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OF

MEDICAL & PHYSICAL SCIENCE.

EDITED BY

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Art. LXXV.—OBSERVATIONS ON HYDROCELE OF THE TUNICA VAGINALIS, AND ON ENCYSTED TUMOURS OF THE LABIUM.

By Robert L. Macdonnell, M.D.

Licentiate of the King and Queen's College of Physicians, and of the Royal College of Surgeons, Ireland; Physician to the Montreal General Hospital; Lecturer on the Institutes of Medicine, University of M'Gill College.

Large Hydrocele of Tunica Vaginalis radically cured—Spermatozoa found in the Fluid—Observations.

A man, aged 45, was admitted under my care into the Montreal General Hospital, March 6, 1846, labouring under chronic bronchitis, with emphysema of both lungs. He was treated for the pulmonary disease, for some days, before it was discovered that he had a large tumour which he had taken great pains to conceal, occupying the left side of the scrotum, and extending from the inguinal ring almost to the knee. This tumour was of an oval shape, its upper and lower portions being of the same size, whilst in its middle it was dilated; it could be traced to the ring, but it was not possible to force it through that opening; its surface was smooth, and the scrotum was stretched tightly over it; it was not painful in any part, and yielded a dull sound on percussion. On coughing, the tumour was evidently *moved*, but the peculiar impulse of hernia was not perceived. *The testicle was found situated at the very lowest point of the tumour;* it was not enlarged nor adherent to the neighbouring parts, and pressure upon it, caused the usual sensation—the epididymis and cord could be traced at the back of the tumour for a short distance, but at the ring they could not be distinguished.

The patient could not give an accurate account of the origin or mode of growth of this tumour, except that he noticed it on the side of the scrotum shortly after he was attacked with the bronchitis, seven years ago, and that since then, it had gradually increased, but had never caused any uneasiness, except a dragging sensation in the loins, from its great weight. It was evident, that the tumour was either a hernia or a hydrocele; its shape, *the position of the testicle*, its having first appeared after severe coughing, countenanced the idea of a hernia, whilst the want of impulse, and its smooth and even surface, notwithstanding its shape, were characteristic of hydrocele. With either disease, we might have complete dullness of sound on percussion; for a hernia on the *left* side, even of equal size, might be composed almost entirely of omentum, but the transparency of the tumour, as proved by the transmission of light, clearly showed it to be hydrocele.

March 19th.—The fluid, amounting to *forty* ounces (accurately measured) was drawn off, and an injection, composed of two parts of tincture of iodine and three of water, was introduced. In a few days the tumour had attained nearly half its original size, but there was no evidence of lymph being effused, and at the end of three weeks, there being no attempt at cure, the fluid was again drawn off, and a method treatment adopted, which was recommended by Dr. Adams, of the London Hospital, in vol. ii. of the *Lancet*, for 1843.

A camel hair brush, dipped in undiluted tincture of iodine, was passed through the canula, and freely applied to all parts of the serous membrane within its reach. The next day, the tunica vaginalis was partially filled with a solid mass of lymph about the size of a large orange. This gradually became absorbed, and in less than three weeks he was discharged perfectly cured.

He again consulted me, in June, 1848, for benign polypi of both anterior nares, which had displaced the bones and cartilages of the nose, and had given rise to great deformity and difficulty of breathing. I took advantage of the opportunity to examine the seat of the hydrocele, and found the parts in precisely the same state as when he left the hospital. And again to-day, Feb. 22, 1849, I examined the parts and they are found in a perfectly healthy condition.

The foregoing case is not devoid of interest to the practical surgeon, on the following grounds:—

1. The situation of the testicle is quite unusual. Authors have varied in their statements as to the position which this gland occupies, in hydrocele—according to some, it is more frequently met at the posterior part, a little above the centre of the tumour; others again assert that it is usually found *below* the centre, and some have stated that we find it occasionally in front of the tumour, but as far as I am aware, no one has described its occasional appearance *at the very lowest part of the tumour;* on the contrary, the best practical writers agree, in considering this situation of the testicle, as a most valuable diagnostic sign between hydrocele and hernia; and it is not unlikely that a careless or superficial examination of the above case, would have led to this error; its history, and the appearance of the tumour, being more calculated to mislead the surgeon, than elucidate the nature of the disease.

2. The failure of the Tincture of Iodine injection is extremely unusual. In a note to *Chelius' Surgery* by *South*, it is stated that out of "eleven hundred and forty-eight cases treated by iodine injections, only *three* cases failed."* In instances like the above, I would strongly

* There can be but little doubt, that this success is greatly exaggerated, for I find my friend Dr. Bellingham, of Dublin, has recorded the failure of iodine injections in some of his cases, and feels disposed henceforth to adopt Mr. Adam's method of applying that remedy.—*See Dublin Medical Press.*

recommend the iodine to be used in the same manner as employed by me, for it does not give more pain than the injection, and is not so likely to excite excessive inflammation as the introduction of a quantity of pure tincture of iodine, a method of treatment advocated by some surgeons, and it is certainly a much less painful plan than that lately recommended by Professor Porter, whose operations I have had opportunities of witnessing.

3. A short time ago, the late Mr. Liston showed that the fluid of hydrocele frequently contained spermatozoa, and he drew from this circumstance the conclusion, that in such cases a radical cure by the obliteration of the sac is not to be expected; for, instead of the latter being composed of the tunica vaginalis (a serous membrane), it is formed of the mucous lining of one of the seminal ducts —a dilatation of which has taken place, in a manner similar to that observed in the formation of the tumour of Ranula. When this discovery was first announced, a good deal of importance was attached to it by the illustrious author himself, and by writers on systematic surgery,* yet in the case under consideration, a most complete refutation of these views was furnished; for though the fluid contained an immense quantity of spermatozoa, as proved by careful microscopic examination, the cure was accomplished by adhesive inflammation of the walls of the sac: the result of treatment was also strongly opposed to an opinion recently advocated, viz., that it is not by the effusion of lymph, and adhesion of the walls of the sac, that the cure of hydrocele is effected, but by a restoration of the functions of the absorbents of the part. That a cure is frequently accomplished without obliteration of the sac, is no doubt true; but, on the other hand, it is equally certain, that it is by adhesion that the radical cure takes place in a great number of cases, for we know that it is by producing this condition that the success of various plans of treatment of hydrocele is established.

In reference to the presence of spermatozoa, Mr. Liston says—"This subject deserves further investigation to discover—first, if the limpid fluid, drawn from cysts of the scrotum and inguinal region, uniformly, or often contains spermatozoa. Secondly, What connexion subsists betwixt the seminiferous tubes and their cysts? Thirdly, Whether or not, dilatation of these parts of the epididymis or vas deferens, by obstruction or otherwise may not, in some instances, give rise to these collections? If so, this being a pouch lined by mucous membrane, we should have an easy solution of the difficulty regarding a radical cure not following injection as in the serous cyst. The microscopic examination of the lining membrane of a recent cyst would easily settle the nature of the secretory surface."

In the same volume of the "Transactions," we find a paper on the "*Presence of Spermatozoa in the Fluid of Hydrocele,*" by Mr. Lloyd of St. Bartholomew's Hospital. This gentleman's observations go to prove that we do not find spermatozoa in the clear limpid fluid *devoid of albumen,* as stated by Mr. Liston, and consequently their presence is not exclusively confined to hydrocele of the cord; for in one of his cases, "the situation of the fluid was such that there was no reason to doubt the case being hydrocele of the tunica vaginalis." In his second case he merely mentions that he found spermatozoa in the fluid of a hydrocele, which he had previously tapped 15 or 16 times, but no mention is made of its precise nature, but in the third case, in which "the situation of the fluid appeared to be very much like that of common hydrocele of the tunica vaginalis," spermatozoa were found in great numbers, although the fluid "displayed very much the appearance described by Mr. Liston, as exhibited by the fluid of the encysted hydrocele in which he had discovered spermatozoa, but in one respect it differed from that, as it contained a considerable quantity of albumen"—the product of secretion from a serous membrane.

