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CANADA

MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

Experience among some of the Wounded who fell at the Battle of Limestone Ridge, June 2nd. By WILLIAM CANIFF, M.D., &c.

Believing that the medical profession of Canada, in common with every other class of the community, has regarded with patriotic eagerness the events connected with the Fenian invasion, I have thought it might prove interesting, if not instructive, to supply to the Canada Medical Journal such facts relating to the wounded as came under my notice, especially those with which I had to do. Others who acted a more prominent part in treating the wounded I trust will furnish their experience, so that what I may say will prove valuable as a connecting link.

Actuated by a feeling which I doubt not many of my professional brethren experienced, to do something in the common cause, I, as soon as aware that fighting had commenced, set out for the scene of action. Leaving Belleville, on Saturday the 2nd inst., the day of the battle of Limestone Ridge, I proceeded to Toronto, which place I reached Sunday morning between four and five o'clock. Learning that a military train would leave at ten for the front, I arranged to go thereby, and through the kindness of Col. Mountain I was enabled to reach Hamilton at about mid-day. I there joined a company of Hamilton physicians who were about to leave by a special train, carrying provisions and necessaries, to render any service that circumstances might require. Shortly after passing St. Catherines, we crossed a train bearing some fifteen of the wounded from Port Colborne. They were to be taken to St. Catherines, where an hospital was being hastily prepared for their reception. Our train reached Port Colborne about four in the afternoon. The place was thronged with the military and civilians. Already an hospital had been established here under the care of Surgeon Ryal of the 13th Battalion, and everything

was being done that could possibly be in the way of nursing, comforts, &c. Dr. Beaumont of Toronto, was in constant attendance in consultation with Dr. Ryal. There were nearly twenty then in hospital. I did not examine many of the wounded. Indeed there seemed a serious danger that the crowd of medical men who had arrived, would prove to be deleterious to the wounded who so badly required repose. I casually glanced at a few, two of whom were Fenians. One of these had been at work in Hamilton until a few days before the invasion. One of them had received a scalp wound, and the other a cutaneous wound near the groin. Towards night, a boat arrived from Fort Erie with a few wounded under the care of Dr. Elliott, who had been active during the engagements, and who had found it necessary to amputate at the thigh one of the unfortunate men. At Dr. Elliott's request, I examined this patient immediately upon his arrival, and found that notwithstanding the shock incident to battle, and the subsequent operation, and then the removal, he was doing very well. Dr. Elliott had him conveyed to the hospital and placed under the care of Dr. Ryal, and then returned to his post at Fort Erie.

The following morning I was requested by Dr. Ryal to proceed to the battle field and take charge of the wounded yet remaining there. Mr. Routh of Hamilton, specially desired me to take charge of his brother, Lieutenant Routh, who was very severely wounded. Taking such medicines and comforts as were available, I at once started by an impressed carriage. I reached Ridgeway, distant from Port Colborne twelve miles, at eleven o'clock. The battle field lies somewhat to the north of Ridgeway and something over a mile. I was told at Ridgeway village that the wounded were at the "Smuggler's Home," and a small house close by it. Upon my arrival, I found Dr. Billings of Hamilton in charge of the wounded, also Dr. Allen of Brantford, a homeopathic physician who had been early upon the field. Dr. Billings wished to return immediately to Hamilton, and after explaining to me the nature of the wounds and what had been done, took his departure. During the time he had been there he had done much to make the wounded comfortable. Finding them lying in the clothes in which they had fallen in the field, he had assisted to place them in comfortable beds, and in other respects had contributed to their comfort. There were two of the wounded in a small house, Lieutant Routh of Hamilton, and Private White of Toronto. A third, Private Lugsten, of Toronto, was at the Smuggler's Home, a tavern, which was not far off. The small house had been inhabited by a German, and was by no means cleanly. It consisted of two rooms, a patient lying in each. Lieut. Routh was in the kitchen part, the cooking stove being

very near his bed. I immediately ordered a place to be prepared without for the stove, and to have it removed forthwith, which was accordingly done. Kind-hearted but thoughtless visitors were constantly coming to see the wounded. This was strictly forbidden; and quietude of body and mind was in in every possible way promoted.

As to the nature of the wounds I shall not speak with any great certainty. I found the patients placed in bed after having been examined by not a few medical gentlemen. It was not my duty to subject one of the poor but brave fellows to a moment's needless examination, to satisfy my curiosity. In the main I accepted the opinions that had been offered respecting the character of the wounds, and acted accordingly. Private White, who first came under my notice, I was told had received a ball through his left arm breaking the bone and entering the body, and was supposed to remain somewhere near the spine. Dr. Billings had placed the arms in splints; and ice water was being applied to the arm and also to the chest. The patient said this gave him great relief, and it was consequently continued. In the after part of the day his arm became very painful, and I removed one splint and placed the arm upon a pillow quite exposed and had ice directly applied. This gave him great relief.

Lieutenant Routh, I was told, had received a chest wound, and that the ball had pierced the lung and made its escape. I could not see the posterior wound, but from the situation of the anterior one, which was external to the heart, I thought it quite possible the lung had escaped. I found no symptoms of lung wound. The shock had evidently been severe to the unfortunate fellow, but from this he was rapidly rallying. From experience I had during the American war, I was enabled to offer a more favourable opinion than had been given before, which had a very happy effect upon him. Private Lugsten I found occupying a more comfortable room at the tavern. His brother and a few friends were giving him evidently too much attention. He was supposed to be very near his end, his case having been pronounced hopeless. His was also a wound of the chest, the ball having passed somewhat to the right side, perhaps at about the sixth rib. Here again I was led to doubt whether the ball had entered the lung. He was in a state of great excitement and restlessness, the pulse being in the neighbourhood of 140. I was inclined to attribute this in part, to the incessant conversation kept up respecting his own willingness to die. I at once told his brother that as he was well prepared to die it were much better to stop talking to him and keep the room very quiet. Quite contrary to the opinions which had been given, I said I certainly thought he might get well. I asked Dr. Allen, who was in immediate attendance what he was giving him, and he said nothing. I

suggested something to lessen the heart's action, and inquired if he had antimony or digitalis, (I was then ignorant of the fact that Dr. Allen was a homeopath,) he replied no, but he had veratrum viride, which he said he would give in any dose I suggested. I saw Lugsten at a later hour of the day, when I found he had been resting—the action of the heart was much reduced; and there was a gentle moisture of the skin. At this time, accidentally hearing that Dr. Allen was a homeopath, I felt it my duty to say to that gentleman and to the brother of the wounded man, that I could not consistently continue in consultation. Dr. Allan replied that we were there as surgeons not as physicians. To this I said that medicines were required to control the heart's action, which I thought could not be done by infinitesimal doses. He said that was no dogma of homeopaths, and that he was not giving the medicines in such doses. The brother, who knew me very well when practising in Toronto, and who was a believer in homeopathy, earnestly begged me to remain in consultation. I pointed out to them that inasmuch as homeopaths professed to practice medicine upon theories directly antagonistic to ours, as they incessantly declared they had discovered a true theory which showed ours to be wrong, it would not only be inconsistent but absurd for us to consult together. I further said that if surgical interference were required I should, under the circumstances, sink my objections. However, at the earnest and renewed request of the brother I did call, as a friend, occasionally, while I remained at the place. I have fully stated these facts, because at the present time there is some feeling among the members of our profession against one or two who do not refuse to consult with homeopaths. I cannot conclude the matter without speaking of the very gentlemanly manner in which Dr. Allen conducted himself towards me. My time henceforward was confined to the two other wounded. By this time the small house in which lie my own patients, began to assume somewhat the appearance of comfort. Under the hands Lieut. Routh's brother, mother, and young wife, who had come since my arrival, much had been done, not for the brother alone, but the wounded comrade. Up to this time bread, butter and milk constituted the best of the food which could be procured, for the Fenians had thoroughly stripped every farm house; but now some beefsteak and eggs improved the appearance of the table, which was set in the wood-shed. Upon inquiry I learned that neither of my patients had a motion of the bowels for several days. I had not many drugs with me, and gave each a dose of calomel, and an anodyne draught. The house possessed but one bed beside those occupied by the men. This was brought by Mr. Routh. While this bed was set apart for the two ladies I am sure neither slept, from the fact that I always found one by

the side of the wounded. I was glad to be allowed to recline upon a short bench, having for a covering an outside coat which had belonged to one of the dead. I had found it necessaay to change the position of the arm of private White, after which he slept for some hours. Routh did not rest so well. Towards midnight the pulse of each was considerably increased in frequency; but this was in a few hours remedied, and in the latter part of the night they both rested tolerably well. Probably they would have rested better had it not been for the shameful conduct of a person in an officer's uniform, who having purloined a bottle of stimulant, made himself somewhat noisy. In striking contrast to him I must mention the kind nursing of an elderly lady, Mrs. Rebecca Danner, upwards of sixty-five, who lives within a mile of the battle-field. When our volunteers were advancing, she, like a prudent woman, at once thought they would be wanting bread, and set to work to bake for them; before she had completed this noble-hearted work, she was alarmed to see them retreating in some confusion. They rushed to her door demanding water! water! She and a young daughter, who indeed where the only females who had not left the neighbourhood, drew water for them—first one and then the other; and when there would be a lull, they would fill pails and vessels of every kind for the next that came; but, bye-and-bye, Fenians came. These suspicious wretches would not drink the water thus drawn, lest it had been poisoned, and the women were compelled to draw afresh. It is but justice to say that they treated Mrs. Danner and daughter unexceptionably. Shortly after Mrs. Danner set out for the battle field, and all the night long was engaged with others (among whom was her sister, a Mrs. Douglas, and a young lady) in administering to the wants of the agonized men. It was this Mrs. Danner who sat up the livelong night, and patiently and tenderly watched over private White, applying ice to the wounded arm and wetting the dry lips. In this connection I must mention the name of Dr. Brewster, who lives at Ridgeway, and who, when our forces retreated, advanced into the enemy's lines and did all he could for the brave fellows who had been left behind. He worked all the day and all the night, in connection with the brave, noble woman, to whom I have referred. The following morning I found the three men in all respects better. My own patients were particulary comfortable. The bowels had not yet moved, so I proceeded to administer an injection to each, having brought with me the necessary syringe. After repeating, each had a free evacuation. But I now felt it my duty to leave. Information reached us in the forenoon, direct from Toronto, that fighting had commenced below Kingston. As my fellow townsman had been ordered to that point, and my family lived in that region, I deemed it right that

I should depart, notwithstanding the interest I already felt in my patients. Mr. Routh at once acknowledged the superior claim, and immediately sent a telegraphic dispatch to Hamilton for Dr. Mullin to take my place. Before leaving I took every pains to make the men comfortable, and secured the willing promise of Dr. Brewster that he would see them every few hours until my successor arrived.

My short stay did not allow me to become fully acquainted with the course the balls had taken; but I left a note offering my opinion that in the case of private White, he ought to be very shortly put under the influence of chloroform and the fracture of the humerus thoroughly examined —that I believed the bone was badly shattered—that pieces would require removal—that very likely the fracture had extended into the shoulder joint and that amputation would be found necessary. According to the daily prints of Toronto this operation has subsequently been performed. By the same authority I have learned that the other patients are in a fair way of recovery.

I have to acknowledge the kindness of Mr. Spicer, of the Grand Trunk Company, in affording me facilities in going to and from the battle field.

Belleville, 19th June, 1866.

Three Cases of Fracture of the Skull. By Thomas Simpson, M. D. *Sault Ste. Marie.*

The following abridged notes of three cases of fracture of the skull seriously complicated, occurring in my practice within eighteen months, are, I think, of sufficient interest to warrant their publication, as furnishing additional instances of grave injury to the skull, unaccompanied or followed—at least in two out of three—by constitutional disturbance of such consequence as to demand special consideration. Indeed in the last case in which the depression in the centre of the fracture considerably exceeded the thickness of the skull, and a circular portion of dura matter over an inch in diameter was completely destroyed and removed, there was not, from the commencement, any derangement of the functions of the nervous system, or any constitutional disorder. The danger to be apprehended in a case of this description is the supervention of diffuse menitgitis, which generally proves fatal. Always imminent, it almost invariably follows a comminuted fracture in this situation, if by any chance the careful and complete removal of every detached fragment of bone, or sharp projecting spicula in contact with the dura mater, be neglected.

In the case of L——, I think from the time and manner of recovery, we

may infer that the insensibility was owing in a great measure to alcohol; still I think the bleeding was warranted by the state of the skin, pulse, and the nature of the accident.

August, 1863. Called one evening to see L——, aged about 50 a confirmed drunkard, who I was told had been seen staggering into a stable about one hour previously, very drunk, and was found a short time afterwards lying insensible on the floor, and bleeding from the temple.

