however, proceeding to collate the proofs which seem to support the doctrine advanced above, I may be allowed in passing to mention the result of an examination which was made this spring on the web of a frog's foot.

In Mr. Hassall's highly valuable work the following paragraph had attracted my notice very forcibly, and I felt naturally anxious to observe, if possible, the phenomenon described.

Mr. Hassall observes, "it is not alone the aggregation of the colourless corpuscles that may be seen *in* the minute vessels; their escape *from* the vessels may likewise be detained by a prolonged examination of them."

If, after the continuance of this congested condition of the vessels for twenty four or thirty six hours, they are again examined, it will be obvious that certain of the corpuscles have become entangled in *the fibres* which form the walls of the vessels, and that certain others have altogether passed the boundaries of the vessels, and now lie external to them. If such a phenomenon as this could be proved true, of course there would be a

great step gained; but, aware of Prof. Williams' instructive remarks on the condition of the vessels in determination of blood, I was induced to watch the more narrowly. The following are Dr. Williams' observations:—

In the frog's web gently irritated by an aromatic water we see the arteries become enlarged, supplying a larger and more impulsive flow of blood to the capillaries and veins, which all become enlarged also; and the whole vascular plexus, including vessels which before scarcely admitted red particles, then become the channels of a much increased current. This is determination of blood.

As these phenomena have not been distinctly described by observers apart from the further effects resulting from over irritation, which leads to obstruction and inflammation, I will state shortly some results of many observations on the circulation of the frog's web, under the influence of moderate stimuli applied to it. These observations were made in the summer of 1841, and some of them are mentioned in my Gulstonian Lectures, published in the *Medical Gazette* of July, 1841.

The arteries may be distinguished from the veins in the frog's web, not only by the direction of their current and its greater rapidity and transparency, but also by a series of lines along their course, marking the size to which they have been distended at some previous time. These lines or channelings are most distinct, and are more remote from the artery at its angles or bifurcations. They are to be seen at some points along the veins, but much less distinctly.—Now these lines are in themselves proofs of the varying distention of the arteries, and they also furnish the means of measuring this varying distention.

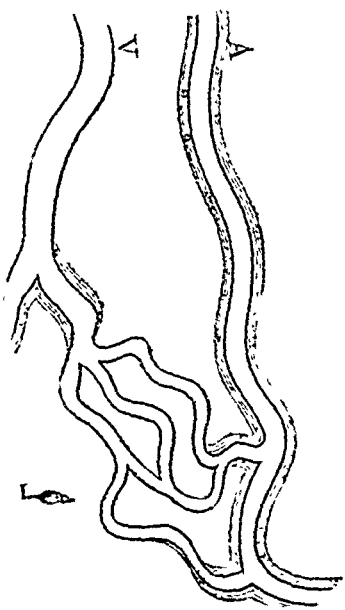
When a weak infusion of capsicum is applied by a camel's hair pencil to the web, there is a momentary retardation of the current in the veins, and the artery distinctly shrinks in size. But in a few seconds the reverse takes place; the artery swells to beyond its former size, and reaches the outmost line of its channel; the flow of blood through it is too rapid to be distinguished, and all the capillaries present a scene of busy motion: in some the particles passing in numbers and speed greater than the eye can appreciate; in others, before invisible, single files force their way in more deliberate, but continuous motion: whilst in the veins the movement is again more rapid. This motion soon begins to flag, and becomes remittent or oscillatory in some capillaries; and it is seen that the arteries have already begun to shrink in size, and the channeled lines which they had reached re-appear. Sometimes, in shrinking, the artery assumes for a time a more tortuous shape than before, so that its walls cease to be parallel with the lines, which seem to shew that it contracts in diameter before its length is proportionally reduced. The contraction of the artery, and consequent reduction of the quantity and movement of the blood in the vascular plexus, was promoted by repeated applications of cold water, which in some instances stopped the motion of blood altogether, by contracting the artery to so small a size, that no blood particles entered it. A solution of acetate of lead also produced this effect.

The determination of blood thus excited produces an increased redness quite visible to the naked eye, but it is less intense and of lighter hue than the redness of inflammation or congestion.

Thus keeping in mind the existence of "channeled lines," as described by Dr. W. and the assertion of Mr. Hassall, I was engaged in April in some microscopic examinations of the circulation in the foot of a frog, and which had been two days before under observation. After a time in one of the vessels in which the blood was flowing along by single file of globules, I noticed three white corpuscles lying, as I thought, outside the line

of the vessel, and felt, of course, gratified at the sight. Whilst intently watching the three bodies, the circulation became gradually increased in the vessels, and, to my surprise, I saw the white out-laying corpuscles re-enter the circulation, and vanish from the field of view, and on subsequent occasions I have witnessed the same phenomenon.

Finding that the white corpuscles had only passed within the channeled lines, and had again entered the onward current, the next question which arose was as to nature of the lines themselves—How are they formed? Dr. Williams considers them "as proofs of the varying distention of the arteries, and that they also furnish the means of measuring this varying distention that the vessel contracts." It would appear from the observations alluded to above, that the channelings were apparent when the vessel was in a state of collapse from exhausted irritability, and that only the central portions of the vessel admitted the passage of the current, so that a vessel, under such a state, if divided transversely, would present the appearance represented by the following diagram, at L. I have given the diagram from Dr. W.'s work, placing the white corpuscle in the position in which they were seen :—



The opinion which seems to be generally current, attributing to the capillaries the power of suddenly altering their calibre, has always appeared to me to be far too mechanical a method of accounting for the increased admission of blood-corpuscles, and does not seem to be borne out by observation. It may frequently be noticed that the vessels enlarge sometimes all at once; and again, that some remain enlarged while others appear to contract. We further notice, that while the part is not under irritation, that comparatively fewer vessels are seen; but so soon as an irritant is applied, "a busy scene of motion" commences, and the blood-corpuscles are now seen to enter channels which before were not noticed; and if the irritation be sufficiently prolonged, a stagnation of the blood is the result, and the whole field is occupied by an indefinite sheet of red colouring.

Whatever the nature of the forces which determine the flow through the capillaries, it appears that these vessels, at all events, do not enact any very important *physical* power over the fluid passing through them. The rapidity of the current through one or two branches, while in the neighbouring ones the flow is much more slow, would certainly lead to the supposition that the movements were regulated or under the guidance of dynamical forces. The readiness with which these important vessels open to the course of the moving corpuscles would seem to point out their almost passive state, and that their distention or collapse would be occasioned by the mere passage of greater or less quantities of corpuscles through them. The readiness with which the corpules go into vessels and as quickly retrograde again has induced some to look on the capellaries as channels through the tissue. It seems, too, that we may in this way account for the appearance and disappearance of the lines. I have, hour after hour, watched for the transit of the white corpuscle, but in vain.