Mr. Dalrymple, in the 27th volume of the same work* mentions that both he and Mr. Liston had lately found these animalculi in the fluid of common hydrocele, and accounts for their presence in such cases, by supposing that the testicle or cord had been punctured during the operation, and thus an exit afforded for their escape; and at a subsequent meeting of the Society, Mr. Paget stated, "that the most probable explanation of these cases, therefore, seems to be, that certain cysts, seated near the organ which naturally secretes the materials for semen, may possess a power of secreting a similar fluid; and this explanation is in some measure supported by the analogy of those cysts which are found in the ovaries, and more rarely in other parts of the body, especially beneath hairy parts of the skin, and in which the ordinary products of the skin, such as epidermis, sebaceous matter, hair, &c., are formed on the genuine cutaneous tissue of their internal surface."†

It appears to me that neither of the above explanations is satisfactory. It is true that by a careless operator the testicle or cord might be punctured in a small hydrocele, but in one so large as to contain forty ounces of fluid, and in which the testicle and cord were removed to a great distance from the point of entrance of the trocar, the escape of spermatozoa cannot be accounted for on such grounds. And Mr. Paget's solution of the difficulty seems equally untenable; for without resting our objection to it on the fact that cysts in the neighbourhood of other glands, whose secretions are purely (or nearly so) excrementitious, as the kidney and liver, are not found to contain the most essential elements of these secretions, and that the fluid of cysts developed in close contact with the testicle and seminal ducts is found destitute of seminal animalcules, as proved by the recent observations of Gosselin,† it is impossible to believe that a diseased serous membrane should possess the property of se-

* "It is still matter of dispute, whether these had escaped from an accidental wound or giving way of the tubular structure, either of the testicle or of the epididymis; or whether the cyst, from which they were derived, had been formed by dilatation of the tubular structure—as takes place in lacteal tumours of the breast, and in ranula. If the latter opinion prove true, as is inclined to by Mr. Liston, little benefit need be expected to result from injection in such cases."—*Miller's Practice of Surgery,* page 614, American Edition.

* Medico-Chirurgical Transactions, vol. 27, p. 401.

† Vide Archives Générales, tom xvi.; and British and Foreign Medico-Chirurgical Review, No. IV. p. 533.

creting a fluid so elaborate as the semen, and one which is so clearly endowed with vitality. *

It seems to me to be more consistent with the facts of the different cases that have been placed on record, and with the particulars of that just detailed, to suppose, that in the first instance, the disease is merely a simple dilatation of one of the ducts, the result of contraction or obliteration of its canals from local inflammation, caused by some injury to the part: that after some time, this dilatation gives way and pours its contents into the cavity of the tunica vaginalis—in which they accumulate in some instances without interfering with the functions of the membrane; but in other instances the serous membrane takes on diseased action, and its secretion becomes mixed with that originally poured out from, and still secreted by, the ruptured cyst. In this way we can understand how the fluid may, in one case, present a limpid appearance, devoid of albumen; and in the other exhibit a copious admixture of albumen, and a variety of colours; and in both, we may find, on microscopic examination, a quantity more or less abundant of spermatozoa, in some cases alive, in others dead and partially disorganized.

The practical deduction to be drawn from the above case, however, is, that the detection of spermatozoa should not deter us from attempting the radical cure, which, if we should not accomplish by one method, we may succeed in effecting by another.

—

Encysted Tumour of the Labium, successfully treated by the application of Nitrate of Silver to the Interior of the Cyst.

Having met with some cases of Encysted Tumours of the Labium, which presented greater difficulty in diagnosis, than is stated by writers on diseases of females, to exist, and having derived the greatest satisfaction from a peculiar method of treatment of these tumours, I am induced to lay the particulars of the following case before the profession, in illustration of these two points:—

I was consulted, March 16, 1846, by a lady aged about thirty-six. She stated that three years before, after a severe and long journey in the winter, during which she suffered much from the bad state of the roads, she was attacked with violent vomiting, which lasted for a fortnight. After it ceased, she noticed, for the first time, a small tumour inside the left labium, which she considered a rupture produced by the efforts at vomiting, having formerly been subject to inguinal hernia. This tumour gradually increased up to the present time; it was not at any period painful, never receded or diminished in size, never emitted any gurgling sound, nor was it ever the seat of œdematous swelling, heat, or redness. It had gradually increased, and had latterly interfered with her movements, and for some time previous, had prevented her sitting down, except with the body reclining far back, and when she sat on a hard seat, pain of an acute kind used to shoot upwards from the tumour through the sacrum. Latterly she has been annoyed by a sensation of dragging from the loins. The tumour itself had never been the seat of pain, nor had there been any discharge from the vagina, or any irritability of the bladder. The catamenia had been absent for years—she had never been pregnant but once, and then she had a miscarriage.

On examination, a tumour of an oval shape was found occupying the left labium; it was about the size of a goose egg, running along the labium from the anterior fourchette to the perinæum, *and sending a process upwards, for about two inches along the wall of the vagina.* The skin covering it, was devoid of any inflammatory appearance and moved freely over it. There was no pain complained of when pressure was employed. On coughing, *an impulse was given to the tumour when it lay in its usual position,* but when it was lifted up, it did not receive any. On percussion, it yielded a dull sound. All attempts at making it enter the abdominal cavity completely failed. No irregularity was noticed on its surface; no rumbling heard at any time, nor was there the least change produced by the varying conditions of the bowels; when constipated, the tumour was as soft and as small as when they were relaxed.

March 20.—An exploratory puncture was made, and matter of a dark olive colour, devoid of odour, escaped. The opening was enlarged, and a tumbler full of thick fluid flowed out, which was of a creamy consistence, and on microscopic examination, was found to be composed of decomposed pus globules, with a large quantity of what appeared to be epithelial scales intermixed. The sac of the tumour was freely cauterized with nitrate of silver conveyed on a probe, and the orifice kept open by means of a plug of lint.

March 27.—The sac of the tumour has been filled up with solid secretion, and has undergone great diminution in size; no general disturbance.

April 1.—Scarcely any trace of the tumour to be detected, except some thickening of the labium, giving to it a greater fullness and prominence than the other. Ordered to apply mercurial ointment.

April 10.—Perfectly recovered—no traces of the disease left.

With the exception of Dr. Ashwell, no writer on diseases of females, that I have consulted, speaks of the points of similarity presented by some cases of encysted tumours of the labium and vaginal and pudendal hernia. He remarks, "that he has known much difficulty arise in diagnosis of this tumour, from a hernia," and in the case I have detailed, the size of the tumour, its shape, supposed origin and apparent connexion with the interior of the pelvis, by means of the ascending process already described, together with the fact, that the patient was predisposed to rupture, and that the tumour received some impulse from coughing, made me proceed cautiously with an exploratory puncture before laying open the cyst.

It is true, that for one case which will present any difficulty in diagnosis, we may meet with a dozen where no such obscurity exists, yet from the occasional occurrence of such cases, the practitioner should examine all, with care, before attempting a cure—for it is strange that though the differential diagnosis between inguinal tumours and hernia in the male, is insisted upon with great force by all writers on these subjects, yet, in works especially devoted to diseases of women, little

* Vide Müller's Physiology, by Bayley, vol. 1, p.

attention is paid to the matter. But it was more especially with the view of illustrating a method of treatment, which I have found invariably successful, and productive of little annoyance to the patient, and of easy application, that I have laid the foregoing case before the profession.