The following appearances were noted—face flushed, pupils dilated and sluggish, breathing slow and occasionally sterterous, pulse 60 and full, skin bathed in perspiration. Insensibility almost complete, which his friends insisted was caused by drink. He frequently raised his hand to his head as if to arrange his cap, which he had been in the habit of wearing constantly, in order to conceal his baldness; no contraction of muscles, twitching or convulsion; liquid swallowed with little hesitation. A wound one inch in length situated on the left side of head, a little below the middle of the temporal ridge, and leading to a fracture having, from size and shape, evidently been inflicted by the calk of a horse's shoe: it extended through both tables, and the depression was very apparent. On removing the clots and washing the wounds, an artery commenced to bleed freely; the blood was allowed to flow until the pulse became reduced in volume. The wound was then dressed—cold applications to head. Directed an enema to be given at once, and to be followed in four hours by a dose of calomel and jalap. On the morning following he was sitting up somewhat excited; answered rationally when questioned; had been restless towards morning; had got up whenever the medicine operated; in short, he had the usual appearance and manner of a drunkard on the morning following an evening's debauch. One week after the accident he was walking about as usual; and in six weeks I met him on the road, drunk—the wound still discharging pus freely; in two months the wound had completely healed, leaving a marked depression.

November, 1863. Called to see C——, a miner, aged 52 said to be seriously injured underground by a premature explosion. Note the following one hour after the accident—conciousness unimpaired, calm, pulse 58 and full, skin moist and cool, respiration natural, face contused and swollen; both eyes injured, the right slighly, the left completely destroyed, the humors having escaped. Wound immediately above the inner angle of left eye, about an inch in depth, extending through the orbital plate of frontal, and of sufficient width to admit of the easy introduction of the index finger. Extracted three loose fragments of bone. There was also a fracture of the sup. maxilla at their junction an inch and a half in breadth, displacing the alveolar and palate processes backwards,

requiring considerable force to rectify, and a fracture of both bones of the right forearm at about their middle. He complained for a few days after the accident of headache, and suffered from slight fever, but made a rapid recovery.

January, 1865. T——, a powerful healthy miner, aged 46, had fallen into a shaft 24 feet in depth, its bottom covered by masses of broken rock. Saw him in about an hour after the accident. Found him in bed, calm and sensible, with the following injuries: Fracture of the left thigh through the trochanter major, fracture of both bones of the left forearm about two and a half inches above the wrist and slight displacement of the bones of the carpus in their relation to each other, as well as to the radius and ulua, a wound of the scalp situated at the superior part of frontal bone near the median line, which discovered a fracture of the skull deeply depressed, of oval shape, about one and half inches in length and one inch broad, in a line with and one inch to the right of the sagittal, and crossed by the coronal suture, an additional fissure extending an inch down the frontal bone. By means of dissecting forceps, elevator and scalpel, eighteen pieces of bone, ranging in size from small spiculæ, to a fragment one inch in length and half an inch wide, were removed. In none was the full thickness of the skull represented, each being a part of the outer or inner table with a portion of diplôe. The fracture of the inner table was much more extensive than that of outer. The dura mater was much torn, and shreds removed with the bone. Convolutions of the brain *seemingly* shrunken, giving one the idea that the brain did not fill completely its proper chamber. No wound of the substance of the brain noticeable. Hæmorrhage slight. Tepid water dressing to wound. On the second day the chasm was completely filled with a firm, gelatinous, semi-transparent substance of a pale red colour which slowly scaled over, the wound healing rapidly under the incrustation, with a trifling discharge of healthy pus and without a single untoward symptom. At the end of ten weeks he was able to go about on crutches.

Lectures on the Diseases of the Eye, recently delivered before the Ophthalmic Class of the Toronto School of Medicine. By A. M. ROSEBRUGH, M.D.,

(*Continued from page* 452.)

PART II. LECTURE 1. THE OPHTHALMOSCOPE.

The invention of the ophthalmoscope by Helmholtz, in 1851, marks the commencement of a new era in ophthalmic medicine and surgery,

for up to that time nothing was, or indeed could be known of the appearance of the deep structures of the living eye, either in health or disease. With the help of this instrument however, all the parts of the eye involved in vision can be brought under the eye of the surgeon. We can now determine the existence and variety of cataract even in its earliest stage. It demonstrates floating bodies in the vitreous humour as well as turbidity of that body. The ophthalmoscope discloses apoplexy of the retina in which mercurials would be contra-indicated; it also shows us cases where lymph is effused upon some of the deep structures of the eye in which the influence of alteratives may prove advantageous. It reveals the characteristic appearances of glaucoma which indicates a resort to *iridectomy*, an operation which has been very successful in relieving blindness from this cause. In short we may say with Hasner "what the telescope is to astronomy, the ophthalmoscope is to ophthalmology."

These are a few of the many uses of the ophthalmoscope, and if we wish to keep pace with the advances of ophthalmic science, it is absolutely necessary for us to thoroughly understand the use of this instrument.

In the short space of one lecture however, I can not hope to do more than give some hints that may prove of service to you in your future investigations of this subject. Those who wish to pursue it further should refer to the elaborate works of Rainy, Hulke, and Zander, and to the coloured plates of Liebreich.*

The cause of the blackness of the pupil.

It is well known that under ordinary circumstances the pupil of the eye appears to be perfectly black, and that all parts behind it are perfectly invisible: this was formerly thought to depend on the complete absorption of all the rays of light that fall upon the fundus or posterior internal surface of the eye, so that none of them passed out again from its interior.

That this is not the case can very easily be demonstrated by a simple experiment suggested by Coccius:—"Having previously dilated the pupil of a cat's eye by a solution of atropine or belladonna, drop some

* Theory of the Ophthalmoscope. By G. Rainy, M.D., Glasgow. The use of the Ophthalmoscope. By J. W. Hulke F.R.C.S., London. The Ophthalmoscope. By A. Zander, translated by R. B. Carter, F.R.C.S., London. Atlas der Ophthalmoscopie, Dr. R. Liebreich, Berlin.

The above works can be procured through Dawson Brothers, Montreal, or through Chewett & Co., Toronto.

water into the eye while the eyelids are held apart, and cover the cornea with a thin plate of glass. The optic nerve entrance and the vessels of the retina can then be distinctly seen slightly magnified."

In this experiment we in reality neutralize the refracting condensing power of the convex surface of the cornea. The water, filling up the space between the cornea and the piece of glass, changes the *convex* to a plane surface. From this it is evident that as the fundus of the eye comes in view, when its refractive power is to a certain extent neutralized, *therefore the blackness of the pupil and the invisibility of the parts behind it depend solely upon the refraction of the light by the ocular media.*

This phenomenon of refraction may be demonstrated with any small camera obscura by simply placing a piece of pasteboard behind the ground glass so as to exclude all light from the camera except what reaches it through the lens; the ground glass being in focus, distinct images of objects in front of the lens are formed on its surface, notwithstanding which, the interior of the *camera* when viewed through the lens appears absolutely black.

In the camera obscura we have an imitation of the eye, its ground glass screen representing the retina, and its lens—the cornea and lens of the eye.

This can also be very simply demonstrated with an ordinary pill box, by making a circular aperture in the centre of the lid about half an inch in diameter, and cementing behind it a convex lens of $1\frac{1}{2}$ inch focus. If the bottom of the box is exactly at the principal focus of the lens, namely $1\frac{1}{2}$ inches behind the (optical centre of the) lens, rays of light from distant objects in front of the aperture will form a focus at the bottom of the box, where an inverted image of those objects is formed. The aperture of this box may be exposed to the brightest sun light, and still its interior will appear perfectly dark, and nothing will be seen behind it except the reflections of light from the surfaces of the lens. We might imagine that the rays of the sun, entering the aperture, would illuminate the whole of its interior, so that the eye of an observer, occupying some position in front, would be able to see some portion of the bottom of the box. We must remember however that the beam of solar light, after passing through the convex lens, is converged to a focus and illuminates a portion of the bottom of the box not larger than the head of a pin.

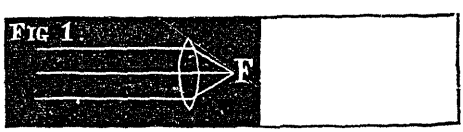
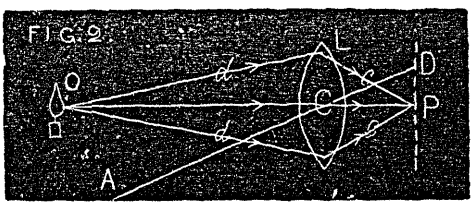


FIG 1.

Thus, in Fig 1, P P represent parallel rays falling upon a convex lens by which they are converged to a focus at F in the axis of a line passing through the optical centre of the lens.

The same thing occurs if the aperture is exposed to the light of a lamp. If the light is brought near the aperture however, the distance must be increased between the lens and the bottom of the box. For example, if the light is six inches from the lens, the focus of the convergent pencil will be formed two inches behind the optical centre of lens; the bottom of the box must therefore be placed this distance behind the optical centre in order to receive the inverted image of the flame. I may here remark that such an apparatus would represent a myopic eye having for its "far point" a distance of six inches. In whatever position the light is held the aperture will still appear dark, and the interior of the box will remain invisible. If the light is placed between the eye and the aperture, so as to place the eye in a line with the light entering the box, the light of the lamp will dazzle the eye, so that nothing can be seen.



In Fig 2, let the lens L represent the 1½ inch convex lens behind the aperture of the pill box, O the flame of the lamp, d d diverging rays from the lamp, c c these rays converged by passing through the lens, and P the point at which the convergent rays form a focus at the bottom of the box. By an inspection of this figure, it will be seen that the eye of an observer looking in the direction of the line A C, will not be able to see the illuminated point P, for the reason that a line passing from A, through C, the optical centre of the lens, would pass to D_2* a point not illuminated; hence the aperture would appear dark. If the eye of the observer should be placed between the light and the box. the light could be obstructed and the box would not be illuminated.

If we could reverse this experiment, and place the flame at the point P, rays of light would diverge from this point P portions of which (CC) would fall upon the lens L, by which they would be refracted, and converged in the direction D D, to the point O, where they would meet in a

* A ray of light passing through the optical centre of a convex lens will continue in the same direction after leaving the second surface of the lens.

focus. In this experiment, P is the object, and O, the image; in the previous experiment, O was the object, and P, the image. *

If, when the flame is at P, diverging rays that fall upon the lens L are so refracted that they meet only at O, these refracted rays can not therefore fall upon the eye of the observer at A, hence the eye of an observer at A can not see the flame at P. The eye of the observer can therefore assume but one position in order to see the flame at P; that position is only in the direction of O C.

In these experiments, we have a very good model of the living eye. When the box is so arranged that the bottom is exactly at the principal focus of the lens, (parallel rays being brought to a focus at that point,) the normal eye is represented; when the bottom of the box is farther from the lens than its principal focus, the myopic eye is represented; when the bottom of the box is between the lens and its principal focus, a hypermetropic eye is represented.

The human eye, in its antero-posterior diameter, is about one inch in diameter. Its refractive media are, the cornea, aqueous humour, crystalline lens, and vitreous humour. Their combination is equal to the refractive power of a double convex lens having a focal distance of a little less than one inch. The optical centre of the eye is supposed to be near the centre of curvature of the cornea. As in the case of a double convex lens, parallel rays are brought to a focus at the pricipal focus of the eye. When the normal eye is in a state of rest, the principal focus is exactly on the basilar layer of the retina.

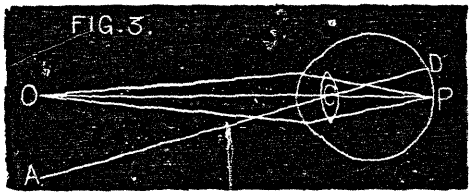
"When a properly formed eye is exactly accommodated for a luminous object, the diverging rays from this incident upon the eye, are refracted by the ocular media in such a manner that they unite at a point on the surface of the retina, which is the image of that object. The retina, in consequence of its transparency, transmits much of this light to the *choroid*, by which most of it is absorbed; but many of these rays are reflected in the same direction in which they entered the eye, and return to the object whence they started. The object, then, and its image on the retina are reciprocal points; they may be considered conjugate foci, each being in turn object or image."†

Thus, let E (fig. 3.) represent an eye accommodated for the object O. In this case the diverging rays from O, falling upon the cornea of the eye E,

* When a person is "sitting" for a photographic picture, the person is the object, and the inverted picture in the camera is the image; but during a magic lantern entertainment, the inverted painted slide in the instrument is the object, and the enlarged view upon the screen is the image.