That the white corpuscles are most intimately concerned in nutrition, is a doctrine supported by the best observers, and their conversion into higher developments would seem to be influenced by determinate laws. It does not seem to be in accordance with general observation to admit the actual escape of the white corpuscle from the vessel; on the contrary, the views held by Messrs. Carpenter and Addison are much more probable, and are not so generally brought forward as their intrinsic merits deserve, and, as they have an important hearing on the question before us, they are given in full—

"That the white corpuscles are concerned in the process of nutrition, there is more evidence to show than there is in favour of their connexion with that of secretion. The question to be solved, however, is, in what way do these corpuscles administer to nutrition? do they contribute to nutrition and growth, by their direct apposition to and incorporation with the different tissues of organs? This is the opinion of Mr. Addison, who says of them, that they are the "foundations of the tissues and the special secreting cells, the link between the blood and the more solid structures, the unity from which the pluralities arise."

In regard to the purpose of the colourless corpuscles in the animal economy, a view has been brought forward by the author, which increased consideration has only served to strengthen; and which he advances here with some degree of confidence that it will be found, on attentive examination, warranted by a large number of physiological analogies, though not capable of being directly proved. That it may be rightly understood, a general sketch of certain known operations of cells in Plants and Animals will be first given. It is not difficult, on taking a comprehensive survey of the assimilating processes, to find a number of examples in which *cells* are developed in a temporary manner;—growing, arriving at maturity, and then disappearing, apparently without having performed any particular function. In the albumen of the seed, for instance, this often takes place to a remarkable extent. In the Yolk of the Egg there is a similar transitory development of cells, of which several generations succeed each other, without any permanent structure being the result. In the Germinal vesicle, again (according to Dr. Barry), several annuli of cells are seen to

occupy its cavity, when it is prepared for fecundation; and the oldest and largest of these contain another generation: yet all these disappear by liquefaction, as soon as the two *permanent* cells begin to be developed in the centre. Further, in the subsequent development of all the cells which are descended from these, and form the "mulberry mass," the same process is repeated; a great number of temporary cells being produced, only to liquify again as soon as the two temporary central cells make their appearance. It can scarcely be imagined by the well-judging physiologist, that all this *cell-life* comes into existence without some decided purpose: and if we can assign to it an object, the fulfilment of which is consistent with the facts supplied by analogy elsewhere, this may be reasonably considered as having a fair claim to be received as a physiological induction. In all these instances, and in many more which might be quoted, the crude alimentary materials are being prepared to undergo conversion into permanent and regularly organized structures.

We have thus a class of facts, which indicates that the conversion of the Chemical compound into the Organizable principle—the *aplastic* into the *plastic* material—is effected in the particular situations where it is most wanted, by the vital agency of transitory cell-life: that is, by the production of cells which are not themselves destined to form an integral part of any permanent structure, but which, after attaining a certain maturity, reproduce themselves and disappear—successive generations thus following one another until the object is accomplished, after which they altogether vanish. We shall now consider another class of facts, which seems to indicate that a change of this kind is continually effected in the nutritious fluids of animals, during their circulation through the body: by Cells, which are either carried about with them, or which are developed for the purpose in particular situations, as in plants. The former is the more common occurrence: since the conditions of animal life, usually involving a general movement of the body, require also a *general* reparation of its parts, and an adaptation of the circulating fluid therefore to the wants of the whole fabric.

It has been already shewn, that cells, which seem identical with the white corpuscles of the Blood, are to be met with in the Chyle and Lymph,—fluids in which the elaboration of plastic fibrin is going on; and that such an elaboration must be continually taking place in the blood itself, to supply the plastic material which is being as continually drawn off by the nutritive processes. Hence there would seem reason for attributing this important function to these floating cells: the number of which present in the fluids seems to bear a very close relation with the energy of this elaborating process.

Believing with Mr. Addison that these bodies, whether appearing in the blood as floating cells, ready to pass into higher developments, or as exudation corpuscles without the vessels, are "the foundation of the tissues and the special secreting cells, the link between the blood and the more solid structures, the unity from which the pluralities arise, let us see how far we are able to trace the white corpuscles to their destination and transformation, both under healthy and diseased conditions. Messrs. Todd and Bowman would seem to entertain almost similar views with those of Mr. Addison, and have perhaps more fully explained the circumstances under which the various forms of tissue are developed, and the share which various parts of the cell performs in the change.

The changes which the cell undergo in the formation of the tissues, may be described under two heads; first, those affecting the cell-membrane; and, secondly, those in which the nucleus is concerned. In those tissues, whose

ultimate elements are fibrous, that is, consisting of real or apparent fibres, as areolar and fibrous tissues, the cell-membranes become elongated, and so folded or divided as to give the appearance of a subdivision into minute threads or fibres. In the tissues, which are composed of tubes of homogeneous membrane containing a peculiar substance within them, as muscle and nerve, the cells are joined end to end, and, the partitions of each being removed, their cavities communicate, so that they together form a tube, or sheath, in which the deposit of the pupular muscular or nervous substance takes place. The smallest or capillary blood-vessels also are formed by the coalescence of the walls of the cells, not at one or two, but at several points, owing to their elongation, here and there, into pointed processes, which unite and form the ramifications of the vessels.

From the preceding brief and necessarily imperfect sketch, it seems evident that, in the various metamorphoses of the fœtal into the perfect tissues, both the elements of the cells take a part. In no instance does there appear to be an actual conversion of either cell-wall or nucleus into the ultimate elements of the tissues. The cell-walls may be changed into a part, *accessory* to the complete texture, as the sar-collemma or sheath of the muscular fibre; but the further organizing process takes place on its outer or inner surface. And the nuclei, likewise, may be changed into parts, which contribute to the nutrition of the tissue; but not into its essential elements. These, it must be remarked, are always the product of an ulterior organizing process, connected chiefly with the cell-wall.

There seems reason to believe, that during the organizing process which occurs simultaneously with the changes of the cell, a chemical alteration takes place; for the cells of cartilage sometimes contain fat, and the cartilage of bone prior to ossification contains chondrine, but, after the ossific process, gelatine is found: and it is also stated, that the element which may be obtained from the young cells of areolar tissue is pyine; whereas gelatine is yielded by the fully-formed tissue.

The formation of cells does not cease with the infancy of the organism. These minute organic elements are most important agents in various functions of the body at every period of its existence. By them the secretions are separated; and it is not improbable that they contribute largely to those changes in which nutrition immediately consists. They are found floating in immense numbers in the blood, as well as in the chyle and lymph; *and even in diseased secretions, as pus,* they exist in great quantity. In the inflammatory process, they are formed in great abundance; and in the malignant growths, which infest the body, so as to manifest themselves at different parts of it, such as the various forms of cancer, the same organic forms are to be found.

In short, Schleiden and Schwann have proved that the nucleated cell is the agent of most of the organic processes, whether in the plant or animal, from the separation of the embryo from its parent, to the development, growth, and nutrition of the adult individual.

Mr. Paget in his Lectures on the Process of Repair and Reproduction has perhaps more fully stated the nature of those forces by which the progressive developement is effected.