Four plans of treatment have been recommended for the cure of these temours:—1. Complete dissection out of the whole of cyst—a plan which must be extremely difficult in most cases, in all, extremely painful, and in such a case as mine, quite impracticable. 2. Laying open the cyst, and filling it up with charpie. 3. Seton; and, 4. Removal of the fluid, and then compression, so as to bring the walls of the cyst into close opposition. The plan of treatment which I have employed for some years past, has been to cauterize with nitrate of silver, the lining membrane of the cyst, so as to cause adhesive inflammation, and this process I have found to be so readily excited by the caustic, that I have never been obliged to repeat it a second time. In some instances I have touched the granulations, occasionally, during the progress of the cure, for the purpose of hastening the filling up of the sac; and these were instances where I believed that the nitrate of silver had acted, not by inducing adhesive inflammation, but by effecting a change in the functions of the membrane, in consequence of which, it threw out granulations, instead of secreting, as formerly, a peculiar fluid. In every case in which I have used the nitrate of silver in this manner, a speedy cure has followed, unattended by any bad consequences, and the patient has not been aware, in the majority of instances, that anything beyond the mere puncture of the cyst has been attempted. When we reflect for a moment upon the difficulty of dissecting out a cyst even of moderate size, and upon the excessive pain the patient must endure, both in this operation and in the second and third I have mentioned, and when we recollect the extreme difficulty, if not impossibility, of applying accurate pressure, it will be allowed, that the method I propose, is at least, unattended with any of these inconveniences; and if it should prove in the hands of others, as successful as it has done in mine, and I have little doubt but it will—it must be considered a plan of treatment preferable to any recommended for the cure of this disease.

The method of preparing the caustic may not be known to some of my readers, and I shall therefore make no apology for describing it:—A large-sized probe should be dipped in caustic, which has been rendered fluid by melting in a watch-glass, over a spirit-lamp or wax candle, until there is a complete coating of the caustic on the probe. When this cools, we have the nitrate of silver, in a form well suited for being conveyed through a small opening and into a deep cavity, and by bending the probe, we suit it to the shape of the cyst, and thus it can be brought into contact with all parts. In large cysts, such as that under consideration, I have had two or three probes thus prepared, as the quantity of caustic coating one, is not enough for the extent of surface to which it must be applied.

This method of destroying cysts, I have been in the habit of employing, in other diseases, as in the encysted tumours of the eye-lids, and in sebaceous encysted tumours; and lately I succeeded in curing a lady of a tumour of the shape, and about twice the size of an almond, which had been growing for some months on the left jaw, and which had resisted every plan of treatment proposed by her former attendant, who had at last recommended its extirpation. A small puncture was made into it, and its contents, which were composed of a reddish jelly-like substance, were squeezed out. A probe, coated with caustic, was introduced, and freely applied to its interior. For the next few days, a small quantity of bloody serum oozed out, but the tumour gradually diminished in size, and now, no trace of it remains, nor is there the least scar visible, which I need not say, is a matter of some importance.

The discovery of this method of conveying lunar caustic to deep recesses, has been ascribed by some of the writers in the *Dublin Quarterly Journal of Medicine*, to my friend Mr. Wilde; but the paper in which he first alludes to it, was submitted to myself as Editor of that periodical, before its management fell into his hands; and in a note referring to the matter, he attributed the discovery to Mr. J. Morgan, from whom he had learned it. I mentioned, at the time, that Mr. Morgan was not the discoverer, for the plan was quite familiar to myself and other surgeons long before Mr. Wilde's paper was written, and had been spoken of at a meeting of the Surgical Society, in connexion with the treatment of small nævi. The paper was published, and the obligation to Mr. Morgan omitted; and hence the origin of "Mr. Wilde's method of applying caustic."

Art. LXXVI—OBSERVATIONS ON CHOLERA.

By George Griffin, Esq., Surgeon, (H.P.) 85th Light Infantry, Quebec.

(*Continued from page* 270.)

During the prevalence of this disease amongst us, Quebec was visited by Dr. S———, who had resided for many years as a practitioner of repute in the West India Islands; and had written on "The Blood." At the desire of Dr. Skey, he addressed the medical officers; his observations, from notes taken by me at the time, were principally as follows:

"He advocated the use of the common seidlitz powder, with a view to lessen febrile irritation and remove depraved secretions from the bowels."

"When the stomach was very irritable, mustard sinapisms to the epigastric region—for cramps in the extremities, friction with hot flannel."

"A powder, after the action of the seidlitz was secured, containing carbonate of soda 3ss, muriate of soda 9j, chlorate of potass, gr. vij, dissolved in warm water, repeated in severe cases every half hour, in less severe every hour, and in some *malignant* cases every fifteen minutes, and continued till the circulation was fairly restored; sometimes in very severe cases the chlorate of potass was given alone, in ten grain doses."

"A strong solution of muriate of soda was also

thrown into the rectum, at as high a temperature as the patient could bear."

"In severe cases the patient was placed in a hot saline bath, with evident advantage. It is well known that a saline fluid is a better conductor of heat, than fresh at the same temperature, but, independently of this, a portion of the saline matters may be absorbed by the skin, and the patient also benefitted by respiring the saline vapours."

"Seltzer water in any quantity was allowed as drink. A strong solution of green tea, also used with advantage."

"It is necessary to keep a large fire, both night and day, in the room occupied by cholera patients, for when the temperature of the blood is low, as in collapse, if the air which the patient breathes be cold, it of itself may be a cause of sudden death."

"Not one particle of opium to be given in any shape, except where the stomach is irritable. In these cases twenty-five or thirty drops of the Tr. opii may be given in a small enema."

"All discharges from the bowels and stomach to be immediately removed, and the rooms fumigated twice a day with gunpowder, and, as a last resource, the injection of a saline fluid into the veins."

Proportions of solid and fluid matter in healthy and "cholera blood."

Healthy Serum, - - - - - - -	55
Crassamentum, - - - - -	45
	100
Cholera Serum, - - - - - -	32.34
Crassamentum, - - -	67.66
	100.00

"Taking a vessel into which a patient had just been bled, he poured off the serum, and observing upon the dark colour of the firm coagulum, he added common salt, and the bright scarlet hue imparted to it, he insisted, was in support of his opinion, that cholera was the sudden abstraction of saline particles from the blood, and your aim should be to restore it, which, if you succeeded in effecting, the patient recovered."

The only comment I shall hazard in the practice recommended by this gentleman, Dr. S———, quoted above, is an extract from a few manuscript notes given me by Mr. Watt, the manager of the telegraph station at Quebec; his respectability and character generally are well known; he is still alive, hearty and vigorous, though a very old man; I give it in his own words:

Quebec, 15th August, 1832.

At 4 o'clock, I was attacked with a looseness, violent, (about a quart every two hours) of a thin fluid; no pain. It continued all the day pretty regular; I went to bed ten minutes before midnight, and fell asleep as usual, but waked about 2 o'clock in great pain, like as if I was sawed with rough saws all over my body, and in all directions; a violent motion was in my bowels, twisting, pulling, and noisy, sometimes like rushing water, at others a cracking; pulse, full, soft and regular, but was accompanied with a recoil, hitherto unknown. I got out of bed and took the four grains of calomel, and about one grain of opium, that was in waiting on the table, and waited the result; the stools continued full and frequent; the pulse gradually quickened, and grew harder and smaller, even to the size of a fine wire, and irregular. At 3 o'clock cramps began, and the Asiatic Cholera Morbus was in powerful possession of me. Now I began to doubt the power of the medicine, for the disease was gaining on me fast; to repeat the dose could not be of use, for calomel does not act on me in less than five or six hours, and in less than that time death would arrive most likely. I had always attentively considered the remedies, &c., recommended in newspapers, but was not satisfied with any of them, to be a direct cure—because any of them could not stand examination by just reasoning to be a specific, or even probable cure for this disease, and, moreover, by considering the Board of Health's report published, I find the number of deaths (nearly) equal to the number of admissions into hospital, or, in other words, that "they only went in to die."

Having ever been of opinion that the *cholera* is a *disease of the blood*,* and that its first and principal seat is the stomach, I determined to try the effects of a vomit, and, as light emetics work on the upper part of the contents of the stomach, therefore are likely to leave the seat of the disease below unremoved; I resolved on something heavy, but having no choice in my reach, and as no time was to be lost in sending to the town for drugs, (the telegraph station was in the citadel) I fixed on common salt mixed with a little mustard. Having boiling water ready, I took *about* a quart of salt, the heaped full of a table spoonful of mustard-seed in powder, mixed dry, and then poured on boiling water enough to melt the salt only and no more, to have it as heavy as possible. For I had in view the aim:

1st, To cause the solution to possess the lower part of the stomach, by being heavier than its contents.