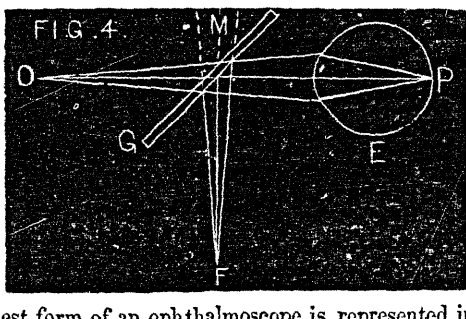
† Hulke.

are refracted by the media of the eye and collected at P, a point in the retina of E. This point, P, in E's retina, is the image of the object O;



and since the rays, when reflected from the eye, undergo a similar refraction but in an opposite direction, the rays from the retina at P will return only to the object O. These reflected returning rays cannot therefore meet the eye of a person at A, but the pupil of E will appear black. And, if the observer's eye be placed in the line OE the illuminating rays will be intercepted. From this it is apparent that without some special contrivance, a person cannot bring his eye into the direction of the rays returning from the eye under examination, without at the same time intercepting the incident rays. *This is effected by substituting reflected for direct light*, the observer placing his eye behind and looking through the mirror into the illuminated eye.

THE OPHTHALMOSCOPE.

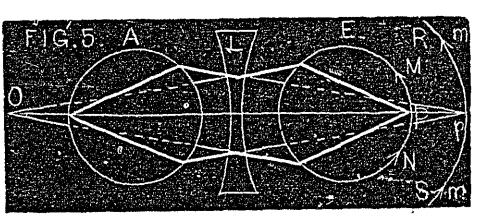


The simplest form of an ophthalmoscope is represented in fig. 4; E is the eye under examination, F the flame of a lamp, and G a piece of plate glass. Rays of light diverging from F, in the direction of M, fall upon the plate glass G; some of these rays pass through the plate glass, but others are reflected from its surface into the eye E, by which if the eye is accommodated for the flame, they are converged to a focus on the retina at P.* The rays that diverge from P, in the direction of the axis of vision, are so refracted by the ocular media that they leave the

* Plate glass has the property of partly transmitting and partly reflecting rays of light that fall upon its surface.

cornea convergingly, and would, if not intercepted, meet in a focus at the point O, which is the same distance behind the plate glass that the flame F is in front of it. These converging rays however fall upon the plate glass, by which they are partly reflected and partly transmitted; those that are reflected are converged to a focus at the point F, and those rays that pass through the plate glass are converged to a focus at the point O. The eye of an observer placed at the point O, or in a line with O C, will be able to see a pinkish reflection from the fundus of the eye E, but as these rays that emanate from the eye are *convergent*, they can not be brought to a focus upon the retina of the observer's eye; hence he is not able, with his unassisted eye, to distinguish any details of the fundus of the eye under examination. To enable the observer to distinguish these details, it is necessary to give the converging rays that are reflected from the patient's eye, a degree of parallelism or slight divergence before falling upon the eye of the observer. This is effected by placing a concave lens of proper strength before the observer's eye. When the examination is conducted in this manner, the vessels of the retina, &c., &c., upon the fundus of the eye under examination, have their natural erect position, and the examination is called the *direct method.*

OPTICS OF THE DIRECT METHOD.

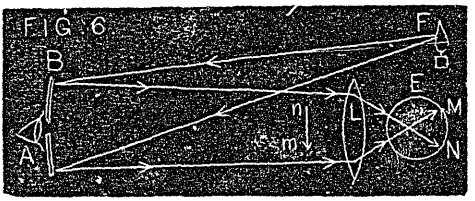


Let E (fig. 5) represent the eye under examination,—the point P, on the retina, being illuminated by the method illustrated in fig. 4; L, a concave lens, and A, the eye of the observer. Both the eye of the patient and the eye of the observer are represented in this figure to be myopic; that is, adjusted for diverging rays. The eye E is adjusted for the point O. Rays that are reflected from P, on the retina of E, would be converged to the point O, but being intercepted by L, these *convergent* rays are rendered *divergent*, which, falling upon the cornea of A, are converged to a focus on A's retina. Thus, the observer will be able to see the point P, on the retina of E. These diverging rays that fall upon A, after being refracted by L, have a direction as if they diverged from a point P, beyond E; the point P will, for this reason, appear to occupy the position of P_1 behind E, and will, consequently, appear larger than natural.

OPTICS OF THE INDIRECT METHOD.

ILLUMINATION.—When the fundus of the eye is examined by the indirect method, it may be illuminated by the light of a lamp reflected from plate glass as above described, or from a concave mirror, in the manner to be described hereafter.

When the light of a lamp is reflected into the eye, a small portion only of the retina becomes illuminated, the diverging rays falling upon the cornea being brought to a point on the retina, as is shown in fig. 3. In making ophthalmoscopic examinations, however, it is necessary to illuminate a larger field, so that a larger portion of the retina can be seen at the same time. This is effected by placing a $1\frac{1}{2}$ or 2 inch double convex lens about two inches from the cornea, between the mirror and the eye under examination.



Thus, in fig. 6, let F represent the flame of the lamp; B, a concave mirror with a central aperture; L, a 2 inch double convex lens, and E, the eye under examination. Diverging rays from F, falling upon B, are reflected convergingly towards L, by which they are refracted to a focus near the pupil of E; these rays, after crossing, proceed in nearly the same direction, and, as they diverge, a large portion of the fundus is illuminated at M N.

REFLECTION.—When the indirect method is employed, the mirror is held at a distance of from 12 to 18 inches from the eye to be examined. The eye of the observer looking throuh the sight-hole of B, will not see the erect image of the fundus of E at M N, but its inverted image will be seen instead in front of the convex lens at *n m*.

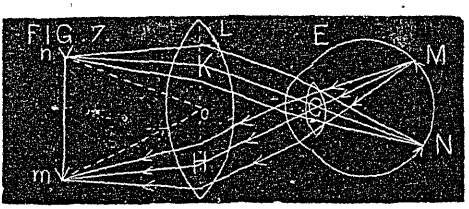


Fig. 7 explains this. Let E represent the eye illuminated as represented in fig. 6, and L a bi-convex lens of 2 inch focus, and at a distance

of two inches from the cornea of E. Rays of light reflected from M,* in the fundus of E, leave the cornea nearly parallel;† these parallel rays, falling upon the lower part of the lens at H, are brought to a focus at *m*, which is exactly at the principal focus of the lens, namely two inches from its optical centre. In the same way rays from the point N leave the cornea parallel and falling upon the upper part of the convex lens at K, are brought to a focus at *n*. There will then be formed in the position *n m*, an inverted image of the fundus of the eye E, which can be seen by the observer (see fig. 6) looking through the sight-hole of the mirror B.‡

LIEBREICH'S SMALL OPHTHALMOSCOPE.

I agree with Hulke in thinking that Liebriech's small ophthalmoscope is the best that can be recommended for ordinary examinations of the internal eye.|| It can be used for both the erect and the inverted image. This ophthalmoscope consists of a metallic concave mirror, two double convex lens 1¼ inches in diameter, and five lenses, (one convex and four concave) ¾ of an inch in diameter. The mirror is 1¼ inches in diameter and has a central aperture or sight-hole one-sixth of an inch in diameter. The principal focus of the mirror is seven inches from its surface. A small spring slip for carrying any of the small ocular lenses, is attached to the back of the mirror by a jointed limb. The large objective lenses are 1¼ inches in diameter and their focal length is 1½ and 2 inches. The small convex (ocular) lens has a focal length of 12 inches and the four concave (ocular) lenses have a negative focus of 6, 8, 10 and 12 inches respectively.§

* Rays of light diverge from M the same as they do from the flame of a lamp; the fundus of the eye does not reflect light as a mirror reflects it.

† When the pupil of the normal eye is dilated with a strong solution of atropine, the eye is adjusted for parallel rays.

‡ It will seen from fig. 7 that a line passing from M through the optical centre C, is continued, after leaving the cornea, in the same direction to H, and that this line M C H determines the direction that the parallel pencil will take after leaving the cornea. It will be seen also that the direction in which H *m* is refracted by the lens L, is determined by a line passing from *c*, the optical centre of L, to the principal focus *m* parallel to M C H. This is true also of N C K and K *n*.

|| In Zander's treatises on the ophthalmoscope, translated from the German by Carter, there are no less than 37 varieties of the ophthalmoscope described; 2 of which are binocular; 5 stationary, and 30 portable. The binocular instruments are those of Giraud Teulon of Paris and J. Zachariah Laurence of London. Carter prefers the ophthalmoscope of Coccius for the inverted image. The ophthalmoscope of Zehender is considered the best for examing the erect image.

§ Liebreich's small ophthalmoscope and the author's demonstrating and photographing ophthalmoscope can be procured of C Potter, optician, No. 20 King Street, Toronto.

THE OPHTHALMOSCOPIC EXAMINATION.

THE INDIRECT METHOD—The evening is the best time for making ophthalmoscopic examinations; if made during the day, the room must be darkened. The best light is that from the oil lamp, or from the argand gas burner. Before the examination, the pupil should be dilated with a solution of belladonna or atropine. For simply dilating the pupil, a solution of $\frac{1}{4}$ of a grain of atropine to the ounce will be strong enough, but in order to paralyze the accommodation of the eye, it is necessary to use a solution of 4 grains to the ounce. If the left eye, for example, is the one to be examined, the patient can either sit with his back against the side of a table and his left shoulder just in front of the lamp, or he can sit with his left side at the end of the table and the lamp placed opposite his left ear. The observer places his chair in front, and to the left of the patient, so that, when seated, he will be *tête à tête* with the patient, and facing the lamp. The mirror is taken in the right hand, and holding it about 6 inches before the patient's left eye, the light of the lamp is reflected upon it. Keeping the light upon the eye, the mirror is gradually withdrawn to a distance of 16 or 18 inches. The mirror, the eye of the patient, and the lamp should be nearly on the same level. The right eye of the observer is now brought to the sight-hole of the mirror, and the instrument kept steady by resting its upper part against the brow. The observer will now see the illuminated fundus of a pink colour and apparently the size of the dilated pupil, but no details can be distinguished. One of the large double convex lenses is now held by the thumb and fore finger of the left hand, between the mirror and the eye under examination, the hand being kept steady by resting the other fingers against the brow; the lens is at first placed about an inch from the eye of the patient, and gradually withdrawn until the pupil appears to be enlarged to the size of the lens, which will be about 2 inches from the eye, if the 2 inch convex ocular is the lens that is used, and about $1\frac{1}{2}$ inches from the eye when the $1\frac{1}{2}$ inch lens is used. If the eye of the patient is at this time looking directly at the mirror, the observer, at first, will, probably, see only the bright reflections of light from the two surfaces of the lens and from the anterior surface of the cornea. The patient should now be directed to fix his eye upon the observer's left eye; this will cause the eye under examination to be turned at the proper angle for the observer to search for the optic nerve entrance, which is always the first point to be discovered in making examinations of the internal living eye.

It cannot be denied that for beginners, this examination is a very difficult one; in fact few are able to see more than the pink reflection from

the fundus of the eye until they have made a number of attempts, under the direction of a practical teacher. The following are the principal difficulties that are met with by the beginner, (1) a small bright reflection of the mirror from each surface of the objective double convex lens; (2) a large bright image of the mirror reflected from the cornea: the circular aperture in this image of the mirror is sometimes mistaken by beginners for the optic nerve entrance 3, and in making this examination by the indirect method, the eye of the beginner is usually accommodated for an object near the back of the eye, instead of being adjusted for the inverted image in front of the objective lens. The character of the reflections from the lens can be studied separately by throwing the light upon the lens when the eye of the patient is closed. The reflection from the cornea can be examined by throwing the light from the mirror upon an eye, the pupil of which is not dilated, and by holding the object lens at different distances from the cornea.

THE DIRECT METHOD.—When the eye is examined by the *direct method*, the mirror is used only, and the eye of the observer should be about 2 inches from the eye of the patient. If the eye under examination is hypermetropic, it can be examined without the addition of an ocular lens behind the mirror. When a hypermetropic eye has its accommodation paralyzed with a 4 grain solution of atropine, it is adjusted for *divergent* rays; these divergent rays, falling upon the eye of the observer are brought to a focus npon the retina withoutt he aid of a concave or convex lens. If both the observer and the patient are hypermetropic, it will be necessary to use a convex lens behind the mirror, so that the diverging rays, emanating from the eye of the patient may be rendered convergent, the hypermetropic eye of the observer will then be able to bring these convergent rays to a focus on its retina. If the eye of the patient and the eye of the observer are neither hypermetropic nor myopic a weak concave ocular lens (*e g.*—12, is placed in the clip behind the mirror to give the parallel or slightly convergent rays that are reflected from the patient's eyes a slight degree of preliminary divergence before falling upon the cornea of the observer's eye.* If the observer is myopic he must either wear the necessary spectacles for correcting his excessive refraction, in addition to the concave ocular lens; or he must place a concave ocular lens in the clip of sufficient strength, not only to neutralize his myopia, but also to render slightly divergent the parallel rays that emanate from the eye under examination. If the patient is also myopic,

* When the normal eye is directed to a distant object, the accommodation is relaxed and the eye is adjusted for parallel rays, but when the eye is directed to a near object it is involuntarily adjusted for divergent rays.