The accepted doctrine in physiology seems to be, that each structure in the body has the power of taking from the blood, by a kind of elective affinity, certain appropriate materials, and of so influencing them that they assimilate themselves to it; i.e. they adopt or receive its form and properties, and incorporate themselves with it; or else it is held that each cell or structural element of a part, whilst developing itself into some higher form, leaves behind or produces germs, cytoblasts, or off-shoots, which shall pass through the same development as itself, and in due time succeed to its place and office.

But since we see that the continual mutation of particles for the growth and maintenance of the living body by nutrition manifests in all essential

things, the peculiar features that characterized the first formation of the same body from the germ, we seem justified in holding that it is one and the same power which, being maintained continuously from the germ to the latest period of normal life, determines all organic formation. Whatever be the properties of the germ from which this formative power emanates, they must, in due measure, be communicated to all the materials that the germ appropriates, and, successively, to all that enter into the construction of the developing body, so that in all its living parts there is a measure of the same power as was most vividly concentrated in the germ. Under this power the organic assimilative force, in some instances, and the production of successive tissue-germs in others, appear to work as subordinate agents: but in many instances, as in the formation of blood, the power seems to act more directly upon amorphous organic matter, which is the perfect body, as in the germ, it informs according to the law of specific character.

In the greater part of congenital malformations we find arrest of development, but no hindrance of growth: as a heart, in which a septum fails to be developed, yet grows to its full bulk. So, *if tadpoles be excluded from due light and, heat, their development will be much retarded, but their growth will be less checked.* So, too, in the miscalled cultivation and improvement of flowers, growth is increased, but development is hindered; and an excess of coloured leaves is formed, instead of the due number of male and female organs. Or again, *in an old ulcer or a sinus, cells may be continually reproduced, maintaining or even increasing the granulations, yet they will not develope themselves into cellular tissue and cuticle for the healing of the part;* and so, even when repair and reproduction have gone far towards their ultimate achievement, that which takes a longer time, and oftener fails, is the improvement, the perfecting the new material, by its final development. This is observed in all cases of reproduced limbs, and even in ordinary scars.

These facts (and there are many others like them) seem to justify the expression that, not only more favourable conditions, but also a larger amount of organizing force is expended in development than in growth. If we may thus interpret the facts, they will be collateral evidence for the belief that, in different species of animals, the reparative power will bear an inverse ratio to the amount of development already passed through; so that, for each species in its perfect state, the reparative power might be measured by the degree of likeness between the embryonic and the perfect form, structure, and composition. The greater the sum of dissimilarities in all these respects between the embryo and the perfect animal, the less seems to be the reparative power in the latter.

In the British and Foreign Medical Review, we find an interesting commentary on Dr. Carpenter's views on the Mutual Relations of Vital and Physical Forces, which certainly seems to promise a much more philosophical exposition of the various phenomena which are exhibited in the animal body than any which has hitherto been attempted.

"Looking at the phenomena of Life from the same point of view as that from which we are now taught to regard Physical phenomena,—namely, as the results or manifestations of certain *forces* acting through those forms of matter termed Organized, which forces may be provisionally termed *Vital*, it should be our first object to ascertain whether these phenomena (such of them, at least, as are neither Chemical nor Physical) can all be referred to the agency of one Force, operating through a variety of instruments, or whether it is necessary to have recourse to the idea of a number of distinct forces. 'Our clearest idea of the agencies essentially concerned in the production of vital phenomena, is derived from the study of the development of any single organism;" and the simplest vegetable cell is selected by Dr. Carpenter as presenting

this series of phenomena in their least complicated form. Now in the growth of such a cell (belonging to any one of those simple Cryptogamic tribes, in which each cell may be regarded as an independent organism,) from its germ, we notice in the first place, that it exerts a power closely allied to, if not identical with, that of ordinary *chemical transformation*; for it decomposes carbonic acid, and unites its carbon with the elements of water; at the same time decomposing ammonia, and uniting its azote with the oxygen, hydrogen, and carbon, derived from the sources just named; thus forming *organic compounds*, such as no operation of ordinary chemistry has yet been able to imitate. This process, as is well known, can only be effected under the stimulus (to use the common phraseology) of Light; but it would rather appear from the preceding considerations, that Light is the *force*, which, acting through the Vegetable cell as its instrument or "material substratum," *produces* those new Chemical attractions, which determine the formation of these new compounds. Dr. Carpenter then goes on to show, that *in the application of the nutritive materials thus generated to the development of the cell, we must distinguish a force of assimilation or vital transformation*, by which these materials are rendered plastic or organizable, and a force of *organization* or complete vitalization, by which they are incorporated with the solid texture, and become possessed of its properties.—Now, although we may provisionally designate these as distinct forces, on account of the diversity of their manifestations, it is impossible not to see that they are mutually dependent, and that they form the successive elements of a continuous series of phenomena belonging to the same category, that of *cell-life*; and further, we observe that they operate under the same conditions, namely, the presence of a cell-germ and of the materials of its growth, and the action of Light and Heat. Again, in the *multiplication* of the original cell, by whatever method performed, we cannot but trace the continued action of forces of the same character; since this operation takes place as a continuation of the process of growth, and under precisely the same influences."

The reciprocal actions which are constantly going on in the body, point out the impossibility of any of the parts of the great whole exercising an undue or superior influence, and we must admit that both the blood from which the nutritive material is taken, and the tissue or organism to be repaired exercise reciprocal influences on each other, as has been shown by Rokitansky and Mr. Paget. The condition of the blood in determining that of the effused plasma is very strikingly seen in the result of the application of the same irritant to the same parts in different persons.

Evidence may be obtained by examining the products of similar inflammations excited in several persons, in whom the state of the blood may by considered dissimilar; and here the evidence may be more pointed than in the former case; for, if it should appear that the same tissue, inflamed by the same stimulus, will, in different persons, yield different forms of lymph, we shall have come near to certainty that the character of the blood is that which chiefly determines the character of an inflammation. To test this matter, I examined carefully the materials effused in blisters raised by cantharides-plasters applied to the skin of thirty patients in St. Bartholomew's Hospital.—Doubtless, among the results thus obtained, there might be some diversities depending on the time and severity of the stimulus applied; still, it seemed a fair test of the question in view, and the general result proved it to be so.—For, although the differences in the general aspects of these materials were slight, yet there were great differences in the microscopic characters, and these differences so far corresponded with the nature of the disease, or of the patient's general health to whom the blisters were applied, that at last I could generally

guess accurately, from an examination of the fluid in the blister, what was the general character of the disease from which the patient suffered. Thus, in cases of purely local disease, in patients otherwise sound, the lymph thus obtained formed an almost unmixed coagulum, in which, when the fluid was pressed out, the fibrin was firm, elastic, and apparently filamentous. In cases at the opposite end of the scale, such as those of advanced phthisis, a minimum of fibrin was concealed by the crowds of corpuscles imbedded in it. Between these were numerous intermediate conditions which it is not necessary now to particularise. It may suffice to say that, after some practice, one might form a fair opinion of the degree in which a patient was cachectic, and of the degree in which an inflammation in him would tend to the adhesive or the suppurative character, by the microscopic character of these exudations. The highest health is marked by an exudation of the most perfect and unmixed fibrin; the lowest, by the most abundant abundant corpuscles, and their nearest approach, even in their early state, to the character of pus-cells. The degrees of deviation from general health are marked, either by increasing abundance of corpuscles, their gradual predominance over the fibrin, and their gradual approach to the character of pus cells, or else by the gradual deterioration of fibrin, in which, from being tough, elastic, clear, uniform, and of filamentous appearance or filamentous structure, it becomes less and less filamentous, softer, more paste-like, turbid, nebulous, dotted and mingled with minute oil-molecules.