2d, To cleanse the inner coat of the stomach.

3d, To get some of it into the duodenum unmixed with the morbid matter in the stomach, before the vomiting began, if the pylorus allowed it to pass through.

4th, To lift up and keep afloat *all* the contents of the stomach, pressing on the *cardia*, ready to be expelled (the whole) by vomit.

As soon as it cooled to drinkable, I swallowed about the half; this was at 4 o'clock, a.m., 16th August. Being in great pain, and the cramp incessant in the extremities, (below the knees and below the elbows,) my opinion was, that an hour would decide my fate, by death or recovery. I viewed myself in the looking-glass, found my face seemed much longer and narrower than its health-form and ugly, eyes sunk, and at times lost sight for five or six minutes, and resumed it again; my feet to above the ankles, and my hands above the wrists, were of a lead colour, and the veins darker, fingers long and small, their nails had a dry glossy white, and much curved, the skin on the back of the hands was crumpled lengthwise, like ploughed land; pulse at the wrist hard, quick, and small, but I could not count

* The italics same as in the original.—C. G.

it, for the frequency of the cramps did not allow a minutes interval, and before the vomiting came on it ceased to be felt, and the sense of feeling with the finger nearly so; the tongue was wet, porous, and looked half rotten. At twenty minutes past 4 o'clock, I took some more of the solution, kept warm, and continued to do it by mouthfuls, at five minutes intervals, till 4½ o'clock, when the vomiting began. The first discharge was of undigested food, more than a quart, with very little fluid; no bad smell—the taste of it was overpowered by the solution; this gave no relief; I continued taking more of the solution, warm, till another discharge was obtained, which was large and nearly all fluid; in it there was a tough substance like cob webs, or the thing that grows in vinegar, feeling my stomach easy, and by the large quantity thrown off, I believed it was entirely empty, three quarters past four o'clock.

Behold, in four or five minutes time I felt a full and perfect cure.

1st, Freedom and liveliness of the legs which were stiff.

2d, A general glow of warmth everywhere, and the cramps fled.

3d, Cheerfulness of spirit, and agility of body.

4th, The fancy that an inch wide augur hole was bored through my head from ear to ear, was gone.

At 5 o'clock, finding all well, I took half a gill of mucilage, to heal and comfort the inner surface of the stomach; went to bed and slept; had my breakfast at eight, and did duty at the telegraph.

The flux of the bowels stopt for thirty hours, when it became of a proper consistence. Thanks be to God.

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Many, if not all your readers, may consider a communication of this nature from a non-professional and, in some respects, ignorant man, as ill-timed, and it may be they are right, but his plan of treatment,—though not with the same avowed object in view as Dr. S———, viz., the restoration of the saline particles to the blood,—is still in its result so much in accordance with that principle, and vouching, as I can do, for the truth of the old man's story, I could not resist its insertion here, principally, however, as I have said, as being so appropriate to the theory of Dr. S———; and let no one despise the graphic but quaint and homely language in which it is written, so as to overlook the important pathological fact it seems to teach.

There are six cases detailed in the manuscript; the first, a man in the service of the telegraph station, and was under the care of the surgeon quartered in the citadel, died; the other five—two men, himself, and his son and daughter, of ten and fifteen years, were subjected to the same plan of treatment, and all recovered. I have selected his own case, and it is but fair towards him to state that he tried the experiment on himself first. There can be no doubt, I fancy, but that he had the disease, cholera, and well marked too.

Dr. S———'s theory and example of the addition of salt to the dark crassamentum, appears founded in error, and he loses sight perhaps of the reason why the venous blood is dark in the veins, which we are taught arises from a very different cause than the one he insists on, but it is hardly possible he could have been ignorant of this fact, though in pursuit of a theory he may have been unmindful of it.

OF REMEDIES.

The first in order, were emetics, and those in most frequent use were common salt with an addition of mustard, particularly if there was much exhaustion, or ipecacuanha combined with soda, 3ss of ipecacuanha and 3j of sodæ carbon, the object being to free the stomach from crude and undigested substances. Salt emetics were latterly abandoned altogether, as they left a sensation of coldness and uneasiness in the stomach, whereas the mustard was complained of as aggravating a sense of heat and uneasiness, which all complained of more or less. Tarter emetic was never used at all, with this indication. The ipecacuanha and soda remained in use.

Blood-letting was much practiced where the state of the circulation and symptoms admitted its employment. Immediately after the emetic, relief was often marked and satisfactory, though only a few ounces of blood were lost. The stomach became less irritable; discharges of rice-like fluid from the bowels less frequent and copious, and the dreadful sense of oppression about the chest and "heart," removed or relieved. It was said by some writers that the blood should be allowed to flow, till it was no longer dark; this was never made a rule; on the contrary, the blood was not uniformly dark, but in many instances so fluid as to be mistaken for arterial blood. In mild cases it was practised so late as the second or third day, if griping pains and tenderness of the belly were complained of, and the state of the pulse permitted its employment; in some instances the blood was buffy, and in two or three cupped also, but neither of these appearances were frequent; but these encouraging results of blood-letting were only obtained early. In a state bordering on, or in collapse, the blood obtained was treacle-looking,—and the quantity obtained, merely the contents of the superficial veins.

Enemas.—These remedies were of much use, as much from their imparting a comfortable sense of warmth, as from their stimulating and anodyne effects. Large injections of very warm water alone, aided by external pressure to prevent escape, were very serviceable. If there was suspected to be a collection of fæculent or other matters, this was preceded by an enema of gruel, and 3j of antim. tartar.; in protracted cases, enemas of a mixture of castor oil and spt. terebinth, of each two ℥ss. in gruel, were found useful, imparting at the same time internal warmth, and relieving spasmodic griping. Sometimes it was of no apparent benefit; indeed the same may be said of most, if not all, remedial measures. The tobacco infusion was also used once or twice, but without benefit.

Blisters—Used in cases where there was beginning symptoms of depression of the nervous energy, or the circulation losing its force, applied to the spine from the occiput to the sacrum. It is not possible to speak of the efficacy of this practice in those who recovered, if there was time afforded for their peculiar action; they probably assisted in rousing the flagging powers of life. Their

action was too slow for urgent cases; their use was more apparent when applied the third or fourth day, to relieve tenderness at the epigastrium. They were recommended to arrest spasm of the extremities, being applied in a circular form around the thigh or leg, it was thought with some benefit; of course they were applied to the nape later in the disease, when there was a tendency to coma.

Stimulants—Were at first very freely used in the collapsed stage, but it is extremely doubtful if they were of the least service, and they were even considered injurious; they had no effect in restoring the pulse, or heat of surface, the great object in their employment. In the after treatment, indeed, combined with some light nourishment, in small quantities—and very cautiously given, they were of service, and good brandy was the best form of administration; it formed an agreeable addition to sago or arrow root, and disposed the debilitated stomach to retain and digest it. There was no end to the various forms of stimulants recommended on all sides, and of the most active kind,—ammonia, cloves, cayenne pepper, camphor, and all the more powerful essential oils, cajeput, peppermint, carraways, &c., &c.

Opium—Was considered, and indeed found, a remedy of very doubtful character, and requiring the greatest caution in its employment; it was never prescribed alone; I allude to it, of course, in its solid form. The tincture of opium was constantly employed in combination, but cautiously, it seemed to increase the tendency to coma; interfered with the action of the biliary organs, as it is well known to do; and all are aware how anxiously the restored action of the liver and the appearance of bile and urine was looked for, as the most favourable symptom to the sufferer, and giving, as it did, a promise of recovery, though often under otherwise desperate circumstances.