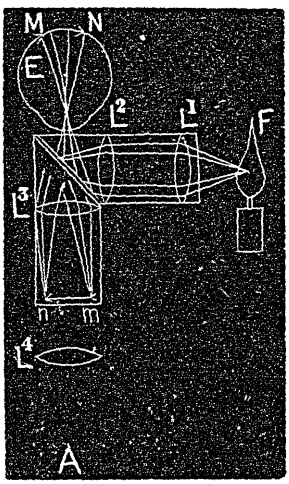
the observer must wear his correcting glasses, and in addition, there must be placed in the clip behind the mirror, a concave lens of sufficient strength to render slightly divergent the convergent rays that emanate from the myopic eye of the patient. When, however, the patient and the observer are both myopic, the examination can be more satisfactorily conducted by the indirect method. When the eye is examined by the direct method, objects at the fundus appear to be considerably enlarged. This arises from the fact that an eye has all the properties of a convex lens, having a focus of about one inch. It is well known that when an object is held just within the focus of a convex lens, the object is apparently enlarged and appears to be placed at a greater distance from the lens than the position it really occupies. So, in looking into the eye by the direct method, the whole of the posterior concave has the appearance of an eye several inches in diameter. Thus in Fig 5, rays emanating from P in the eye E, after being rendered divergent by the lens L, fall upon the eye A, as if they diverged from a point *p*, behind the eye E. By examining the eye from different directions the whole of the fundus can be surveyed, which will appear to occupy the position *m n* behind M N.* The direct method is the only mode of examining the eye for detachment of the retina, opacities of the vitreous humour, crystalline lens, &c., &c. In this latter examination, the eye under examination is viewed at a distance of 10 or 12 inches, and the patient is directed to turn the eye in different directions according to the part that is being inspected.

DEMONSTRATING OPHTHALMOSCOPE.

The theory of the author's demonstrating ophthalmoscope will be seen from the accompanying figure. F is the flame of a lamp; L^1 a double convex lens for rendering parallel the diverging rays of light from F; L^2 a second double convex lens of 2 inch focus for converg-

* Beginners are recommended to endeavour to familiarize themselves with the ophthalmoscope by practicing upon an artificial eye constructed of an ordinary pill box as described in a former part of this lecture; the ocular lens of a microscopo or telescope, or one of the objective lenses of Liebreich's ophthalmoscope can be cemented behind the aperture of the lid; a rude imitation of the optic nerve entrance and the branching of the retinal vessels can be made with pen and ink upon the bottom of the box; the normal, hypermetropic, and myopic eye can be imitated by fixing the bottom of the box at different distances behind the lens as already described. The eye of the cat is more easily examined by the ophthalmoscope than the human eye, as the *tapetum* reflects the light much better than the choroid of the human eye. The rich colouring of the tassetum makes the fundus of the cat's eye, a very interesting object for examination with the ophthalmoscope.

ing these parallel rays of light to a focus two inches behind the lens; G is plane plate glass for reflecting these converging rays in the direction of the eye E. These converging rays are represented to cross near the pupil of E and to diverge to illuminate the fundus M N. Rays that are reflected from the fundus of E leave the cornea nearly parallel,



and falling upon the plate glass G, are partly reflected and partly transmitted; those rays that are transmitted pass to L^3, a 2 inch double convex lens, by which they are brought to a focus at *n m*, where an inverted image of the fundus M N is formed. This inverted image can be seen by the eye of an observer in the position of A, and can be very considerably enlarged by placing a magnifier of 4, 3, or 2 inch focus (L4) at a proper distance behind *n m* and between it and the eye of the observer. By placing a screen of ground glass in the position of *n m*, the inverted image of the fundus of E will be formed upon it.

With a modification of the above ophthalmoscope the author has demonstrated that photographs can be taken showing the details of the fundus of the living eye.*

Lateral Illumination.

The examination of the cornea, aqueons humour, iris, and crystalline lens, is not complete unless they are also examined by what is called *lateral illumination* (called also "oblique illumination" "focal light" &c). This examination must be conducted at night, or in a darkened room. The lamp must be placed at the side and a little to the front of the patient and nearly on a level with his eyes. The observer sits or stands in front of the patient and with a $1\frac{1}{2}$ or 2 inch double convex lens he directs a cone of light laterally across the anterior chamber of the eye under examination. The parts thus illuminated, can be examined by a magnifier of 2 or 3 inch focus.

"This mode of illumination is especially valuable for minute scrutiny of the iris and papilliary margin; for inspection of the ciliary processes which may be singly followed to their origin; but, above all, for the

* A description of the above instrument appeared in "The Canadian Journal for March, 1864, and also in "The Ophthalmic Review" for July, 1864.

examination of wounds and exudations of the capsule, and for determining the consistence of the cortex, or the size, colour, and position of the nucleus of the lens. In examining for cataract and parts behind the iris, the pupil must be completely dilated with atropine."†

Interpretation of Phenomena Observed.

The following paragraphs on the interpretation of the Phenomena observed with the ophthalmoscope, I have copied entire from "The Theory of the Ophthalmoscope," by G Rainy, M.D., Glasgow.

"In making ophthalmoscopic examinations, the observer is often apt to be misled by optical illusions in drawing conclusions with respect to the magnitude, colour, position, and stability of the objects which he sees; and to err in his diagnosis in consequence of preconceived opinions with regard to the appearances which are consistent with a normal state of the eye, and in consequence of erroneous impressions about the causes which combine to produce varieties in the appearances. A knowledge of the varieties which may be met with in different eyes, and of their rationale, is of such practical importance and so intimately connected with our subject, that I may be allowed to make a few remarks upon the more important phenomena to be met with in health and disease, and upon some of the illusions by which the observer is most likely to be misled.

"The fundus of a normal eye, when examined ophthalmoscopically, presents a pretty uniform orange or pinkish colour; and, when the observer's eye is accurately adjusted for it, a finely granulated appearance, except at the place where the optic nerve enters, which will occupy the centre of the observer's field of vision when his axis of vision is directed somewhat downwards and inwards at an angle of 20° to 25° to that of the patient. The general hue of this spot is yellowish or greyish white, but it is not quite uniform in shade; and its form is nearly circular, but it is occasionally somewhat longer in its vertical than in its horizontal diameter, and irregular in its contour even in sound eyes.

"The retinal vessels are seen to rise out of the entrance of the optic nerve, near its centre; and they can be traced from this over a great part of the fundus oculi. Before leaving the surface of the optic disk they generally divide into two principal arterial and two principal venous trunks; an artery and vein going upwards and outwards, and another pair downwards and outwards, and arching round the site of the macula lutea, which is free from vessels. The venous trunk grnerally divides into its two principal branches in the substance of the nerve, near the surface;

† Zander on the ophthalmoscope.

the artery often remains single for some little way after it has come to the surface; but there are considerable varieties in the distribution of these vessels. The arteries may be distinguished from the veins by their smaller calibre, lighter colour, and more strongly marked double contour, which arises from the greater reflecting power of their coats.

"By slightly altering the focal adjustment of his eye, the observer may often see the observed fundus oculi streaked something like the skin of a tiger. This arises from the perception of the larger choroidal vessels, and their interspaces through the superficial layer of pigment cells; and the appearance is most easily seen towards the circumferential parts of the fundus oculi, and in the eyes of persons of fair complexion.

"When we proceed to investigate the rationale of these appearances, we are at once struck with the difference between the dark brown colour which is presented by the fundus of a dissected eye seen by ordinary day-light, and the clear orange tint which it assumes when illuminated by the ophthalmoscope. This is, no doubt, to be explained partly by the intensity of the light concentrated upon it, partly by the absence of contrast with other objects equally well illuminated, and partly by the presence of red blood circulating in it. If we trace the course of the light sent from the ophthalmoscope after it has passed through the refracting media, it must be observed that it first meets with the retina, which, being smooth and transparent, reflects but little light either regularly or irregularly when the light falls nearly perpendicularly on it ssurface, as it must of necessity do. It next reaches the layer of hexagonal pigment cells covering the choroid internally, when a considerable proportion is absorbed, some is transmitted, and a good deal seems to be reflected.*

"The transmitted light arrives at the choroidal vessels in the next place, and some is reflected, some transmitted, and some absorbed; the pigment in the interstices between them will, if strongly developed, absorb most of the light which falls upon it; but some may be transmitted through both it and the vessels to the sclerotic which has a great reflecting power. The light returning from these more deeply seated parts suffers loss from absorption in passing again through the more superficial ones; and it is also dispersed by them in such a way that it

* E. Jager regards this layer of hexagonal cells as the principal reflector in the normal state of the fundus oculi. This opinion seems to be mainly grounded on the fact that, in certain cases where the superficial layer has been partially destroyed the fundus oculi presents a lighter colour than it does in parts denuded of this layer; and he states that its pigment appears reddish or orange by strongly concentrated light. Others, however, explain the matter differently.

Ergebnisse der Untersuchung des Menschlichen Augus: Wien, 1855.

rather tends to affect the colour of the latter, and the apparent brilliancy of the image which we see, than to give us a definite perception of the form and colour of the objects from which it is reflected. The less strongly developed the superficial pigment is, the less absorption and dispersion will take place.

"In various diseased conditions, the layer of the hexagonal cells becomes destroyed or atrophied, and in this case we may see the choroidal vessels very well, with their interstices filled with dark pigment; but this may sometimes be observed in the case of persons with well-developed interstitial pigment and little development of pigment in the superficial layer; such cases, however, are rare. Again, both superficial and deep-seated pigment may be destroyed by disease, and then we see the choroidal vessels against the white grouud of the sclerotic; but something similar may be seen in the eye of an albino, or a person of very fair complexion. Finally, the choroid may be atrophied in its whole thickness at particular parts, as in staphyloma posterius, and we then see nothing but the white sclerotic; but this may be simulated by white patches, the result of inflammatory exudation, fatty deposits in the retina, &c.

"In all such cases, we must pay particular regard to the complexion of the patient, and the history of his case. The seat of the abnormal appearance may often be detected by observing whether the retinal vessels pass in front of or behind the white patches observed; and we may generally distinguish the results of atrophy, or destruction of pigment, from those of original conformation, by the irregularity of the distribution of pigment in the former case, and from those due to exudations, &c., by their not being so sharply defined in general.

"Masses of dark pigmentary matter are sometimes observed on the fundus oculi. They may or may not be of pathological importance; and a sickle-shaped deposition of this kind is common at the margin of the papilla optica.

"The transparency of the retina may be impaired, and its reflecting power increased in consequence of inflammatory disease. The effect of this is to give a hazy indistinct appearance to the fundus oculi. The observer must be on his guard against confounding this with an indistinctness due to improper focal adjustment of his own eye, or to turbidity of the media, or with the faint greyish colour which the fundus presents occasionally in very dark subjects—a phenomenon which appears to me to be explicable upon principles referred to in Part I. When the retina is separated from the choroid by serous exudation, some parts may appear bluish grey, or white, and others almost black, according as the light regularly reflected from their surfaces enters the eye of the observer

or not. A good deal also depends upon the colour of the subjacent fluid, and upon contrast with parts of the fundus which present the usual appearance.

"The papilla has a red appearance in various diseases, owing to increased vascularity of its surface; but its colour varies considerably in the normal state, often resembling grey cerebral substance, and it is sometimes pinkish. Some parts of it appear to reflect more light than others—a fact which is attributable to our being often able to see back to the lamina cribrosa. The more brilliant parts correspond to those in which fibrous tissue predominates, and the darker ones to the transparent substance of the nerve-tubes along the axes of which we look.

"The papilla sometimes appears abnormally large and vascular, viz., in cases of staphyloma posterius at a certain stage where the sclerotic appears around the optic disk in consequence of atrophy of the choroid; and small retinal vessels, which cannot be distinguished on the orange ground in a normal eye, come into view. Eyes affected with this disease are more or less myopic; and the optic disk proper appears hardly so white as the surrounding sclerotic denuded of the choroid.

"The choroid hardly comes to the margin of the place of entrance of the optic nerve, even in a normal eye, and on this account the nerve substance may be much atrophied, according to Liebreich, without an apparent diminution in the diameter of the disk.

"The papilla optica, which does not really present the form of a globular elevation during life, sometimes appears as if it did so when it is observed by means of the ophthalmoscope. One cause of this has been pointed out by Liebreich, who has drawn attention to the fact that the distribuition of dark and light coloured parts resembles that in a representation of a sphere well executed on a plane surface.* When the optic nerve is cupped or excavated, as in glaucoma, this appearance comes out more strikingly, especially when the indirect method of examination is employed, because we are then subject to another optical illusion depending upon the reversal of the image, and similar to that in consequence of which the inverted image of an intaglio produced by a biconvex lens, resembles a cameo; the shadow thrown by the margin of the cup or excavation appearing to be on the side opposite to that from which the light comes to it instead of on the same side.† This illusion is, for obvious reasons, most striking when the image of the disk is near to one side or other of the observer's field of vision.