The *true influence* of the tissue in this respect is best shewn in some of the cases in which inflammation, excited, apparently, by the same means, has happened coincidently in two or more very different parts in the same person. Thus we may find, *e. g.*, that in pleuro-pneumonia the lymph on the pleura is commonly more fibrinous than that within the substance of the lung; and adhesions may be forming in the one, while the other is suppurating. In cases of coincident pneumonia and pericarditis, the lymph in the lung may appear nearly all corpuscular, and all the corpuscles may show a tendency to degenerate into granule-cells, while the lymph on the pericardium may have a preponderance of fibrin, and what corpuscles it has may tend to degenerate into pus-cells. So, too, one may find, in the substance of an inflamed synovial or mucous membrane, abundant lymph-cells, while all the exudation on its surface may appear purulent.

And Mr. Simon observes,—

The determination of blood, though over and above the usual supply, may *admit of application* according to the ordinary and healthy functions of the part. The biceps muscle of a blacksmith's arm receives, perhaps, as much blood as all the muscles of my upper extremity put together; but there is no blood wasted—all that goes there is turned to account, and contributes to the development of a normal tissue. Or, the supply of blood may be *more than can be used* and appropriated by the organ so copiously supplied; and then it is that we can get a continued superfluity of exudation pervading the tissue, and find that superfluity undergoing an independent development into certain shaped products—cells or fibres, foreign to the healthy structure of the part.

Now, what I have just stated is the distinction between hypertrophy and inflammation; their general pathology has much in common—their causes are often alike—their modes of production identical. But in hypertrophy, however large may be the supply of blood it all goes to the true nourishment of the organ, goes to increase the number of its molecules; while, in inflammation, all that is redundant and unappropriated goes to the formation of new products. After what I have said of the pathological affinities of these two processes, it will not surprise you to be told, that in many organs of the body, hypertrophy and inflammation run into one another by almost insensible gradations—as, for instance, with secreting surfaces, where, after a certain time, that which at first produced a mere excess of secreted material, presently causes to be mixed with that secretion more or less albumin, fibrin, blood, pus and the like.

The exudation out of which these respective tissues are formed, even under disease, is thus described by Mr. Paget:—

It seems to us that the development of such low forms of tissue as arise in inflammatory effusions, is not to be attributed to the formative power of the solids, but to that of the blood, which seems to be augmented in somewhat the same proportion as that of the solids is diminished; and the influence of *the solid tissues* of an inflamed part seems to us to be rather employed in *degrading* the characters of an effusion, which, if thrown out in the form of an otherwise healthy tissue, or upon a healthy surface, would more readily pass into higher forms of organisation. The difference between the modes of development of such material, dwelt upon by Mr. Paget in his "Lectures on the Processes of Repair and Reproduction after Injuries," seems to us clearly to show, that whilst the *blood* acquires a higher plasticity when the state of inflammation is established, the inflamed tissues possess far less power of aiding in the development of the effused material, than do those surrounding a subcutaneous wound, which is advancing, with the least possible disturbance form inflammation, towards complete reparation. That in the proper substance of the inflamed part there is a decrease of formative power, is a point which can scarcely admit of question. All the changes which is undergoes, as Mr. Paget justly remarks, are signs or results of diminished or suspended nutrition in its tissues; they are all characteristic of atrophy, degeneration, and death;—precisely the doctrine, which, as Mr. Paget does not omit to mention, was taught in the pages of one of our predecessors, and there applied, we believe for the first time, to the explanation of other phenomena of inflammation.

Lastly, Mr. Paget observes that—

The tissue becomes soft or quite disorganised; they are relaxed and weakened; they are degenerate, and remain lowered at once in the structure, chemical composition, and functional power: or else, after degeneration, they are absorbed, or are disintegrated, or dissolved, and cast out: they die in particles or in the mass. During all the process of inflammation, there is no such thing as an increased formation of the natural structures of the inflamed part: they are not even maintained; their nutrition is always impaired, or quite suspended. It is only after the inflammation has ceased that there is an appearance of increased formation in some of the lowly organised tissues, as the bones and cellular tissue.

In the concluding portion of these notes, it will be my endeavour to show, from the various experiments and observations which have been made, that there is strong reason to believe that those bodies which are known as pus-corpuscles are arrested developments which either by degeneration or chemical influences contain fat.

That Purulent deposits or secondary suppuration in distant tissues can only be accounted for on the supposition that there is general disturbed condition of the system, under which the nutritive material is not perfected, but is degraded.

To be continued.

Correspondence.

THE PEOPLE *vs.* MEDICAL MONOPOLY.

(*From the Western Progress.*)

A letter, with the above heading, and subscribed a "Medical Reformer," appeared in the *Examiner* of last week—a journal which, like the *North American* and the *Globe*, is one of the strongholds of Quackery in Physic. The writer of the letter in question, it is easy to perceive, belongs to the innumerable bands of Quacks, who are levying black mail upon the ignorance, the folly and the superstition of the public. From several indications I take his place to be in the ranks of Homœopathy, one of the silliest and most mischievous delusions medical, that ever gained temporary credit or currency among mankind. I confess to a sort of satisfaction in catching these knaves in a public print, beyond the pale of the advertising columns; it affords one so favorable an opportunity of exposing the impudence of their pretensions, and the brazen falsehoods with which they support their claims. The first question which I shall examine, is this: Is the Medical Profession as at present constituted, or will it, if granted the corporate privileges which it seeks, be a MONOPOLY? Monopoly is a very hateful and unpopular word, and (rightly used) signifies a very hateful and unpopular thing. My friend, the Homœopathist, is fully aware of this fact, and with a dexterity worthy of a thimble-rigger, at once places the medical profession in an odious light, by dubbing it a "Monopoly." The Medical Profession neither is nor seeks to be a monopoly. All persons who are legitimate practitioners of physic have undergone a preparation and an ordeal which the laws of the country impose upon them; every individual in the land who subjects himself to the same process, can become a licensed practitioner, and place himself on the same footing as those already licensed; no obstacles to practice are interposed in the way of the now unlicensed, which were not interposed in the way of those now licensed. How can this be called a monopoly? Did the licensed practitioners seek to prevent any one from obtaining a license—that would in truth be demanding a monopoly; but they are not such idiots; they know that they are living in Canada and not in Hindoostan, and that any such attempt to organize a medical monopoly or *caste* would qualify them in public estimation for a lunatic asylum. A profession which all men are free to enter, on fulfilling conditions demanded alike from

all, has not a single characteristic of monopoly in its composition; a truth which requires only to be stated to ensure it acceptance in every intelligent and unprejudiced mind.