External Applications.—Tin filled with hot water, the length and breadth of the hospital bedstead, with folded blankets over it, formed the bed of the more severe cases. Friction, with spirits of turpentine, to the parts affected with spasm. Mustard cataplasms to the epigastrium, chest and extremities; flannel, moistened with spirit of turpentine, to the spine. The warm bath, either with or without the addition of salt, at a temperature as high as could be borne; but this remedy fell into disuse; it distressed the sick; moving often favoured the return of spasm; they themselves were averse to it, to say nothing of the risk of raising the body in collapsed cases from a horizontal to an erect posture. Calomel seemed to stand first in the list of internal remedies, it was a question as to quantity—whether alone or in combination; some were advocates for its sedative effect in 3ss and 3j doses—others in small or one grain doses, combined with one-eighth of a grain of solid opium; and I think, where there was time, the smaller dose became the settled practice. I may be allowed to say, that I have seen beneficial effects from both modes of exhibition; it certainly, in some cases, appeared to have a marked sedative effect in the larger, and was entitled to great confidence in the smaller; it was sometimes the practice to give intermediate doses of soda, which was said to favour its retention and ensure its action; however, this might have been, if life was prolonged, so that the usual effects of mercury on the system could be detected, it was welcomed, and not without ample reason, as a most favourable event to the patient.

I commenced these observations by stating that I would hazard no comments, noticing only the remedies used in the Military Cholera Hospital, but I could not well avoid explanation when it seemed necessary. I have not given either the names or the result of all the remedies employed, but I have made a selection of those most used; the remainder are so various, and were found on trial, so useless, that I found myself unwilling to occupy the necessary space in your journal for their detail.

I shall, therefore, in conclusion, proceed to give one of the cases of "transfusion;" it was the first trial, and failed, it was believed, because the process was faulty or the patient in unfavourable circumstances, but the effects were so marked that they encouraged a second, third, and fourth trial, but, sad to say, they all failed, because, perhaps, it was not resorted to, till all other human means were hopeless.

Ingredients for transfusion:—

17 Pints of soft rain-water, carefully filtered.
180 Grains of Muriate of Soda.
204 Do of Carbonate of Soda.
204 Do of Phosphate of Soda.

Kept at a warmth of 90 degrees of Fahrenheit. The pipe was carefully introduced into the Median Basilic vein of the right arm. At this time the patient, private James Williams, of the 24th Regt., was insensible, or roused with great difficulty; his face, hands and feet were deep blue; body tolerably warm, but covered with cold sweat; tongue and breath cold; no pulse at the wrist or arms, and but feeble and slow at the carotids; the eyelids half closed, and the eyes turned upwards; spasms had long since ceased, as well as the vomiting and discharges by stool; in less than ten minutes after the first introduction of the saline fluid, and while the operation was proceeding very slowly, the blue colour completely left his feet and hands; his breathing became quickened; the florid and natural appearance of his face returned; he opened his eyes, and expressed his sense of relief; about twenty-five minutes after the commencement of the transfusion, when about eight pints of the fluid had been introduced, he suddenly vomited a good deal of frothy bilious-looking fluid, he sank rapidly after this, and died in a few minutes.

I have said nothing of the consecutive fever of cholera; we had but few cases of it, but as they required the same management as febrile affections usually do, and these are treated on known principles, it seems unnecessary to do so.

With respect to the promise I made in the early part of this communication, that I would show that the disease was as fatal in India as on its first appearance, and as unmanageable; I submit the following particulars:

"The entrenched camp at Currachee, at the mouth of the Indus, and on its Western bank, in July, 1846, consisted of four Queen's and three Native regiments, with a proportion of cavalry and artillery,—in all seven thousand men, exclusive of women, children and camp

followers, the whole under the command of Sir Charles Napier; the British regiments were in brick barracks, the rest halted as usual."

"The troops while in church, on Sunday, the 15th July, were sensible of the sudden effect of a violent gust of wind, accompanied by clouds of dust and sand, and imparting a chilly feel to all, which, however, soon passed away; camp perfectly healthy at the time. On the march home from church, several men of the 86th Regt., were attacked with cholera; before that night 120 men had died of it—and it destroyed three thousand men, besides an amazing number of women, children and camp followers, before the following Sunday, after which day *not a single case occurred.*"

"Many, indeed the majority, died without any constitutional derangement whatever, but "went out," as you speak of a candle; *some* had vomiting, *some* had purging, *some* had both, but the majority collapsed, and died in five or six hours, from a previous state of apparent health: up to Thursday, from the Sunday, in that eventful week, it attacked the soldiers *alone*, then it left them, and was fatal to the women and children, and *last* of all, to the officers, each class being perfectly unaffected in the interval."

"So dreadful was the panic, that many of the men fled into the desert, and were never seen again; all who could obtain the means got drunk. The officers were obliged to act as orderlies in the hospital, the General setting the example; none could be got to inter the dead."

This dreadful scene can hardly be realized by description, though related by an officer present.

Will any man be found bold enough to assert, after the perusal of this sad history, that the malignant, as well as the mysterious nature of this extraordinary disease is altered by time. Facts upon facts may be stated in support of its capricious and unaccountable appearance. At Halifax the 1st Batt. Rifle Brigade, and the 34th Regt., of which I was the surgeon, occupied the same barracks; the buildings were all connected together somewhat in a circular form, and the men had free intercourse, the one regiment with the other; while the Rifles were attacked by cholera of a malignant form, the 34th Regt., enjoyed a *complete* immunity.

The Rifles were marched to the head of Beaufort Basin and encamped, and the disease left them at once; did not decline, but, as in the instance of the removal of the15th from Montreal to St. Helens, in 1832, ceased.

Much confidence has been restored and satisfaction felt, by the proceedings of the authorities at home, in the removal of quarantine restrictions; but while its epidemic character, and the ease and rapidity with which it travels, set at defiance all precautions of that nature, as in the instances on the Continent of Europe, where the strictest military cordons were found useless, no prudent man, on the strength of that proceeding, would pronounce either on its non-contagious principle or its ameliorated character. The College of Physicians, by a late manifesto, abstain from expressing any decided opinion. Contagious to-day, non-contagious to-morrow, by favouring circumstances, either of atmosphere or terrestrial exhalations, localities or predisposition.

Esplanade, Quebec, December, 1848.

Art. LXXVII.—CASES OF GUNSHOT WOUNDS OCCURRING IN THE MONTH OF JUNE, IN PARIS. No. III.

By George D. Gibb, M. D.
Licentiate of the Royal College of Surgeons, Ireland.

On this occasion I enclose eighty-five cases of gunshot wounds, of the upper and lower extremities, and which will form the conclusion of my subject. They present a very great variety in their nature, almost every part indeed, having been implicated. Many of those, where the joints were involved, have terminated in a most successful manner, some not even being followed by anchylosis, as shown in cases 98 and 99, where the balls traversed the articulation of the knee.

I must here state, that I have detailed some cases which have ended fatally; it would not be doing justice to report all the good and leave out the bad; I have considered this so important in relation to fractures of the thigh, that I have omitted none I had the notes of. The results of the treatment in cases of fractured thigh tend further to establish the doctrines already laid down by the majority of British Surgeons, namely, to perform primary amputation in cases whether of simple or comminuted fracture, as well as in those of a more severe and complicated form.

The experience of the Parisian Surgeons, from the events of February and June, has not advanced our knowledge of Military Surgery a great deal; a perusal of the Reports on gunshot wounds before the Academy of Medicine will clearly demonstrate the truth of this. Instead of amputating in all cases of fracture of the thigh, they have tried to save the limbs in a very great number, and the result has proved most disastrous; and even, when a secondary amputation has been performed, the patient almost invariably has succumbed.

Out of twelve cases which I have given, only six were saved, and they form the entire number of cures throughout the Hospitals in Paris, while many more indeed may be added to the number of deaths. Many cases, also, which were related as cures before the Academy, have since died.

Of the successful cases, the wounded limbs in all were much shorter than the second, and added to deformity in two or three, they cannot be considered as useful members to the patient. The termination of cases 77 and 84 is as yet doubtful. The case of disarticulation at the hip joint, No. 78, is the only successful one out of four performed in the month of June.

Four cases of wound of the knee joint were to be seen in the Hospital St. Louis; two terminated without any bad consequence whatever; one with anchylosis; and the fourth died.

SUPERIOR EXTREMITY.

No. 39. A case in the *Hotel Dieu*, under M. Blandin, where a ball had passed through the shoulder joint from behind, forwards. Inflammation followed, with suppuration, and the case has terminated with complete anchylosis.