* Grafe's Archiv, Bd. i., Abt. ii.

† See "A Fragment on Glaucoma," by Dr. Mackenzie, in No. xi. of the London Ophthalmic Hospital Reports.

"In cases where the optic nerve is deeply excavated, the bottom of the cup appears to be of a greenish colour, while its margin (corresponding to the part of the sclerotic immediately surrounding the nerve, and not covered by choroid) has a yellowish white colour, and a brilliant aspect at certain parts from its reflecting the light like the rim of a cup. The retinal vessels may be seen passing over this to reappear indistinctly at the bottom of the cup, and they may seem more or less dislocated as they do so, according to the position of the patient's eye with respect to the observer, and according to the position in which the biconvex lens is held, if one is made use of."*

"When we perceive opaque bodies situated in the media, by means of light coming from the fundus oculi, they will appear black, whatever their real colour may be; thus, opacities in the lens, though they may appear grey, or even white, under ordinary circumstances and contrasted with a black pupil, appear like black spots or streaks upon the illuminated fundus. We may, no doubt, see light reflected from the fundus oculi, and light reflected from opaque bodies in the media at the same time, and contrast will then determine in a great measure the appearance presented by the latter. Supposing an opacity in the lens to have a power of reflecting light falling almost perpendicularly upon it equal to that of the fundus oculi, the former would appear dark in comparison with the latter, if the illuminated area of the fundus were smaller than the area of the pupil; because the fundus would in that case be better illuminated than the opacity, and the observer would in general see each with nearly its proper brilliancy, provided the pupil of the observed eye were somewhat larger than his own."

"In all cases in which the observer seeks to draw conclusions from the colour of objects seen within the eye, he must remember to make allowance for the quality of the light by which they are illuminated, and which reaches his own eye after undergoing various modifications consequent upon its reflection from, and transmission through other bodies, besides those which it renders distinctly visible."

"It will be found that objects seen by means of the ophthalmoscope generally present a lighter shade of colour than they do when seen by ordinary daylight, especially if the direct method is employed."

"The principles upon which we may determine the state of focal adjustment of the observed eye have been already discussed. The importance of the ophthalmoscope used for this purpose in military and medico-

* The reader will find observations on this subject by Mr. Streatfield. Ophthalmic Hospital Reports, No. xi.

legal practice, has been pointed out by Professor E. Jager of Vienna.* He refers to a circumstance, which may rather embarrass the observer in examining hyperpresbyopic eyes in the direct manner, viz., that in extreme cases the apparent magnitude of the image of an object, such as the papilla, may not exceed a third or a fifth part of what it appears in a normal eye. A much larger area is then seen, and this circumstance may add to the difficulty of illuminating hyperpresbyopic † eyes, referred to in Part II. The illiumination may or may not be fainter than it is in a normal eye, according to the mode in which it is accomplished, and according to the cause of the hyperpresbyopia,‡ whether dependent on absence of the lens, &c., or on shortening of the axis of the eye; but the area illuminated will always appear smaller than of equal dimensions in a normal eye, and it will seldom occupy the whole of the observer's field of vision."

"Apparent motions of objects situated in different planes within the eye, are often very difficult to distinguish from real ones. They may depend on motions of the eye observed, or on motions of the observer, or the biconvex lens held in his hand. The apparent changes in the position of objects caused in this way may be regarded as an exaggeration of those observed in looking at objects from different points of view under ordinary circumstances; except that in the direct mode of examination, it is the most distant objects which appear to move most rapidly, instead of the nearer ones, as is always observed in nature, and generally in the inverted image."

REVIEWS AND NOTICES OF BOOKS.

Contributions to Bone and Nerve Surgery. By J. C. NOTT, M.D., Professor of Surgery in Mobile Medical College. Philadelphia: J. B. Lippincott & Co. 1866.

As was to have been expected, the late unhappy struggle has, from the number of the casualties that occurred, afforded a wide field for the improvement of Surgical and Medical Science. but more especially the former. Now that it has terminated, the laborers are putting together the facts, and drawing such deductions as seem to them to flow from them. Among the few that have already appeared, this unpretending little volume of about one hundred pages is certainly not the least valuable. Opening

† Der Augenspiegel als Optometer, österr. Zeitschrift für practische Heilkunde, March, 1856. * Hypermetropic. ‡ Hypermetropia.

with a brief summary of the views advanced on periostitis, endostitis, ostitis, caries, necrosis, &c., he passes in the second part to give such views on bone and nerve surgery, as cases that came under his notice, while an active participant in the war, seem to warrant. Before noticing one or two things in the second part of the work, we would allude in favourable terms to a method of operating to relieve the great pain of acute periostitis when you have reason to believe suppuration has not occurred, which is, we believe, alone recommended by Dr. Nott. Gross and other surgical writers recommend, whether fluctuation can be detected or not, when great pain exists, to cut down through the skin, and relieve the pent up matter. Dr. Nott says: "In periostitis, accompanied by severe pain and effusion of lymph and serum, with a tendency to suppuration, it is proper to make a small opening, at the lower part of the swelling, introduce a probe-pointed knife flatwise, pass it along between the skin and periosteum to the upper border of the swelling then turning the cutting edge towards the periosteum, and while withdrawing the knife, divide the periosteum subcutaneously down to the bone, through the whole extent of the swelling. When we are sure suppuration has occurred, it is better to make a free incision through the skin." In the second part of the work, alluding to gun-shot injuries, he says: "My general rule has been thus; if a patient comes to me two or more months after a gun-shot injury, with a fistulous opening, unless contraindicated by an inflammatory condition of the part or an unhealthy condition of the system. I cut down boldly in the direction of the sinus, laying it opening to the bone, and then introduce my finger and explore it fully so as to ascertain, certainly what is the condition of the bone, and whether I am justified in removing the dead portion * * * * * * In the majority of cases I find balls, clothing or other foreign bodies, and the patient is relieved at once by the removal." Dr. Nott, in alluding to what sometimes occurs in flap operations, but more particularly in circular operations according to Gross—viz., exposure of the bone, either from sloughing or from sufficient covering not having been left, says:

"In those cases where the bone projects from the stump, the inexperienced surgeon is often tempted to interfere, not only unnecessarily, but injuriously with the operations of nature. I have been often consulted to know whether it is not best to saw off the projecting bone; but though a contrary opinion has recently been advanced by an army surgeon, I unhesitatingly say *no*. Where a bone projects from an open, suppurating stump, under any circumstances, it will require at least a month, and probably six or eight weeks, before the stump can be healed, whether the bone is on or off. Experience teaches that nature will amputate the bone, and generally at a very good point, by the time the rest of the wound

closes up to its margin, and thus do away with the necessity of amputation by the surgeon: the absorbents set to work, at the line between the dead and living bone, and in from four to eight weeks, the work is complete, and the skin soon closes over it."

"If while a stump is swollen, inflamed, and suppurating freely, the surgeon cuts down to remove a projecting end of bone, he adds to the inflammation, causes more or less hemorrhage, shocks the system, makes the condition of the stump altogether worse, and *gains no time* in the final result, even if the case does well. One of the greatest objections to the operation is the danger of extending the necrosis, by exposing a fresh surface of bone to unhealthy pus of the stump. This must necessarily be the most common result; the projecting bone to be removed dies because it is exposed to air and pus, and the surface exposed by the second operation is likely to be followed by similar results from the same causes."

Contusion of bone is, says Dr. Nott, very often followed by serious results. Many cases have come under his observation where the ball has entered, grazed the bone, and passed out, the wound healing rapidly. But in a few months, sometimes weeks—periostitis showed itself, which, going from bad to worse, ended in necrosis and exfoliation of a portion of bone over the sight of the contusion. The climate of the Southern States is according to our author peculiarly favourable to recovery from wounds. He says: "I feel assured that no experience of the old world can compare in success with that of the surgeons of the Southern States, not from any peculiar skill on their part, but from the superiority of the climate." Altogether we consider Dr. Nott's little work a valuable addition to our surgical literature. As here in Canada, we cannot tell the moment that many of us might be called to the field, it becomes our bounden duty to be thoroughly posted in that class of cases—that would certainly come under our observation, and on them Dr. Nott has thrown much light.

Descriptive Catalogue of Fluid and Solid Extracts in Vacuo, also Concentrated and officinal Pills, prepared by HENRY THAYER & Co. Cambridgeport, Mass., 1866.

We have received from Messrs. Henry Thayer & Co., a handsome book of over two hundred pages, being a descriptive catalogue of the fluid extracts and other preparations made by this well-known firm. Within a few years many practitioners have discarded many old infusions, and used in their place fluid extracts of the same article. Some profess to have met with disappointment from their use—complaining that the ex-

tracts are not of a uniform strength. So long as the required strength can be guaranteed by a respectable firm, for ourselves we would prefer to use the fluid extract in many cases. Being concentrated, a smaller dose is required, a desideratum not to be overlooked. In their preface they say, "Except when prepared from officinal formulae, we manufacture a plain extract of the plant, believing that physicians will prefer to form combinations according to their own judgment. In strength each fluid pound of extract contains the strength of a pound of raw material." So far as we are aware, the extracts prepared by Messrs. Thayer & Co. are not much used in Montreal, but we have no doubt that if they prepare them of a uniform strength, and advertise them, they may obtain a share of medical patronage.

Successful Removnl of the Uterus and both Ovaries by Abdominal Section; the tumour fibro-cystic and weighing thirty-seven pounds. By HORATIO R. SRORRER, M.D., Boston.

We have had this small pamphlet in our possession for some time. It gives the details of a case of great interest, and its success will cause many to consider whether the assertion of the unjustifiableness of the operation is correct. Extirpation of the uterus has been principally performed by American surgeons, and including Dr. Storrer's there are now six successful cases on record against eighteen fatal ones. The paper was originally published in the Boston Medical and Surgical Journal.

PERISCOPIC DEPARTMENT.

Medicine.

HOW SHALL WE TREAT CHOLERA?

Notes on the Pathology and Treatment of Cholera. By GEORGE JOHNSON, M.D., F.R.C.P., Physician to King's College Hospital; Professor of Medicine in King's College; etc.

The question which I have placed at the head of this communication, is one which, at the present time, is of the highest interest and importance; and it is one to which probably many of us will soon be called upon to give a practical answer. Dr. Handfield Jones has discussed the subject at some length in the last number of the JOURNAL; and he invites others to give the results of their experience, particularly with reference to these two questions—Is it right to suppress choleraic

diarrhœa at once and decidedly by any means in our power? or is it a better and a more successful practice to encourage the evacuations by eliminants? The whole tendency of Dr. Jones' remarks is in the direction of an affirmative answer to the first of these questions, and of course of a negative to the second. Now, the ultimate appeal for the decision of all questions of this kind must be to facts and to experience; but the difficulty of obtaining trustworthy facts and the results of unbiassed experience is much greater than is commonly imagined. The facts are often seen through the distorting medium of a theory, and the judgment is in consequence perverted. To illustrate this by an example: two practitioners (MM. Briquet and Mignot, *Traité Critique et Analytique du Cholera Morbus*, 1850, p. 514) who believe that the worst symptoms of cholera are the result of the drain of fluid from the blood, treated 200 cases of diarrhœa in the hospital, under the most favourable circumstances, by repeated large doses of laudanum, and twenty-six of the patients so treated passed into the stage of collapse. Now, while it is assumed that the arrest of the disease in the 174 cases was a salutary result of the treatment, it is also assumed that the transition from the stage of choleraic diarrhœa to collapse in the other cases was a consequence of the laudanum having failed to arrest the gastro-intestinal discharges. But a totally different and possibly a more correct interpretation may be given of these phenomena. It is at least conceivable, though it is obviously incapable of proof, that, if these 200 patients had been kept in bed and had taken only copious draughts of cold water or any other simple diluent, not one would have passed into collapse, and all might have recovered more speedily than they did while taking large doses of laudanum. It is an indisputable fact, that a large portion of cases of choleraic diarrhœa will terminate in recovery under the use of the simplest possible remedies which are wholly free from astringent properties. Thus the late Mr. Wakefield, who was surgeon to the Middlesex House of Correction during the last epidemic of cholera in 1854, stated in a letter to the *Times*, that he had treated upwards of 150 cases of choleraic diarrhœa amongst the prisoners by thirty grains of sesquicarbonate of soda in a wineglassful of strong mint tea. The dose was repeated every half hour. No fatal case occurred. "The disease was arrested with a rapidity that was quite magical," and he had rarely occasion to administer the dose more than three times before the sickness and diarrhœa were arrested. While under treatment, the patient was confined to a diet of beef-tea, cocoa, or arrow-root; nothing solid, not even bread, being allowed while the diarrhœa continued. Now, this plan of treatment, which was remarkably well suited for allowing full play to the curative efforts of nature, can scarcely

be supposed to have had any direct remedial effect, and it was certainly not an astringent treatment; yet the results were in the highest degree satisfactory, and form a striking contrast with the results of the opiate treatment in MM. Briquet and Mignot's cases. I am aware that too much reliance must not be placed on a comparison of the results of treatment in cases occurring at different times and in different countries; but there are many facts and considerations which point to the same conclusion as the comparison which we have just now instituted.