Having constituted the Medical profession a monopoly by a dash of his pen, my "Medical Reformer" goes on to show that educated practitioners and uneducated quacks ought to be put on a level, for these "truthful reasons," that "all systems of practice are good for some diseases, none good for all diseases," and that the whole theory and practice of medicine as taught by schools and followed by the "regulars," is little better than "the art of amusing the patient while nature cures the disease."

Very pretty, indeed, my man of infinitesimal doses! Very clever, very satirical, very witty and very wise to be sure, you are! No wonder you "IMPLORE" the exchanges of the *Examiner* to copy your letter, that the whole community may participate in the inestimable delight and advantage of perusing it! You are on such admirable and gracious terms with yourself that I almost repent having undertaken to prick the blown-up bladder of your conceit, afraid that the ridicule of the collapse will be too much for your nerves—it is an idle fear that, though; for

> "Who shames a scribbler? Break one cob-web through,
> He spins the slight, self-pleasing thread anew;
> Destroy his fib or sophistry—in vain—
> The creature's at his dirty work again!"

It is not in the hope, then, of convincing you, that I write; but you have thrust yourself forward as the spokesman of your fellow quacks, and a very apt type and representation of the genus you are; and I take the opportunity of shewing the "people," whom you affect so generously to take under your protection, what sort of stuff you and the like of you are made of. Unlicensed and licensed practitioners are to be placed on the same footing, you say, because the art of the "regulars" is little better than the art of "amusing the patient while nature cures the disease." A man, for instance, gets his hip, his shoulder, or his elbow put out of joint, or some of his bones broken; a "regular" is called in; to amuse his patient while nature cured the disease, would take up rather an inconvenient length of time; the "regular," having been instructed in his business, is able to reduce the dislocation or set the fracture; he does so, of course, from the selfish motive of saving his time, but certainly very much more to the *relief* than the *amusement* of his patient.

A woman is in difficult labour; it is a cross-birth, or a case of flooding, or one of the countless deviations from natural labour; ac-

cording to the correspondent of the *Examiner*, the "regular" has only to amuse his patient while nature cures the disease! Was ever stupidity equal to this? No man who has not been properly trained to his business can afford relief in a case of this kind; and if relief is not afforded, the death of mother and child is inevitable. An educated man can give immediate relief; and yet the art practised by the "regulars" is merely "the art of amusing the patient while nature cures the disease." The patient in a case of this kind finds the doctor anything but an *amusing* character.

A man gets wet and cold, and is attacked with pleuritic inflammation, with "side stitches that pen his breath up," such as Prospero threatened Caliban with. The "regular" treats him *secundem artem*, and in three days all traces of disease have vanished.

Had the "regular" occupied himself with amusing his patient instead of doing his duty to him, nature would have made a pretty mess of this case; inflammation would have been followed by effusion or water on the chest, death would very soon have taken the case out of nature's hands, or the patient would have remained through life a confirmed invalid, unfit for any of the duties or employments, which, thanks to the regular who never thought of *amusing* the patient at all, he can discharge as well as if he had never been sick.

These are cases which occur in "regular" practice every day: instances of a similar kind might be adduced *ad infinitum*, in which "the art of physic, surgery and midwifery," as practised by the "regulars," saves life, prevents lameness and deformity, relieves suffering and pain, and restores health, where the remainder of life would, without its assistance, be only a long disease.

Now, I too, will appeal to the people at large; I will ask them what they think of these quacks, who say that the art of the regular practitioner consists in the amusing of his patient while picking his pocket; a farmer's wife has been in hard labour (plain words must be used to express plain facts), for hours upon hours, and no signs of a termination; the husband and all the attendants are in agonies of anxiety he mounts his horse and rides a dozen miles for a doctor; does he go to the Homœopath, the Hydropath, the Thomsonian, the Eclectic, or one that wears the badge of any tribe of Quackery? not he indeed; he goes to the "Regular," the "Regular" returns with him—and where all was misery, confusion or dismay, in a short time all is order, peace, hope and comfort. I ask the people of this country, then, many of whom have had experience of the truth of the above picture, what they think of these Quacks—what reliance can be placed upon their

assertions or pretensions, when they advance as a "truthful reason," that the "regular" art of physic is little more than the art of amusing the patient, &c.

For my part, I do not hesitate to denounce these wretches as foul and false libellers and liars—utterly unworthy of trust and confidence. Neither do I hesitate to denounce the journals that patronize and defend them as recreants to their office and unfaithful to their duty.—We expect from a journalist that instead of pandering to ignorance and prejudice he shall vigorously assault them; that instead of maligning, insulting and depreciating science, he should give it its best assistance and support, even should the quacks withdraw their advertising custom, but the *Examiner*, the *Globe*, and the *North American* dare not take any such manly and independent position; the *purse* is in danger, and with all Scotchmen, the purse is a vulnerable point.

I have not have-done with the *Examiner's* correspondent yet, but what I have written is enough for one dose: if you will permit me, I will resume the subject next week, and should be obliged by a hint to that effect.

Yours, faithfully,

ANTI-QUACK.

TORONTO, AUGUST 15, 1852.

COLLEGE OF PHYSICIANS AND SURGEONS, CANADA WEST.

The question of Incorporation is one which deeply concerns the medical men of Upper Canada, and we believe that there never was a subject brought under their notice which received such unanimous support.

Some of our non-professional cotemporaries have completely misunderstood our wishes, and have endeavoured to prejudice the Legislative mind by inculcating, or rather attributing sentiments to us, which we do not entertain. Once for all, then, we distinctly state that our sole object is the improvement and elevation of the standard of professional acquirement, and of our general character. We desire to see a uniform plan of education established under a system which shall obtain our own confidence and that of the public also.

In England we find that there are several educational Colleges, many of them possessing the power of conferring degrees, yet their graduates must seek admission into the College of Physicians, or Surgeons' or Apothecaries' Companies, and in very many instances *before*

graduation in their respective Colleges. Now we should oppose most strenuously any interference with the vested rights of existing bodies, and would object at once to any curtailment of the privileges of University Boards in the Province. It is to them that we must look for the elevation of Education; it is our duty to strengthen their hands, and aid them in every legitimate way to give increased tone and standing to future aspirants to medical honours. Let, therefore, the Provincial College fix the standard with which it will be at present content, and below which no College can educate, and the honorable rivalry naturally existing between respective educational bodies will soon induce them to shoot higher above the mark.

An objection has been started against the establishment of one Provincial Board which we think really very weak. It is that Graduates would not like to submit to a *second* examination, after having obtained the degree of their own College. To this we answer that the Colleges themselves have the remedy in their own hands. If they wish to prevent the possibility of their candidates being "*plucked,*" let them make a bye-law never to confer a degree before the candidate has been licensed by the College of Physicians and Surgeons. We think this would be a wise and salutary caution, and must effectually prevent any improper deprecation of University honors.