40. A case in the *Hotel Dieu*, under M. Roux, where a ball had passed through the right arm, immediately below the shoulder, fracturing the humerus, and emerging outwards in front of the chest. Union of the

fractured bone has followed, and the patient was discharged well some weeks ago.

41. A case in the *Hotel Dieu*, under M. Boyer, where a ball had passed through the left axilla from before, backwards. No bad consequences ensued, the wounds quickly healed, and by the end of August the patient was well.

42. A case in the *Hotel Dieu*, also under M. Boyer; and very similar to the preceding. A ball had entered to the inner side of the right shoulder from behind, between the scapula and arm, and passed outwards through the pectoralis major, over the third rib, four inches from the sternum. The patient was discharged well in September.

43. A case in *St. Louis*, under M. Jobert, of fracture of the upper part of the left humerus, from a ball, involving the shoulder joint. Primary amputation at the joint was performed after the injury, which was not followed by any bad consequences, and by the 15th Aug. the cicatrix of the stump had completely healed.

44. A case in *La Charité*, under M. Velpeau. A ball had entered in front through the right pectoralis major muscle, passing through the axilla, and escaping behind near the inferior border of the scapula. Discharged well in September.

45. A case in the *Val de Grace*, under M. Baudens. An officer had his left arm shattered by a ball, below the shoulder joint. Primary amputation was performed through the surgical neck of the humerus. This case has been doing well, but a speedy cure was retarded from the patient's having an impaired constitution.

46. A case in *St. Louis*, under M. Jobert, of wound of the arm, with compound fracture near the shoulder, from a ball, necessitating primary amputation at the joint. The patient was doing well for some weeks afterward, but succumbed from purulent absorption.

47. A case in *La Charité*, under M. Gerdy, of wound of the left arm from a ball, near the shoulder; the Surgeon stated that the brachial plexus was wounded here, as the patient had contraction of the little and ring fingers.

48. A case in *St. Louis*, under M. Jobert, of comminuted fracture of the right arm from a ball, which had entered its centre from the front, and escaping three inches below the shoulder. The bones had united by the 15th of August, but at that time a large quantity of callus remained unabsorbed. He was discharged from the Hospital in September. None of the comminuted fragments of bone were removed in this case.

49. A case in *La Pitié*, under M. Michon. A ball had entered the right arm of a girl, aged 20, at its inferior third, fracturing the humerus in its course backwards and outwards. The wounds have been healed for some time, but union of the fracture has not yet occurred, the two ends of the broken bone being quite moveable.

50. A case in *St. Antoine*, under M. Nélaton. A ball had entered the anterior surface of the upper third of the right arm, passing obliquely downwards and backwards, and escaping at the middle of its inner side, in its course severing the brachial artery and median nerve. This has been followed by loss of sensation in the hand and lower part of the forearm. When the patient coughs, or laughs, or talks loudly, he is immediately seized with a darting pain in the arm, along the course of the wound, which is aggravated on expiration.

51. A case in *La Charité*, under M. Velpeau. A ball had passed across the anterior surface of the elbow towards its inner side, and not injuring the joint. The patient also had a wound of the left hand, rendering amputation of third and fourth fingers necessary; and furthermore he had a severe contusion over the upper part of the left side of the thorax. He was discharged well in August.

52. A case in *La Pitié*, under M. Michon, where a ball had entered at the external part of the left elbow joint, fracturing the external condyle of the humerus, passing posteriorly, wounding the triceps and neighbouring muscles, and escaping at the internal surface of the arm, about four inches from the elbow. Inflammation followed, with suppuration and immense swelling. A few weeks after, some of the ligaments of the joint were sloughing and protruding from the lower wound. Several small splinters of bone were removed, and by the 18th October the wounds had healed, and the case terminated in anchylosis, the forearm being permanently pronated.

53. A case in the *Hopital des Cliniques*, under M. Geraldis, of a compound fracture of the left elbow joint, from a ball, in a lad aged 17. This case was in such a bad state when I first saw it, from the serious implication of the joint, attended with inflammation and other bad consequences, that I considered the lad's life sacrificed, by not having resorted to amputation. However, with the persevering application of cold water from a vessel elevated above the arm, and allowed to pour over it drop by drop, together with the removal of numerous splinters of bone, and other treatment, he was cured by the end of October, with anchylosis.

54. A case in *La Pitié*, under M. Michon. A ball had entered the right arm, immediately above the elbow joint from behind, fracturing the ulna in its course downwards and outwards, and involving the joint. Enormous swelling followed with inflammation and suppuration; several splinters of bone were extracted; and now the case is nearly well, but still some œdematous swelling of the forearm remains. The patient can flex the arm in a very trifling degree, with the aid of his left hand.

55. A case in the *Val de Grace*, under M. Baudens, of an officer who had been wounded in the right forearm, the ball having entered through the muscles at the posterior and upper part of the radius, passing forwards and downwards, and escaping three inches above the wrist on the anterior surface of the radius. This wound was followed by inflammation and suppuration, pus burrowing between the muscles at the posterior part of the forearm to a great extent, requiring numerous incisions. He was, however, nearly well by the end of September.

56. A case in *La Pitié*, under M. Michon, where a ball had entered the anterior surface of the right forearm, above the wrist, at the internal border of the ulna, passing through the muscles obliquely upwards, and escaping near the external edge of the upper third of

the radius. None of the blood vessels were wounded, and the case speedily did well.

57. A case in the *Hotel Dieu*, under, M. Roux. A ball had entered the anterior surface of the left forearm, near its radical border, passing obliquely upwards, and escaping near the external part of the elbow joint; the joint was safe, but extensive suppuration followed, extending the whole length of the trajet of the ball, which had to be slit up with a bistoury. It terminated favourably, but with some stiffness during the act of pronation and supination.

58. A case in the *Hotel Dieu*, under M. Blandin. A ball had passed across the dorsal surfaces of the lower ends of the bones of the right forearm, fracturing them both. Very great swelling followed, with inflammation, and afterwards extensive suppuration, requiring numerous incisions to evacuate pus, and the case looked as if secondary amputation would have to be resorted to. A splint was applied along the anterior part of the forearm; greased charpie was used as a dressing to the wounds, and after the lapse of twelve weeks the bones had united. Now the case is nearly well, with little or no deformity.

59. A case in *St. Louis*, under M. Malgaigne. A ball had entered the left forearm from behind, fracturing the shaft of the ulna, and escaping in front near the elbow. This case did well, without any accident; a small sinus communicated with the fracture in front; it was slit up, and two splinters of bone extracted, and the wound quickly healed.

60. A case in the *Hotel Dieu*, under M. Blandin, of comminuted fracture of the right forearm, from a ball, which had entered posteriorly near the elbow, and escaping in front three inches above the wrist. Some fragments of bone have been removed, and the fracture has united, but with great deformity.

61. A case in *La Pitié*, under M. Michon. A ball had entered the upper third of the inner side of the right forearm, over the ulna, where it had become lodged, not producing any fracture. Two several attempts were made to extract it, but it could not be found, and it was presumed it must have tumbled out before the patient was brought into the hospital. Secondary hœmorrhage occurred eight days after, which was arrested by pressure. It again occurred on two occasions, requiring on the fifteenth day ligature of the brachial artery, which was performed at the lower third of the arm. Now the arm is nearly healed, and the patient can flex the forearm with perfect ease, but cannot supinate it.

62. A case in *St. Louis*, under M. Jobert, of severe wound of the left wrist joint from a ball, in a female, which was followed by sloughing and suppuration, leaving a deep fissure extending almost through the carpus, and presenting the appearance as if inflicted by a sabre. The ulcerated surface was covered with numerous and large flabby granulations. Secondary amputation was resolved upon at the end of July, but the operation was delayed, and finally not performed; the wound has since become partially healed, and at this period the patient is in a very precarious state of health, suffering evidently from hectic fever.