That treatment of choleraic diarrhœa is obviously the best, which most speedily and completely puts a stop to the purging without subsequent ill effects. In the treatment of this disease, we must never lose sight of an important principle which, with reference to another class of diseases, has been forcibly impressed upon us by one of the great living masters of our profession. Dr. Latham says: "There is a lesson which we are apt to learn slowly, but which all of us come to learn at last. It is this: that while present pain and present peril call loudest for relief and rescue, still in relieving and rescuing the ultimate well-being of the patient must not be disregarded altogether. To compare great things with small, it is not only in the art of war, that an imprudent victory has been the beginning of many disasters." (*Lectures on Diseases of the Heart*, vol. i, p. 236.)

For several years, I was in the habit of treating summer diarrhœa, and during the epidemic of 1849 choleraic diarrhoea, by opium and astringents; and it was the frequent failure of this method that led me to search for some better theory of cholera than that which suggested this unsuccessful practice.

My observation of the effect of opium in cases of choleraic diarrhœa is briefly this. When the diarrhœa is abruptly checked by the opium, the patient sometimes passes at once into a state of collapse; while, in other cases, the diarrhœa, having been arrested for a time, returns as soon as the effect of the opiate has passed off. In other cases, again, the diarrhœa continues, in spite of repeated doses of opium, for a period sometimes of several days; the patient meanwhile having a hot skin, a quick pulse, a coated tongue, headache, and other febrile symptoms.

On the other hand, I found, during the last epidemic of cholera, that when choleraic diarrhœa was treated by emetics, mild laxatives (castor oil), and cold water, the disease subsided much more speedily, and not one case, out of a large number so treated, passed into the stage of collapse. They all quickly recovered; although I am now convinced that in several instances much more castor oil was given than was necessary or desirable.

This is a true account of the results of the two opposite modes of treating choleraic diarrhœa, as I have observed them. And, moreover I maintain that these results are in strict accordance with the most rational theory of cholera.

What is the most probable explanation of choleraic diarrhœa? It seems likely that a poison enters the blood, either through the lungs or through the gastro-intestinal canal; and that this poison excites certain zymotic changes in the blood, in consequence of which some blood-constituents undergo morbid alterations which render them not only useless but noxious. These morbidly changed blood-constituents are then excreted by the mucous membrane of the stomach and intestines, and are ultimately ejected by vomiting and purging. It appears then that there are three distinct and well defined stages: 1, zymotic blood-changes, consequent on the imbibition of a poison or ferment; 2, vascular excretion of morbidly altered blood-constituents; and 3, intestinal elimination of the morbid excreta. Now, if this be the true explanation of choleraic diarrhœa, what is the probable effect of opium upon each stage of the disease? Is there the slightest reason to suppose that opium has the power to prevent or to check the zymotic changes in the blood; I know of none; and I believe that there is no evidence that opium has any such influence. But, from the known power of opium to check excretion, we should infer that it would probably prevent or retard the escape of the altered blood-constituents by the mucous membrane of the stomach and bowels; and then, if it be true, as we believe, that the immediate cause of choleraic collapse is an arrest of the circulation through the lungs, induced by morbidly altered blood, we can understand that the astringent action of opium upon the gastro-intestinal mucous membrane may be quickly followed by collapse. And here again facts and theory agree in a remarkable manner.

Yet, once more, what is the effect of opium when a quantity of morbid secretion has been poured into the intestinal canal? Obviously it must retard the escape of these offensive matters, and so prolong the disease. What says Sydenham upon this point? "By checking the disease at its onset, I should wear out the patient by an intestine war, and I should just confine the enemy to his seat in the bowels, should impede the natural evacuations, and detain those humours which were seeking for an outlet." And, again, with reference to the use of astringents, he says: "They keep the enemy to his quarters, and they change him from a visitor to a denizen. Besides this, the disease is prolonged, and there is danger in the delay. Vicious humours creep into the blood; ill conditioned fever is excited; and the patient suffers not only a severe

disease, but a long one too." (*Sydenham's Works*, translated by Dr. Latham. Sydenham Society. Vol. i, pp. 163-4.) This statement is quite in accordance with my own experience, that the effect of astringents, and especially of opiates, in the early stage of epidemic diarrhœa, is to prolong the disease, and in the same degree to increase the risk of disaster.

The only period of the disease at which opium can be given with safety and advantage, is after the elimination of the intestinal excreta. It then allays pain and irritation, and often affords great relief and comfort to the patient. Whenever a dose of opium appears speedily to arrest an attack of choleraic diarrhœa, it is because the process of intestinal elimination had been well nigh completed before the dose was given; whereas, if given earlier, when the bowel contains an abundance of morbid secretions, the opium retards their exit, and so prolongs the disease. Whether an opiate does harm or good, then, will depend upon the period of the disease at which the dose is given. The rule is, not to close the door until "the enemy" has been expelled.

The cholera poison, whatever may be its nature and source, whether it be "ponderable" or not, is a reality, and no figment of the imagination. And it is as drastic a purgative as any drug in the list of the materia medica. This violent purgative action of the poison is a result, probably, of morbid zymotic changes which it induces in some of the blood-constituents; and, since it is likely that the longer it remains in the system the greater will be the mischief which it occasions, it seems obvious that nothing should be done to prevent the speedy exit of so active and so destructive an agent. Those who, under the influence of an erroneous theory as to the essential cause and nature of collapse, would not dare to give a purgative in cholera, are apparently blind to the fact that the tendency of their opiate treatment is to retain within the system an agent rapidly self-multiplying, and more violently purgative than any ordinary drug. Henceforth, let them bear in mind that so long as the morbid poison remains in the system, there is going on a rapid manufacture of *cholera-cathartine*, which must and will purge itself away.

Fortunately, in most instances, the purgative action of the poison is sufficient to overcome the astringency of the opium, just as a large dose of elaterium, in combination with opium, would purge itself away. And so choleraic diarrhœa, treated from the first by opium, in the great majority of cases, comes to an end, only somewhat less speedily than it would have done if left alone.

Dr. Handfield Jones asks whether the cholera poison is a thing conceivably capable of being eliminated. And he says that "the malarious

poison, which much resembles choleraic, is not got rid of by any purging." Now, as to the cholera poison, there is a something thrown out of the body with the morbid excreta which is very generally believed to be poisonous; and it is considered highly important to disinfect, destroy, and put out of the way these poisonous excreta as speedily as possible, in order to prevent the spread of the disease. And here let me ask, is it possible that these cholera stools can be at once innocuous to the patient while they are retained with his body, and yet a source of danger to others when they are discharged from his bowels? If the cholera excreta are so poisonous that the discharges from a single patient, if allowed to pass into the sewers without previous admixture with disinfectants, may, as is supposed, infect a whole district (See Dr. Wm. Budd's papers in Association Journal, 1854), is it conceivable that they can be artificially retained within the intestines without detriment to the patient? Is there not an inconsistency here between the attempt to restrain the evacuations by opiates and astringents and the practice of disinfecting the stools? If the excreta are poisonous, they surely ought to be allowed—nay, assisted—to escape as speedily as possible from the patient's bowels. If they are not poisonous, why take the trouble to disinfect them before they pass into the drains?*

Then, as to the malarious poison, I would ask Dr. Jones what evidence he has that it resembles the cholera poison. I am not aware that we have any knowledge of morbid poisons, except that which we gather from observing their effects upon the functions and structure of the body; and, as the history and symptoms of cholera are certainly very different from those of ague, I should infer that their specific causes are essentially different. Dr. Jones surely will not maintain that the fact of the malarious poison being irremoveable by purging is evidence in favour of the astringent or against the eliminative treatment of cholera. What would Dr. Jones say to this argument? The malarious poison and the cholera poison much resemble each other. Quinine will not cure cholera: probably, therefore, it is not a cure for ague!

Again, Dr. Jones says that "the analogy of most other diseases of a

* Surely the advocates for the opiate and astringent treatment of choleraic diarrhœa, in opposition to the eliminative method, must be the legitimate therapeutical descendants of those amongst the cotemporaries of Sydenham, who, on theoretical grounds, so violently opposed that alarming innovation—the admission of cold and fresh air into the chambers of small-pox patients. They thought it essential for the safety of these unhappy patients to close the doors and windows, and thus to surround them with a poisoned atmosphere. Sydenham ventured to admit fresh air, and so to *eliminate* the poison.

toxic kind would lead us to think that the morbific action is to be counteracted rather by endeavours to prevent its morbid effects, or to strengthen and hold up the vital forces against it, than by any process of elimination." Now, in reply to this statement, I would remark that, in most instances, we have no power to prevent the effects of morbid poisons. How powerless, for instances, we are to arrest the morbid phenomena of small-pox or typhus fever! And, again, in those rare instances in which we have the power to prevent certain effects of morbid poisons, we find that repressive measures are injurious. Take, for instance, the eruption on the skin in the case of scarlatina. By continued exposure of the surface to cold, we can drive in the eruption, and relieve the sensation of heat and tingling; but a very common result would be inflammation of the kidneys and dropsy. Surely the true aim of scientific medicine is to study the phenomena of each disease, with a view to ascertain their nature and origin, and especially to determine the natural process of cure; to be very careful in arguing by analogy from one disease to another; and in all cases to avoid such measures as would tend to repress a natural effort at cure. This principle, I conceive, would equally forbid the use of cold lotions to the skin in scarlatina, and opiates in choleraic diarrhœa. Would Dr. Jones consider it good practice to check the profuse acid perspirations of rheumatic fever by cold, or by other direct repressive means?

So unwilling is Dr. Jones to admit the principle of elimination, that he endeavours to account for the good effects of purgatives in some cases by supposing that they substitute "another kind of action, of less pernicious character, for the original;" and he gives what I cannot but consider a most incomprehensive and improbable theory of choleraic collapse, to show that castor-oil may be beneficial as a "modifier of an irritated state of the intestinal surface." In confirmation of this view, he states that he lately observed an instance "in which a dose, taken on account of some abdominal uneasiness, removed the symptoms speedily, without acting at all on the bowels." He means, of course, without acting as an aperient. But I venture to suggest that the oil in this case may have relieved the patient by driving some irritating material from the more highly sensitive small intestines into that more torpid receptacle, the large bowel. When castor-oil expels offensive and irritating secretions from the bowel, it seems more reasonable to attribute the relief to that kind of elimination, than to any mysterious alterative influence of the oil upon the surface of the intestine. There are some pathologists who, assenting to the doctrine that the natural cure of cholera is effected by the elimination of the morbid excreta, yet maintain that the poison itself is sufficiently emetic and purgative, and that no attempt should be made to increase the discharges by artifi-

cial means. Now I admit that, so far as regards the excretion from the blood-vessels into the stomach and bowels, no artificial aid is needed. So long as the circulation remains free, this process will be sufficiently active; and when the circulation fails in consequence of the arrest in the lungs, as it does in cases of extreme collapse, no purgative will keep up the excretory process. But the process of elimination may be arrested in what I have before referred to as the third stage, that of intestinal elimination. The morbid secretions may accumulate in the gastro-intestinal canal, or they may be but slowly and incompletely discharged; and then artificial aid may be of real value. Absorption is very active during the stage of reaction after collapse; and then poisonous excreta which have been retained within the bowel may re-enter the circulation and excite mischief. By copious draughts of cold water or other simple diluent drinks, by an occasional emetic, and by some mild but quickly acting purgative, the stomach and bowels may be freed from their poisonous excreta—the gastro-intestinal sewer may be flushed—and this with great advantage to the patient. Castor-oil, as a purgative in these cases, has the advantage of being very quick in its action, and yet it is unirritating, and has very little tendency to increase the drain of liquid from the blood.

It is to be hoped that a more rational pathology of collapse will henceforth lessen the unfounded dread of excessive purging. Death *may* be a result of excessive purging in cholera; but it is a comparatively rare result; and the probable reason in this. When the poison is so abundant or so virulent that its excretion would involve a fatal loss of blood-constituents, it so affects the circulation through the lungs that the flood-gates of the pulmonary artery are closed; the excretory process then ceases for want of arterial blood; and death results, not from exhaustion, but from an arrest of the circulation.