That the Profession is labouring under serious disadvantages every one admits, and until we are placed in as favorable a position as our brethren of the Law, we shall continue to be degraded and low in the social scale. The Law Society require, as we have elsewhere stated, that all candidates for law honors shall pass examinations before themselves; and surely, if such be the practice with them, we cannot be wrong in adopting it.

The evils under which the Profession labours are many of them the effects of our own ill-judged acts. Instead of there being a close bond of union formed by the very character and nature of our pursuits, it must be admitted that paltry jealousies and the most contemptible feelings but too often estrange us. Our aim should be to work together as brethren, provoking one another to seek out and develope the truths of science—stimulating each other by an honorable rivalry, and never allowing a momentary feeling of disappointment to provoke us to wrong or injure one another.

The advantages which are offered by the General Hospital and other Institutions in Toronto and elsewhere, ought to be made available for purposes of study and observation; and instead of being obliged to hunt for papers for our periodical, its pages ought to be pressed

for room. As yet we have been but scantily supplied with such material. Now we do not wish to be misunderstood, nor have it supposed that we attach all the blame to the medical officers of the General Hospital: on the contrary we believe that there are difficulties in the way preventing that complete and perfect organization which would render the Institution very effective as a school and as an asylum for the sick. As the Hospital is at present conducted, the Students are unable to acquire that practical acquaintance with disease which is so desirable; for except during the visit of the Medical Officer, or by special permission, he is not permitted to enter the wards, to take down the history of any disease, or to witness the varying changes which a malady like fever may undergo.

In the General Lying-in Hospitals of this City, Students have for two years been allowed to enter the wards at regular hours, for the express purpose of gathering the daily history of cases, and in no one instance has there been either complaint, nor as far as we can learn, cause for complaint. Their conduct has been excellent, and such as we would expect from those who are treated as Christian gentlemen.

Medical students are, by the generality of persons, looked on with feelings of rancorous dislike, and they are shunned and put aside by those whose very duty it is to win them to good ways. If, instead of snarling and snapping at their foibles, and taking every opportunity to stigmatise them, the teachers of religion would seek their society, and use them as helps to soften and win the confidence of the patients, we should soon see a very great difference in the conduct and characters of pupils. We have seen the good effects of such a course at Guy's Hospital and other London Hospitals, and we therefore insist on such an one being pursued here. If the student was allowed to aid in collecting the details of cases for the Physician and Surgeon, accurate information would be obtained against the time of visit, and the patients would soon learn to feel the benefits of that sympathy and kind care which is gratefully displayed in large Hospitals. Thus would they become familiarized with disease, and teach a discerning public that the medical student is not different from his fellows, but can, under a kind and friendly persuasion, be brought to compare in acts of sympathy and charity, with their reverend Pastors.

These and other evils must of necessity be mitigated. If we had in Upper Canada a College authorized to watch over the interests of the Profession, we should then be enabled systematically to set about placing the whole educational system on such a footing as to convince students that there was no necessity for their going to the Hospital,

and Schools of the United States for information which is lying within reach of their own home; and the Colleges and Schools of Upper Canada would not be disgraced by the annual migration of their students from these shores.

THE CLIMATE OF UPPER CANADA.

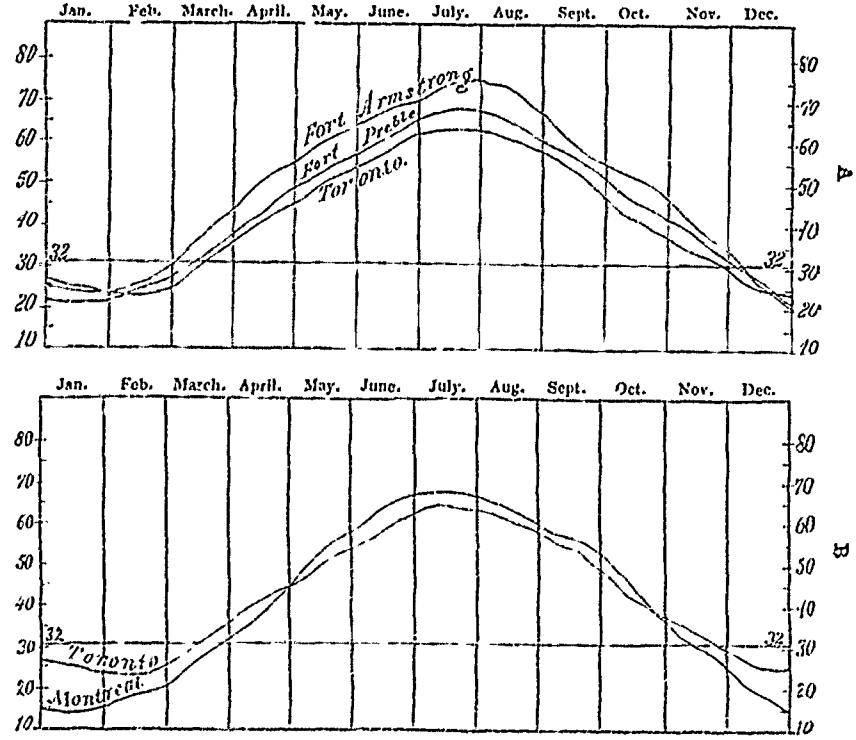
THE diagrams on the opposite page have been reduced from a series exhibited by Professor Hind at the late conversazione of the Canadian Institute. The diagram marked A exhibits the approximate curves of the mean monthly temperatures at Toronto, Fort Preble, and Fort Armstrong. The climate of Toronto (lat. 43°·39, long. 79°·) may be taken as an illustration of the ameliorating influence of the great lakes upon the climate of Western Canada generally.

The curve for Toronto is deduced from the metereological observations made at H. M. Observatory in the neighbourhood of that city, under the able direction of Capt. Lefroy, R.A., F.R.S. Dr. Forrey's treatise upon the climates of the United States, has furnished the data for tracing the curve exhibiting the mean monthly march of temperature at Forts Preble and Armstrong.

Fort Preble is situated in the State of Maine, in lat. 43°·38 and long. 70°.18, it is, therefore in the same latitude as Toronto, and nearly 9°· long. east of that place. Fort Armstrong, in the State of Illinois, in lat. 41°·28 and long 90°·33, is upwards of 2° of latitude south, and 9° of longitude west, of Toronto.

It will be observed upon inspection of the diagram, that the general direction of the curve for Toronto exhibits a much milder climate than do the corresponding curves for Forts Armstrong and Preble. From March to November, both inclusive, the march of temperature progresses with considerable uniformity at all the stations. At Fort Armstrong,—as illustrative of the climates of the Western States,—we observe the intensity and duration of summer heat strongly characterzed: the direction of the curve for the months June, July, and August, corresponding to the mean monthly temperatures of 73°·59, 77°·82, and 76°21; the temperatures at Fort Preble and Toronto being respectively 64°·29, 69°·71, 67°·19, and 61°·42, 66°·54, and 65°·76.