63. A case in the *Hotel Dieu*, under M. Blandin, of compound fracture of the wrist and forearm, from a ball. Extensive suppuration followed, with sloughing of the integuments or the posterior part of the forearm, leaving the muscles quite bare. A splint was applied along the palmar aspect of the arm; and, although the case did appear to be a very bad one, it has done very well; union of the fracture having occurred without any deformity, and the wounds are nearly healed.

64. A case in the *Hotel Dieu*, also under M. Blandin. A ball had traversed the wrist joint of right hand, entering at its ulnar side and fracturing the lower portion of the radius. The hand was very much swelled, but the bones have united and no bad consequences have resulted, excepting some stiffness in the joint itself.

65. A case in the *Hotel Dieu*, also under M. Blandin, where one ball had passed completely through the centre of the wrist, and another through the index finger of same hand. The finger was amputated, and the cicatrix resulting was a long time healing, from the irritation caused by the wound through the wrist. The latter wound has healed after the removal of some fragments of the bones of the carpus.

66. A case in the *Clinique*, under M. Geraldis, where a ball had entered the anterior surface of the left forearm just above the wrist, and remaining lodged in the hand. Enormous swelling of the lower part of the forearm and hand ensued, which was followed by suppuration and the formation of an abscess in the palm, which was opened. The ball was not extracted until some weeks afterwards, and then the wounds began to heal, and the case speedily did well.

67. A case in the *Val de Grace*, under M. Baudens, of wound from a ball in the centre of the back of the left hand, not completely through. The patient did not know whether the ball had fallen out or was still lodged. The Surgeon stated he had removed the fractured ends of three of the metacarpal bones, and on examining the wound on 22nd July, he thought he could discover the ball. Chloroform was administered; the wound was dilated, and after probing it, an irregularly flattened ball was extracted, which, said the Surgeon, must have struck some piece of iron before entering the hand. A catheter-shaped director was then introduced into the wound, and cut down upon from the palmar surface for upwards of an inch and a half, and a seton passed through the opening. Ice was then ordered locally. From that date the case progressed most favourably, and the hand was quite healed in the month of October.

68. A case in *La Pitié*, under M. Michon. A ball had passed through the centre of the back of the hand, fracturing the metacarpal bone of middle finger, and making its exit in front of the hand near the melacarpo phalangeal joint of same finger. Suppuration followed, but the hand was very quickly cured.

69. A case in *St. Louis*, under M. Jobert. A ball had passed through the right hand, shattering the metacarpal bones of the fore and middle fingers. The wound resulting was frightful, a large hole existing through the hand. By the 15th August the wound was closed, and the hand was nearly well, but, from the loss of substance, the index finger was much shorter, and the hand slightly deformed.

70. A case in *St. Louis*, also under M. Jobert. A ball had entered the dorsal surface of right hand, near the thumb, passing across and escaping outwards near the ulnar edge of the metacarpal bone of little finger. A great deal of swelling followed, with suppuration, but the cure was effected by the middle of August.

71. A case in *St. Louis*, also under M. Jobert. A ball had struck the joint of the middle finger, producing a fracture with a large bulging mass of flesh around the wound. The finger was saved.

72. A case in the *Hotel Dieu*, under M. Blandin. A ball had entered the palm of the left hand, over the fourth metacarpal bone, and passing out through the first phalanx of fourth finger or its dorsal surface, fracturing it, as well as the melacarpo phalangeal joint. It did well.

INFERIOR EXTREMITY.

73. A case in *St. Louis*, under M. Jobert. A ball had entered the middle of the right thigh from before, backwards, fracturing the femur in its course outwards. The fracture has united, and the leg is two centimetres shorter than the left. No splinters were extracted; an abscess, formed at the inner part of the thigh, which has healed, and excepting very little suppuration from the anterior wound, the case is considered cured. The patient has lately been getting baths, and his health is perfectly good.

74. A case in *St. Louis*, also under M. Jobert, of fracture of the right thigh in a boy aged 15, the ball having entered at its inner side high up, taking a direction obliquely backwards to the right side, fracturing the shaft of the femur in its course. Suppuration to some extent followed; no splinters of bone were extracted; and the union of the fractured bones is now perfect, but with shortening of the leg a little over two centimetres.

75. A case in *St. Louis*, under M. Malgaigne, of fracture of the right thigh from a ball, in an elderly man. The union of the bones progressed most favourably from the commencement, permitting of his discharge from the Hospital, cured, before the 24th October. The fractured leg was shorter than the other.

76. A case in *St. Louis*, also under M. Malgaigne, of simple fracture of the right thigh from a ball which had entered in front. No bad consequences whatever followed; the bones have united, but with curved deformity, the convexity being outwards; no splinters were extracted, and the limb is three centimetres shorter than the other. The patient is now perfectly well.

77. A case in *La Charité*, under M. Velpeau, where a ball had passed through the left thigh from before, backwards, fracturing the femur in its course. Union of the fractured bones has occurred with deformity, and the limb is nearly five centimetres shorter than the other. At this period the anterior wound still discharges pus, but the man's general health is good.

78. A case in *St. Antoine*, under M. Nélaton, where a ball had extensively wounded the right hip joint, fracturing the femur, requiring primary amputation, which was performed by the Surgeon above named, at the articulation. By the 4th August, the wound of the flaps (which was then about five inches long and one and three quarters wide) was doing as well as could be desired, and there was very little suppuration indeed. The patient has continued to progress favourably, excepting a period of relapse brought on by the *friendly* visits of the Police, from his being an insurgent, and now the cicatrix has nearly healed. A small fistula exists, from which exude a few drops of pus. His general health is delicate, but he will shortly be removed to another hospital for change of air.

79. A case in the *Val de Grace*, under M. Baudens, in the person of General Demesne, who had his left thigh fractured from a ball, for which primary amputation was performed by the flap operation. He progressed favourably till the latter end of July, when he was so far well, the stump being healed, as to be allowed to descend into the gardens of the hospital for the benefit of fresh air. He became suddenly ill, and died within two or three days, from purulent absorption.

80. A case in *La Charité*, under M. Gerdy. A ball had passed through the left thigh above the knee, fracturing the femur. Union of the bones appeared to be going on favourably for the first ten weeks, but the patient succumbed in the middle of September.

81. A case in *St. Louis*, under M. Malgaigne, of comminuted fracture of the left thigh from a ball, in a boy. On the 8th August the case promised well, after very extensive suppuration and the removal of a number of fragments of the broken bone; the leg was considerably shorter than the other, and a fistula had formed in front, from which existed a discharge. A large amount of callus had been thrown out, which was not absorbed. M. Malgaigne pronounced a very favourable prognosis at that time, but afterwards the boy's health began to decline, the wound commenced suppurating anew, and the poor fellow succumbed by the end of September.

82. A case in *St. Louis*, also under M. Malgaigne, of fracture of the left thigh from a ball, the patient, a Corporal of the 13th Regiment of the Line. Secondary amputation was performed on the 5th August, but death followed on the 11th of the same month.

83. A case in the *Hotel Dieu*, under M. Boyer, of fracture of the left thigh at its inferior third, from a ball which had become lodged and afterwards extracted. By the 14th August the wound had nearly healed, and the case promised well—union apparently going on well. The patient shared the same fate as the preceding a few weeks afterwards.

84. A case in *La Charité*, under M. Velpeau, of fracture of the thigh from a ball, together with a flesh wound of the leg from a second ball. The patient was doing very well, but left the hospital before he was cured.

85. A case in the *Val de Grace*, under M. Baudens, where a ball had entered the left thigh, fracturing the great trochanter and ilium, and remaining lodged. It was extracted, as well as some portions of the fractured bone, and the case has done well, with some suppuration at its commencement.

86. A case in the *Val de Grace*, also under M. Baudens, where a ball had struck the right femur, becoming flattened around it, and not producing any fracture.

Very great suppuration followed, requiring numerous incisions to let out matter; a portion of the bone exfoliated, and the case was cured before the lapse of many weeks.

87. A case in the *Hotel Dieu*, under M. Blandin, where a ball had entered the left groin, over the course of the femoral artery, and passing outwards anterior to the ischium. The wound was by suppuration, but was very quickly healed without any accident happening.