I venture to suggest that collateral evidence in favour of an eliminative and against an astringent and repressive treatment of choleraic diarrhœa, is deducible from the fact that many practitioners of eminence and repute finding that the treatment by opium is unsuccessful, have long been accustomed to treat these cases by large doses of mineral acids. They give from half a drachm to a drachm of dilute sulphuric acid in water every half-hour, or even oftener, until three or four doses have been taken; and they declare that the diarrhœa thus dealt with ceases much more speedily than when treated by opiates, or when opiates or aromatics are combined with the acid. Now, those who believe that an astringent is essential for the arrest of a diarrhœa, probably consider this to be an astringent method of treatment. But it is an indisputable fact these large doses of mineral acid tend to irritate the bowel, and thus have more of a purgative than

of an astringent action. If any one dispute this proposition, let him, when not suffering from diarrhœa, put the matter to the test by taking four half-drachm doses of dilute sulphuric acid in the course of two hours. He will probably find that these large doses of sulphuric acid act as an unpleasant griping purgative.

Some practitioners are of opinion that the mineral acids act as a chemical antidote to the cholera poison. But, according to Mr. Wakefield's experience before referred to, an alkaline treatment might, with equal reason, be considered antidotal. We know nothing of chemical antidotes for the cholera poison, and probably we never shall.

I am told that there are some weak brethren who are haunted by the fear lest, in treating cholera by purgatives, they be supposed to act upon the homœopathic dogma, *similia similibus curantur*. If the commonly received theory of collapse were true, there would be some ground for this fear; but I have elsewhere endeavoured to show that mere purging will not give rise to the peculiar and distinctive symptoms of cholera, and that purging is not the disease but the cure. The way, then, to escape the dreaded suspicion of an alliance with homœopathy is to adopt a truly scientific pathology. In conclusion, however, I would warn those who persist in giving opiates and astringents, that a do-nothing system of homœopathic globulism will be more successful than theirs. In other words, Nature weighted with narcotics and astringents will lose the race in competition with Nature not so encumbered.—*British Medical Journal.*

TREATMENT OF ASIATIC CHOLERA.

By R. M. Forsayeth, M.D.

At a time when a visitation of Asiatic cholera is not unlikely from its proximity in neighbouring countries, unless otherwise ordained by Providence, I feel called upon to offer a few practical remarks upon alcoholic medication in its treatment, and also such statistics as have come within our reach upon that subject, preventive or curative. A prevalent idea occupies the public mind, that some preventive measure, as alteration or change of diet, or medicine is neceessary at such a time. This seems true only in one or two particulars:—

1st. If the mode of life or diet is acting detrimentally on the health, a change may prove benificial.

2nd. An early abandonment of habits of intemperance. Any other precautionary changes impressing the mind with the likelihood of an attack act injuriously.

A Russian physician states, "It is a positive fact that cholera does

not seize on its victims at hazard, as many say. It has been ascertained that out of every *hundred* individuals who die of this disease, *ninety* are in the habit of drinking ardent spirits to excess."

Mons. Huber, who saw 2160 perish in twenty-one days of cholera in one town in Russia, says, "It is a most remarkable circumstance that persons given to drinking have been swept away like flies. In Tiflis, containing 20,000 inhabitants, every drunkard has fallen—all are dead, not one remains."

Dr. Rhinelander, visiting Montreal in 1832, states, "The victims of cholera are the intemperate." A Montreal journal states, "That not a drunkard who had been attacked had recovered, and almost all the victims have been, at least, moderate drinkers."

Dr. Bronsen of Albany, states, "Drunkards and tipplers have been searched out by cholera with such unerring certainty, as to show that the arrows of death have not been dealt out with indiscrimination; there seems to be a natural affinity between cholera and ardent spirits, and their habitual use in the smallest quantity seldom fails to invite the disease, and render it incurable when it takes place."

Professor Sewall, M.D., visiting New York, says, "That of 204 cases in the Park Hospital there were only six temperate persons, and that these had recovered."

Dr. Mussey, U. S., says, "Upon boats on the river the increase of brandy drinking, consequent on the approach of cholera, has been frightful, and the mortality on board these vessels has been terrible and unprecedented. When this dreadful scourge was raging in New Orleans, amongst the hundreds that were swept off by the disease, only two were sons of temperance, and among the 1200 of that city only three were attacked."

The great and good Mr. E. C. Delevan writes, "In 1832 when the cholera broke out in Albany I was engaged with E. Corning and J. T. Norton in erecting that large block of buildings on Green, Beaver, and Norton-streets. About 100 men were employed, they were all about abandoning their labour, when they were persuaded to remain. They all agreed to keep at their work and abstain from strong drink. A beverage of water, molasses and ginger was furnished them free, and of all those 100 men engaged on the work not one died, nor was the work intermitted a day; one man not under the control of the builders (those excellent mechanics, Fish and Mawley), but employed by the man who furnished the brick, would not adopt the simple beverage offered to him but resorted to the grog shops. He fell a victim."

Professor Miller says, "Of 70 male adults affected with cholera in

Edinburgh Hospital in 1848, only 17, according to their own account, had led tolerably temperate lives, and of 140 females attacked by the disease only 43 were reported sober."

Professor Mackintosh of Endinburgh, who was physician to an extensive cholera hospital, states, "It has been computed that 5-6ths of all who have fallen by the disease in England were taken from the ranks of the intemperate and dissolute."

Dr. Adams of Dublin, affirms, "Our foreign reports testify that drunkards are carried off at once by this dire disease; but those who by a daily use of a moderate quantity debilitate the tone of the stomach and biliary organs become easy victims to the cholera."

The Rev. Wm. Reid of Edinburgh, in his "Temperance Cyclopædia," says, "Dr. A. M. Adams, Professor of Medicine in the Andersonian University of Glasgow, has favoured us with a classified statement of the previous habits and conditions of health of 125 cholera patients treated by him during the epidemic of 1848-49. From this table it appears that whilst those patients, who were represented to him as being of temperate habits, died only in the proportion of 19·2 per cent., those who were of intemperate habits died in the enormous proportion of 91·2 per cent."

I might multiply statements such as these, but assuredly they ought to be quite sufficient to establish the principle of the imperative necessity of refraining from the use of alcohol, or a remedial agent for its cure. This latter I had an opportunity of testing during the dreadful invasion of 1832 as well as 1848, when I relinquished brandy for pure cold water with marked benefit. In corroboration of this statement, I could quote those of some other medical men, but refer just now to only one detailed in THE PRESS of 27th December, 1865, in an article on cholera, translated from the French by Dr. T. M. Madden:—

"The action of cold water in cases of cholera was, however, I believe, first pointed out thirty-two years ago by an Irish medical practitioner, Dr. McCoy, in the *Dublin Quarterly Journal of Medical Science*, and as the article in question is probably unknown to many readers of the PRESS, I shall quote the passage to which I refer:—

"Among the strongest prejudices," says Dr. McCoy, "I brought into the hospital was that against cold water. One of the India reporters, I recollect, states that he never knew a patient recover who was allowed cold water to drink, and other writers denounced it, though not so emphatically. I accordingly requested the nurses not on any account to comply with the entreaties of the patients for cold water. On the 2nd May, a female named Margaret Tusky, aged 21, was admitted into one

of my wards at four o'clock in the evening; she had been nine hours ill; the surface of her body quite cold; her feet, legs, hands, eyelids, and nose were blue; no pulse could be felt at her wrist; vomiting incessant of the rice-water kind; she had two or three alvine dejections before admission; eyelids half closed, and eyes turned upwards; thirst very great; calls for cold water urgent; cramps very distressing; tongue cold and white; pain just below the sternum, which she attributed to having drank two pennyworth of buttermilk during the morning. The necessary measures were resorted to for her. This girl, having observed that a pail of cold water had been left near her bed for some ward purposes, contrived during the evening to draw herself towards it, and putting her head into the vessel drank copiously; it was speedily thrown up again, but the draught was repeated as often as she could without being detected. When I heard of it, I was of course alarmed for the consequences. However, during the night, a patient in the same ward in a convalescent sate, who had felt the deprivation of water herself, not long before, got out of bed several times and supplied Tusky with cold water. This she told me herself the following morning after I had expressed my satisfaction at finding her so much better than when I left her the night before. She recovered. This incident demonstrated that the indulgence in drinking cold water was, at least, not certainly fatal. I therefore commenced giving it in small quantities when called for, and soon after allowed them to drink as much of it as they pleased. I found it the best drink of any I had yet tried, and by far the most agreeable."

I could multiply cases exactly similar, one in particular detailed to me near Cork, confirmatory of the forgoing remarks, "Cold Water *versus* Brandy Cure;" but having already taken up so much of your valuable space, I think it not necessary, sincerely hoping by those few details to draw the attention of our noble and philanthropic profession to the disastrous evils resulting from the indiscriminate alcoholic medication of the present day.

Templemore, May 13, 1866.

BLEEDING IN CHOLERA.

By Ewing Whittle, M.D. Lond., Member of the Royal Irish Academy; Sen. Hon. Surgeon to the Liverpool Dispensaries.

Though fully appreciating the justice of your observation, that a terrible responsibility rests upon any one who writes on the subject of cholera at present, I feel bound to lay before the Profession some observations on the views of the treatment of cholera lately set forth so forcibly by Dr.

George Johnson. This is a subject on which I have a right to speak with some degree of authority; in 1849 I devoted much time to a careful study of the history and pathology of the disease, I was largely engaged in the treatment of cholera cases during the epidemic of that year, and had the charge of the Cholera Hospital of Toxteth-park, a township then containing more than 60,000 inhabitants.

As the result of my practice I showed that the judicious use of the lancet (as practised nearly fifty years ago in India), was the key to successful treatment; that by its use four cases out of five could be saved; that next to its use administration of calomel was most to be relied on; and I believe I was the first to show that the good effects of calomel had no reference whatever to the supposed action of that mineral on the liver. A further experience of the epidemic in 1854 confirmed these views, and you will find the results partially recorded in the *Medical Times and Gazette* for October, 1854. So much for my claim to some attention on this important and now anxious question.

I am quite satisfied that Dr. Johnson is right in his theory that the passage of blood from the right side to the left side of the heart is impeded; this arrest of the blood in its natural course is what calls for relief by blood-letting. Whether this is caused by spasm of the small vessels or whether it be the result of nervous paresis, the fact does not admit of dispute; but I cannot so readily admit the truth of Dr. Johnson's theory, that the vomiting and purging is an effort of nature to carry off the poison. How often is vomiting a symptom of sympathy with a distant organ, as in nephritis, in hydrocephalus, in pregnancy? So much for theory; now with regard to practice. I have invariably observed that if these symptoms can be relieved the severity of the disease is abated; and *vice versâ*, everything which increases the vomiting and purging aggravates the disease, and precipitates the collapse. Under this head may be included all stimulants, astringents, and opium; all alike injurious, not by stopping the discharges, but by increasing them, particularly the vomiting, which is always a most harassing and exhausting symptom. On the contrary, the action of the calomel is the very reverse; it stops the vomiting immediately, and after a few doses the purging. This I have observed myself; it never purges unless it is continued until reaction is fully set up. In support of the correctness of this statement, I can refer you to all the old Indian authorities; for instance, Dr. James Johnson says "that twenty grains of calomel will act as a sedative; and, so far from griping and producing hypercatharsis, it will soothe uneasiness and rather constipate than purge. Annesley and other Indian authors have made the same observation. This appears to me to be the weak point in

Dr. Johnson's views and this I wish to combat—viz., that calomel is given as a purgative or a cholagogue. In fine, one word upon your own remark, that those who read Dr. Johnson's book will come to the conclusion that the best plan is to do nothing in the stage of collapse; if they do, I can assure you that they will lose more patients than they ought to do. Let them open a vein in the arm in all cases; if collapse has gone so far that no blood will flow, the puncture will do no harm; if only a little blood flows, some relief will be afforded; and if the stream continues until the colour changes from black to red (as Annesley long ago recommended), the relief will be very decided, and the patient will in all probability recover, particularly if he has not been previously injured by the administration of stimulants and opium or other irritants. It is generally sufficient to take three or four ounces of blood, the red colour becoming pretty well marked by the time that quantity is drawn from the vein. A few doses of calomel and a spoonful of gruel every now and then, and the application of dry heat and sinapisms to the surface, was the only additional treatment which I found necessary in 1854.

Believing that there is much truth in what Dr. Johnson has advanced, and knowing that his views have made a profound impression on the Profession, I feel bound to enter a protest against what appears to me to be the unsound point both in his theory and practice. However, I am glad that he so far concurs with me as to recommend bleeding at all; this, I think, he did not do in 1854. I practised it with success both in that year and in 1849, and I would beg most urgently of my Professional brethren to give this mode of treatment a fair trial. If they do, they will not be disappointed.

ICE IN CHOLERA.