The diagram marked B exhibits a very remarkable difference between the climates of Toronto and Montreal. High summer and low winter temperatures,—the characteristics of the climates of the North-western and North-eastern States of the Union remote from the lakes,—distinguish the true continental climate of Lower Canada from the almost maratime climate of the Upper Province.


Jan.
Feb.
March.
April.
May.
June.
July.
Aug.
Sept.
Oct.
Nov.
Dec.
80
70
60
50
40
30
20
10
32
Fort Armstrong
Fort Prette
Toronto.
A
Jan.
Feb.
March.
April.
May.
June.
July.
Aug.
Sept.
Oct.
Nov.
Dec.
80
70
60
50
40
30
20
10
32
Toronto
Montreal
B

MONTHLY METEOROLOGICAL REGISTER, at

Latitude, 43 deg. 39.4 min. N. Longitude, 79 deg. 21.5 min. W.

Mgt.	Day	Barom. at ten of 32 deg. *				Temperature of the air †				Tension of Vapour			
		6, A.M.	2, P.M.	10 P.M.	MEAN	6 A.M.	2 P.M.	10 P.M.	MEAN	6, A.M.	2, P.M.	10 P.M.	MEAN.
						°	°	°	°				
c	1	0.031	0 000	0 156	0 095	16	11 6	6 0	6 9	0.381	0 479	0.359	0.388
c	2	0 179	0 124	0.081	0 124	2 6	5 8	4 7	5 2	356	360	25	319
c	3	0.033	0 004	0 020	0 005	6 7	1 3	5 0	5.1	259	417	374	345
c	4	0 060	0 059	.....		1 5	0 3			377	490		..
d	5	0 055	0.088	0 049	0 058	2.5	0 1	1 2	1 3	396	497	416	439
c	6	0 047	0 071	0 031	0 048	3 3	1 9	4 2	2 7	445	613	509	514
c	7	0 057	0.095	0 044	0 032	4 0	12 5	8.3	8.0	494	898	614	647
c	8	0 051	0.095	0 031	0 092	10 7	11.6	9 9	10 8	567	798	605	656
d	9	0.031	0 053	0 023	0.036	11.0	5 9	6 7	4 3	589	693	644	622
c	10	0.081	0.110	0 049	0 079	8.4	5.5	6 7	6.6	496	644	535	561
c	11	0 024	0 123	.....		7 9	7 5	..	..	576	752	..	....
d	12	0.235	0.244	0.147	0 202	1 2	6 0	3.5	3 3	427	457	401	425
c	13	0 015	0.026	0 029	0 009	2 0	7 4	2 7	2 4	460	546	486	493
d	14	0.034	0 074	0.125	0.184	1 1	2.8	0.3	1 0	437	432	405	411
d	15	0.195	0.244	0.240	0.229	3 1	0 1	1 8	0.5	429	435	336	417
imp	16	0.242	0 21	0.091	0 172	3.4	4 0	6 9	1 2	365	442	361	400
c	17	0 014	0 007	0.017	0 008	0 7	2.2	5 0	3.0	377	500	505	477
a	18	0 144	0.226	...	....	0.2	7 7	..	..	329	439	....	..
a	19	0 292	0.335	0 268	0 296	7 4	3 0	6 4	5 1	315	362	357	357
a	20	0 200	0 212	0.193	0 219	4 9	4 0	3 4	2 9	375	574	529	506
a	21	0.119	0.059	0 010	0 088	4 3	15 2	9 7	10 5	520	516	497	525
a	22	0.004	0 041	0.091	0 095	11 1	9 0	4 3	8.6	547	629	401	488
b	23	0 024	0.043	0 076	0.054	1 0	2 6	1 8	2.9	403	513	454	461
b	24	0 119	0.116	0 064	0.097	1 9	0.9	2 3	0 6	318	429	411	415
b	25	0 095	0 028	...		0 7	5 1	..	..	426	516	....	..
b	26	0.191	0 166	0.074	0 110	2 9	3.8	0 9	1.5	503	459	346	433
c	27	0 066	0 037	0.100	0.047	2 7	2.7	6 0	3.4	334	483	324	372
c	28	0 097	0 069	0.078	0 019	1 5	2.4	7 6	0 7	375	466	467	447
d	29	0 216	0.3 8	0 411	0 324	6 7	5 2	8 1	6 8	544	679	264	572
c	30	0 469	0.368	0.251	0.445	5.1	0 8	1 0	0 2	510	413	384	418
c	31	0.195	0.094	0 0 6	0 096	5 2	10 1	11 4	9 6	356	353	297	329
Mean: Normal..		29 604	29 583	29.589	29 589	59.75	73.91	62 59	66 32				
Observed		29 626	29 616	29 600	29 612	60 31	75 29	62.51	65 68	0 431	0.515	0.427	0.461

* Above or below the mean. † Above or below the mean.

Highest Barometer ..... 29 918, at 8 a.m on 19th } Monthly range
Lowest Barometer.....29.135, at midnigt on 29th } 0.783 inch.

High'st obs'v'd te'perat're 99. ° 1, at 3 35p.m. on21st } Monthly range
Lowest registered " 48 5 at a m. on 31st } 41°.6

Mean highest observed temp. 74° 99 } Mean daily range:
Mean registered minimum 56.65 } 18°.33

Greatest daily range, 29° 3, from 4.30 p.m 22d to a.m of 23rd.

Warmest day, 21st. Mean temperature, 76° 72 } Difference,
Coldest day, 31st. Mean temperature, 56 47 } 20°.25

15th—At 9 0 p.m , large meteor in N falling in a line from alpha Lyræ through Polaris.

The "Means" are derived from six observations daily, viz:—at 6 and 8, a.m.; and 2, 4, 10, and 12 p. m.

The column headed "Magnet" is an attempt to distinguish the character of each day as regards the frequency or extent of the fluctuations of the Magnetic declination, indicated by the self-registering instruments at Toronto. The classification is to some extent arbitrary, and may require future modification, but has been found tolerably definite as far as applied. It is as follows:—

(*a*) A marked absence of Magnetical disturbance.

(*b*) Unimportant movements,—not to be called disturbance.

(*c*) Marked disturbance,—whether shewn by frequency or amount of deviation from the normal curve,—but of no great importance.

(*d*) A greater degree of disturbance,—but not of long continuance.

(*e*) Considerable disturbance,—lasting more or less the whole day.

(*f*) A magnetical disturbance of the first class.

The day is reckoned from noon to noon. If two letters are placed, the first applies to the earlier, the latter to the later part of the trace. Although the declination is particularly referred to, it rarely happens that the same terms are not applicable to the changes of the horizontal force also.

Toronto, August, 1852.

H.M. Magnetical Observatory, Toronto, C. W.—JULY, 1852.

Elevation above Lake Ontario, 108 feet.