A correspondent of the (Calcutta) *Englishman* gives an account of the successful treatment of a case of cholera, apparently in an advanced stage, by the application of ice to the spine, as recommended by Dr. Chapman. The writer says:—On Sunday, the 25th Feb., about 10·30 a.m., my servants requested me to go and see a man who, they said, was dying of cholera, and to give him some medicine. I proceeded to the place, where I found a man lying on the ground in the greatest agony, with the usual symptoms of cholora—vomiting, &c. He was much emaciated, and to me appeared rapidly sinking. I had no medicine in the house. I ordered one of my servants to go around among the neighbours and try to get some medicine, but in this I was unsuccessful. I recollected, however, having read in the *Times* an article bearing the signature of John

Chapman, M.D., 25 Somerset-street, Portman-square (of which I had taken a note), in which the writer advocated the use of a bag of ice down the spine. Feeling that if I did no good, I could, under the circumstances of the case, do no harm, I made up my mind to try whether ice would do any good. I now proceed to give you an account of what I did, and as to what the results were. 10·30 a.m.: The man, a Mussulman, a hackery-wallah, arrived with his own and other hackeries from Calcutta. He had been for two or more hours purging and vomiting violently; voice scarcely audible; pulse imperceptible; hands, arms, legs, and feet quite cold. He was throwing his legs about and twisting his body in great agony; he complained much of thirst. I gave him water with a little carbonate of soda in it. He appeared to be sinking fast. 11·0: I procured some ice from a neighbour. Having no gutta-percha bag, I took the leg of a pair of flannel trousers and made a long bag to reach from between the shoulder-blade to the bottom of the spine, of a width of three inches; into this I put broken ice, and applied it to the spine. After I had applied the ice the purging and vomiting ceased, and by 11·20 the spasms were much diminished. 11·30: The patient was much easier. On questioning him he said in a very low voice that he felt easier. A little before twelve I found that his pulse was perceptibly stronger, and that his arms and legs, which were previously as cold as stones, began to be slightly warm. The flannel in which the ice was put was now saturated with water, and as Dr. Chapman said the cold was to be a dry not a wet cold, one of my natives suggested the use of a bottle, on which I got a preserved-fruit bottle into which I put the ice, and had the bottle held against the spine. 12·30 p.m.: No more vomiting, &c., the arms and legs getting warmer; no pain, very great thirst; I let the man drink as much as he liked. From this time till 1·30 I kept the bottle of ice on his back, when, finding that his hands, arms, legs, and body were becoming hot as if he had fever, I removed the bottle of ice; and as I was about to leave my house for tiffin with a neighbour, I told my khansamah if fever came on to put in the place of the ice-bottle a bottle of hot water. 4·30; I returned to the man. He was fast asleep, and a more deadly object I never saw. At 5·30 he awoke and asked for food. I gave him some thick conjee with sugar and brandy. 7·30 a.m., Monday, 26th: The man is sitting up; convalescent, but weak. He wants to take his bullocks and hackery away. The above are the facts of the case."—*Medical Times and Gazette*, April 28, 1866, quoted from the *Homeward Mail*.

A CASE OF ATAXIC LOCOMOTIVE PROGRESSIVE PARALYSIS.

By R. Murney, M.D., fellow and licentiate of the College of Surgeons, Ireland.

The case which I am about to describe will serve to illustrate a disease (if my diagnosis be correct) which is well known in Dublin, but not so well throughout the country: it is for the latter reason that I venture to bring forward this case so as to ventilate the subject as much as possible. I confess that I cannot throw any new light on the pathology of the disease, inasmuch as I had no post-mortem in the case. The disease ran its course in four months, terminating in perfect recovery, which was contrary to my prognosis, as I looked on the symptoms indicating a disease which experience has hitherto, at least, found to be incurable. The disease I allude to is the ataxic locomotive progressive, described by M. Duchenne. The subject of this paper is a young gentleman of about 20 years of age, tall and well proportioned, fair complexion; he consulted me on the evening of the 7th of last February; said he suffered great pain since four o'clock in consequence of not being able to pass any urine; made some about eight o'clock in the morning, but none since, though having made many efforts; said this incessantly came on without any cause that he was aware of; was in his usual health and spirits the previous day, and slept well the night before. I passed a catheter without the least trouble, urine natural in quantity and quality, and acid reaction, sp. gr. 1020, it was microscopically examined by Dr. Barker, who pronounced it healthy. I passed the catheter twice each day for fifteen days. At this date the bladder recovered itself for a short time. On the third day of my attendance on him his gait became unsteady, and when turning round staggered. In the course of a month, as those symptoms increased and became aggravated, could not walk with his eyes shut, had great difficulty in maintaining his balance, when once in motion he could get on pretty well by moving quick with his head down and eyes fixed on the ground. About a week after the bladder recovered itself, another and distressing condition of it set in, incontinence of urine; his face presented a peculiar sardonic grin, apparently drawn to right side; no anæsthesia on the left; right eye amaurotic; pupil dilated; appetite bad; lived on wine and brandy occasionally. His friends were greatly afflicted at the appearance of his face, being so much changed; did not perceive it himself; his memory was not as perfect as usual; he became careless and filthy in his habits; complained of pains in his thigs and legs. The history he gave of himself was:—Had always enjoyed good health; both his parents and all his family were living; a few days before last Christmas got for the first time in his life pain in his head and back, with sick stomach; went to bed; lay for three weeks.

with a feverish cold, as he was told by the medical man who attended him when convalescent; went to Kingstown; there became strong in a short time; a few days before his return to town got a pain in the lower part of his back corresponding to the fourth lumbar vertebræ; it was very severe for some days; the intensity of it gradually lessened and finally disappeared. When first consulted the treatment consisted of citrate of quinine and iron, a blister over the region of the bladder, beef-tea, wine, &., subsequently tinc of ergot, strychnia, nitrate of silver; the latter medicine he continued to take for four weeks without ever making any change in the colour of his skin. The patient himself imagined he was very much improved, though I could not perceive it. Went to Galway on the 2nd of April to see some friends there; returned on the 4th of May to Dublin, to my great astonishment strong and fat, and to all appearance quite recovered. How long he will continue so remains to be seen. While in Galway discontinued the nitrate of silver pills and all other medicine. On my first interview with him after his return, the first thing he did was to walk with his eyes shut and dance a polka. I confess that on this occasion I was not a little shaken in the correctness of my diagnosis, but when I grouped all the symptoms—the staggering gait, the peculiarity of raising and putting down the foot, the heel first, the head bent, with eyes looking down on the ground, the amaurotic eye, and the inability to walk with eyes shut, which M. Trousseau says is pathognomonic of the disease ataxic—all these symptoms confirmed me in my former diagnosis. The only other disease that this could be confounded with is reflex paralysis. On this subject I published two papers, in one of them the disease was ushered in by loss of power of the bladder, succeeded by incontinence of urine. In those two somewhat similar, but totally different diseases, you have in both the staggering gait, the peculiar motion. In the ataxic the heel is first put down, in the other (reflex) the toes. In the latter the patient can walk with his eyes shut and his head erect, never any affection of the eyes or face, which we know is not the case in the ataxic.

In those cases of reflex paralysis alluded to, both recovered, they were read and discussed at the meetings of the Surgical Society of Ireland.

Chronic Dysentery.—H. Hartshorn, M.D., has treated chronic dysentery of three months standing, where the patient was relieved entirely in a few days by the use, twice daily, of an injection of four ounces of water, containing eight grains of zinci sulphas, with forty drops of laudanum.—*New York Journal of Medicine, vol.* 3, *p.* 271.

Canada Medical Journal.

MONTREAL, JUNE, 1866.

AN APOLOGY.

We owe an apology to our readers for the delay in the issuing of the June number. During the greater part of the month, both editors were on the frontier, in medical charge of volunteers, and this is our excuse. We trust to have the July number ready very shortly, and will use our best efforts to have the future numbers out in good season. May we again solicit the aid of our subscribers for original communications? We must say we find a sad want of energy among the practitioners of this Province, in giving the result of their observations.

THE HEALTH OF THE CITY.

"It is an ill wind that blows nobody good," is an old saying, and we think it has been proved a true one, in relation to the anticipated visit of cholera. From all appearances, and judging from the past, we had good reason to apprehend the appearance of this terrible disease in our midst this season, and we in this Journal, more than once raised our voice against the criminality of neglecting to take all the precautions which sanitary science has taught us, were likely to mitigate or prevent the appearance of the scourge. It was hard to rouse our people to a realizing sense of the duty they owed, not only to themselves and their families—but also to their fellow-citizens; but once fairly roused into action, Montrealers are proverbial for the earnestness and enthusiasm which they throw into all their works. It was late in April before our citizens were aroused to the danger which seemed to be overhanging them—in the threatened appearance of cholera; but then two health officers were appointed, and a sanitary association formed, and the result of their combined labour, is that on the 1st of July, we find our city in a state of cleanliness, such as we believe it has never before in its history been. Of course we do not for a moment mean to assert that this cleanliness has prevented the appearance of the disease—but we do assert, that had we been visited by cholera, the condition in which the city is now in, would have exerted a very modifying influence over its spread and fatality, as it has been exerted, even over the ordinary mortality of the season. Seldom, if ever, after such intensely hot weather as we have had for

month past, has the weekly bills of mortality raged so low, we might say remarkably low, and we think the result of our labours to prevent or arrest cholera, should it appear, has taught us a most valuable lesson, which we trust our citizens will never forget. If by such simple means as have been made use of—we can bring about such grand results—how grave a responsibility would rest upon us—if in the future, we should neglect to employ them. Montreal has ever been remarkable for its very high mortality, and one of the reasons—what others there may be we do not enter upon now—we at all events have ascertained to be the filthy yards, foul emanations arising from putrefying animal and vegetable matter so long usually allowed to remain, and which this season have been compelled to be constantly removed. This should be ever remembered—should be earnestly impressed upon all who have the good of our city at heart; and we feel that much credit is due to the Sanitary association in placing before the people facts, which show them how simply yet effectively preventible diseases may be kept from their doors. We anticipate much permanent good will result from their labours, and whether cholera come this season or not, (for it is yet too early to say whether we will yet altogether escape it, although appearances seem to indicate we will,) we have certainly realized great benefits from its anticipated appearance. In closing we would simply add, events have taught us there is ample work for a Sanitary association in Montreal, and we hope that the association will become a permanent one—the fruits of whose labours will become yearly more visible.

OUR VOLUNTEERS.

Since we last issued the Journal, our country has been roused from Sarnia to Gaspé, by the infamous invasion of the Fenian brotherhood, and never before has Canada been so united, or so nobly responded to the call for volunteers. We hope perhaps ere a great while, to be able to publish something regarding the medical history of the brief—but hard campaign—in the meantime we invite attention to the communication of Dr. Caniff, on some of the wounded at the battle of Ridgeway. Among the Montreal volunteers sent to the frontier, considering they were under canvas all the time, and that the weather was remarkable rainy—that there were many badly clothed and shod, ill-supplied at first with overcoats and blankets—that they performed hard and long marches over most abominable roads, that they were hard worked on guard and picket duty—they were most remarkably free from serious disease. Among the Field Battery, Cavalry, Prince of Wales, Victorias, and Royals, very few cases presented themselves for admission into hosiptal. The majority that

were admitted, were suffering from sore feet, caused in the majority of instances by badly fitting boots. Anticipating active work—the desire seemed to be to keep out of hospital, and ready for any emergency. Slight colds were common from severe wettings—especially after the march from Hemmingford to Durham and Huntingdon.

TO OUR SUBSCRIBERS.

With this number, the second volume of the Canada Medical Journal is brought to a close, and we thank our subscribers for the countenance and support they have given us, since our Journal commenced its existence. The volume just about closed, although not as successful as we could have desired, has yet been in many ways successful. Our subscription list embraces a large proportion of the practitioners of Upper Canada, and a majority of the English speaking medical men of Lower Canada, as well as many of our excellent French brethren, and at the close of the second volume, we find that few have deserted us—while many have come to our aid. For this measure of success we are thankful, and will endeavour by making the Journal the means of spreading the views of the principal medical writers of Europe and America, still further to desire the approbation and assistance of the profession. Those of our subscribers who have not yet remitted the subscription for this year, will please do so at once; and the few who have not yet paid for either volume, must have strange ideas, and the possession of a conscience—the elasticity of which we do not envy them. If they wish the *Journal* continued—they must remit the amount for both volumes immediately. We will furnish the index to volume 2 in the July number.

MEDICAL NEWS.

Mr. Frederic William Lloyd Hodder of Toronto, passed his examination before the Apothecaries' Hall of London, on the 8th February.

Treatment of Coryza.—M. Luc, an Assistant Surgeon in the French army, recommends the inhalation of tincture of iodine in nasal catarrh. "I inhaled tincture of iodine," says he, "from a phial for one minute at a time, at intervals of about three minutes; the heat of my hand was sufficient to promote the evaporation of the iodine; the headache yielded first, sneezing became less frequent, the secretion less copious, and although the inhalation caused a burning sensation in the throat, I was entirely cured at six o'clock, P.M., of a cold which from nine A.M. to three P.M. had been sufficiently violent to compel me to use four pocket handkerchiefs."—*Dublin Medical Press.*