Humidity of Air				Wind			Rain: inches	Weather.
6 AM	2PM.	10	Mn.	6, A.M.	2, P.M.	10,P.M.		
.87	.87	.87	.85	N b E	NE b N	N	0.590	Overcast. Constant rain from 0.40 pm. till 10pm
82	57	60	66	NW	W N W	W b S	... .	Cloud'd with d'tach'd masses. Halo rou'd moon
70	58	86	70	W	W	Calm	.....	Few light clouds am. Uncloud'd, very fine pm.
84	63	..	..	Calm	S	S	.....	Am unclouded. A few passing clouds pm: Fine.
92	65	77	78	Calm	ESE	Calm	mapp	Clear with faint aurora fr'm 10pm cont'g till mid
84	75	84	78	Calm	Calm	"	0.200	Thund'rst'm 7 am, auroral streamers 10 to 11 pm
90	69	88	82	Calm	Calm	"	..	Constant lightn'g fm 8 pm, aur lt 9 30 till midt.
81	69	84	76	Calm	SSE	"	....	Gen'y clear, sheet and forked lightning 8 to 9 pm
84	93	96	89	Calm	S b W	"	0 960	Heavy thunderstorm 1 to 3 pm, rain and hail.
76	67	79	74	N b E	E b S	NNE	. . .	Faint auroral light from 9 30 pm till midnight.
90	53		.	E b N	SSE	W b N	0 180	M'ly cloud'd, thu'd'st'm, lt ni'g, rain 3 to 3.30pm
90	71	83	78	W b S	SE b E	Calm	0 005	Faint aurora from 10.45 pm, shower during night
87	87	81	86	E b N	E	NNW	0 130	Occasional showers, distant thunder during day.
91	49	73	66	NWb W	NW b N	N b W	....	Almost unclouded. Very fine.
76	59	72	67	N	S b E	Calm	..	Unclouded but slight haze diffused. Very fine.
82	48	83	68	Calm	SSE	E b N	..	Very heavy dew a m, light clouds and haze, fine.
73	57	74	68	N b E	SSE	N	.	Light clouds dispersed, generally clear and fine.
65	71	.	..	NEbN	SE b S	Calm	..	Light clouds am, unclouded and very fine p.m
81	55	81	69	Calm	E S E	ENE	..	Generally clear, a few passing clouds, fine.
90	61	85	75	Calm	S b E	S	....	Light clouds and haze during day, clear fm 10pm
90	39	65	6.	Calm	SWbW	Calm	.....	Light detached clouds, haze round horizon, fine.
75	57	64	5.	Calm	S	NWbN	....	Unclouded, haze round horizon, fine day.
78	60	77	6	Calm	SSE	N	.....	Almost unclouded. Very fine day.
74	54	81	6.	N bE	SE	Calm	.....	Light flexuous clouds dispersed, mostly clear.
83	81	..	.	Calm	Calm	"	0 160	Ov'cast, lt rain fm 1.40 pm. Foggy at midnight
91	49	65	6	Calm	NNW	NNW	. ..	Light passing clouds. Mostly clear.
71	51	73	60	NWbN	S b E	NE	. ...	Almost unclouded Very fine.
79	53	85	7	Calm	SE b E	Calm	.. ..	Clear, save a few passing detached clouds, fine.
86	71	72	7.	Calm	S	WSW	1.860	Th'd'st'm 2 to 4 pm great do. fm 10.30pm tomid
91	5.	72	60	SWbS	W b N	NW	..	Dense passing clouds, with large clear intervals.
86	57	81	75	Calm	W b N	Calm	.....	Gene'ly overcast, light clouds, faint aur. at 11pm
				Miles:	Miles:	Miles:		
.83	.63	.77	.72	1.63	5.83	1.81	4 025	

Sum of the Atmospheric Current in Miles, resolved into the four Cardinal Directions:

North.	West.	South.	East.
1063.59	977.60	551.28	505.17

Mean velocity of the wind—3.33 miles per hour.
Max. velocity—17 9 miles per hour, from 2 to 3 p.m. on 30th.

Most windy day—30th: mean velocity—10 16 miles per hour.
Least windy day—25th: mean velocity—0 20 ditto.

Hour of greatest mean velocity—3 p.m: mean velocity—5 84 do.
Hour of least " —1 a m.: do. —1.10 do.
Mean diurnal variation—4.74 miles.

The velocity of the wind for a space of four minutes during the thunderstorm on the night of 29th, was at the rate of 38 miles per hour.

COMPARATIVE TABLE FOR JULY.

Year	Temperature				Rain		Wind.
	Mean	Max.	Min	Range	Days	Inches	Mean velocity.
1840	65 84	79.4	48 2	31.2	6	5 270	Miles.
1841	65 01	86 3	43.2	43.1	10	8 150	
1842	64.28	90 5	42.0	48.5	4	3.050	
1843	64.13	86 1	40 2	45 9	8	1.605	
1844	65.61	86 1	40.5	45.6	12	2.815	
1845	66.24	91.6	45 6	49 0	7	2.195	
1846	67.7	94 0	44.9	49 1	9	2 895	
1847	67 95	87 5	43 8	44 7	8	3 355	
1848	65.82	82 7	46 7	36.0	10	1 890	4.91
1849	64.31	89.1	51 0	38 1	4	3 415	3.52
1850	69 01	81.9	52 8	32.1	12	5.270	4 56
1851	65.41	82 7	52.1	30.6	12	3 625	4.13
1852	66.68	90.1	49.5	40.6	8	4.025	3.33
Mean	66 31	87.23	46.19	41.04	8.5	3 889	4.10.

NOTICE TO OUR SUBSCRIBERS.

In sending forth the August No. of our Journal, we have to ask indulgence for the defects which will no doubt be discovered in it. Up to this time it has received the warm support of the Profession, and has confessedly tended to improve the position which we held in Society, and has been the means of disseminating information on a variety of subjects which would otherwise have remained dormant. To effect these important objects, it was necessary that the Conductor of the Journal should have possessed a cultivated mind, generous, honorable feeling, and withal great firmness and honesty of purpose to avoid all mere party brawls, and independently to stand forward the asserter of the rights and privileges of the whole Profession. We unhesitatingly declare that these qualifications were enjoyed in a high degree by our friend, who had lately the gratification and honor of filling the Editorial chair; and we have reason to know that many of his brethren in Toronto, who appreciated his zeal and intellectual ability, deeply regret his resignation of so important a trust. The duties of Editor of this Journal now devolve on other hands, until some better arrangement can be made; and as the Profession at large will necessarily require some definite pledge for the correctness, of the principles on which the periodical will be conducted, we think it right to state that no changes will be made, nor will there be any deviation from, the plan hitherto pursued.

The object of the work is, 1st, the diffusion of information, and the general improvement of the Profession, by forming a common centre through which our wants and wishes might be expressed.

2nd, The advocacy of those principles which tend to raise the character of the medical man, and place him in the foremost rank in society.

3rd, To bring the Profession more closely together, by cultivating sentiments of mutual esteem and regard—avoiding all selfish and narrow views.

It cannot be expected that the opinions enunciated will always be unanimously entertained; but as we feel convinced that no society can be perfected except through differences of opinion, our aim shall be to let those differences be so brought out, as to be the means of improving ourselves, without the necessary involvement of personal ill-will.