88. A case in *St. Louis*, under M. Jobert, of a severe wound in the groin of a boy, from a ball which had become lodged in the hip. Extensive suppuration followed, with irritative fever, which carried off the patient about the eighth week.

89. A case in the *Val de Grace*, under M. Baudens, of a wound at the inner side of the left thigh, from a ball. Secondary hæmorrhage occurred on the 1st August, requiring ligature of the external iliac artery. No bad symptoms followed, but he remained very weak for upwards of a month and a half, and now he is nearly well.

90. A case in *St. Louis*, under M. Jobert, where a ball had passed through the fleshy part of both thighs, posteriorly from right to left. Cured after some suppuration.

91. A case in the *Val de Grace*, under M. Baudens. A ball had entered the outer side of right thigh, just above the knee, taking a course obliquely upwards and inwards, and escaping at the inner part of the thigh of same leg. Cured.

92. A case in the *Hotel Dieu*, under M. Blandin, where a ball had entered the inner side of the left thigh posteriorly, four inches above the knee, passing forwards and downwards, and escaping just above the external condyle of the femur. Synovitis followed on 26th July, which was combatted, and a cure speedily resulted.

93. A case in *La Charité*, under M. Gerdy, where a ball had passed through the tendons of the inner hamstring muscles of left leg. The wound quickly healed, but was followed by very slight contraction of the leg.

94. A case in the *Hotel Dieu*, under M. Boyer, where a ball had entered the left thigh in front, passing backwards and upwards, and escaping near the ischium of same side. Cured.

95. A case in the *Beaujon*, under M. Robert, where a ball had entered the inner side of the right thigh, near the knee, wounding the inner hamstring muscles, and passing out posteriorly about four inches above the knee. The wounds quickly healed, with permanent contraction of the tendons, to be shortly remedied by sub-cutaneous section.

96. A case in the *Hotel Dieu*, under M. Roux, of a wound just above the right knee joint, from a ball, which was followed by inflammation and suppuration, and subsequently involving the joint itself. On the 19th July M. Roux amputated the man's thigh by the circular operation. He began to sink on the third day, and died on the ninth day of the operation.

97. A case in *St. Louis*, under M. Jobert, where the knee joint became subsequently involved from a wound immediately above. Amputation of the thigh was performed on the 4th August, under the influence of chloroform. Suppuration of the stump followed, which has continued most freely up to this time, the flaps having shown no disposition to unite even by granulation, and the patient is in a very precarious state.

98. A case in *St. Louis*, also under M. Jobert. A ball had entered the front of the knee joint, at the side of the patella, completely traversing its cavity, and escaping outwards posteriorly. This case was perfectly cured without anchylosis, or any bad consequence whatever.

99. A case in *St. Louis*, also under M. Jobert, very similar to that previously described, where a ball had entered the knee joint from behind, escaping lower down and nearer the edge of the patella than in the preceding case, and appearing, from the wound in front, as if it had passed through that bone. This patient was also cured without anchylosis. The leg was in a bent position when the ball entered the knee.

100. A case in *St. Louis*, also under M. Jobert; where a ball had entered the left knee joint, to the right of the inferior border of the patella, and remaining lodged within. Synonitis followed, with the formation of an abscess an inch and a half lower down than the wound, which permitted of the extraction of the ball out of the joint. The patient's wound is now perfectly healed, but he has anchylosis remaining.

101. A case in *St. Louis*, under M. Malgaigne, where a ball had entered the left knee joint in front, above the patella, and remaining lodged within it. M. M. stated that he was not aware whether there was any fracture here, and he did not like to explore the joint for the ball, as his doing so would produce more mischief than its presence. Synovitis followed, and a succession of abscesses formed around the joint, which ultimately carried off the patient in September.

102. A case in *La Beaujon*, under M. Robert. A ball had passed completely through the head of the tibia from before, backwards. Inflammation of the knee joint supervened, followed afterwards by suppuration. The Surgeon wished to amputate above the knee, but the patient would not submit to it, and died on the 6th August.

Autopsy. The ball was found to have entered a little to one side of the anterior tuberosity of the tibia, passing directly backwards and outwards; its trajet communicated with the knee joint. The cavity of the joint contained a quantity of greenish pus, and its structures were completely disorganized. The articulating surfaces of the tibia, femur, and patella, were entirely denuded of fibro-cartilage, and were rough and irregular. Pus extended upwards between the muscles of the thigh to within three inches of the hip joint.

103. A case in *La Charité*, under M. Velpeau, where one ball had entered above and another below the inner side of the knee, and not involving the joint. The patient is cured.

104. A case in *La Charité*, under M. Gerdy, where a ball had entered to the left of the right ligamentum patellæ, passing over it, and then outwards near the right border of the patella, without wounding the joint. Case cured.

105. A case in *La Pitié*, under M. Michon. A ball had struck the inner surface of the left leg, fracturing the

tibia and passing outwards back of the fibula. The fracture has united with some deformity, and the patient is now quite well.

106. A case in *St. Louis*, under M. Malgaigne, of comminuted fracture of left leg, from a ball which had passed through the middle of the tibia from within, outwards. Inflammation followed, with extensive suppuration, intersecting the muscles of the leg, requiring numerous incisions to let out matter. As many as sixteen splinters of bone were extracted at different times. The fracture has now united, and, with the exception of a little suppuration, the case promises to do well.

107. A case in *St. Louis*, also under M. Malgaigne, where a ball had struck the inner side of the right tibia anterior to the malleolus, not entering but fracturing the bone below. The patient is now well.

108. A case in the *Hotel Dieu*, under M. Blandin, of compound fracture of the middle of the left leg, from a musket ball. Consecutive amputation was performed about the ninth week through the articulation of the knee. For some weeks afterward the patient appeared to be doing very well, the stump, although suppurating, healing kindly. Latterly he has not done so well, the stump having commenced to suppurate a good deal; he has been sent from a crowded ward to the Val de Grace, where he will receive the benefit of fresh air.

109. A case in the *Hotel Dieu*, under M. Roux, of comminuted fracture of the leg from a ball. Very extensive suppuration followed, with the formation of a large abscess above the knee joint at the lower third of the thigh, and which subsequently involved the joint itself. Amputation by the circular method was performed on the 14th August. The patient progressed very favourably, the stump being now healed, and his general health is good.

110. A case in *St. Louis*, under M. Jobert. A ball had passed completely through the upper third of the tibia, escaping behind, and not producing any fracture. Extensive suppuration followed. Numerous incisions were made to let out matter, permitting also of the removal of several sloughs of fascia. The patient ultimately did very well.

111. A case in *La Charité*, under M. Gerdy. A ball had entered the inner side of the left leg, passing through the upper third of the tibia without fracturing it, and escaping in front of the fibula; this opening was clearly demonstrated with a probe, the end of which was made to encircle it. Suppuration followed, but the cure was much more rapid than in the preceding case.

112. A case in *La Pitie*, under M. Michon. A ball had entered the middle of the left leg to the outer side of the tibia, passing backwards through the interosseous space, and escaping outwards posteriorly; erysipelas followed, which extended down to the foot, with the formation of a large abscess over its dorsum. At the same time this patient had received another wound on the right leg, a ball having entered the upper fourth of the tibia from left to right, forming a channel in the bone in its course outwards, and not producing any fracture. Both legs are now perfectly healed, but some tenderness exists over the cicatrices of the wounds.

113. A case in the *Val de Grace*, under M. Baudens, of fracture of the left fibula, near the ankle, from a ball. The foot was everted outwards, which was remedied by a splint along the inner side of the leg. Union followed without any deformity.

114. A case in *La Pitié*, under M. Michon. A ball had entered in front of the posterior layer of the muscles of the calf, near the upper third of the fibula, fracturing that bone in its course, and escaping near the inferior fourth of the leg at its inner side. Most extensive suppuration followed, requiring large incisions to let out matter, and the case looked very bad. The wounds, however, are now healed; the fracture has perfectly united, and some tenderness exists over the superior wound, from which numerous small splinters of bone have been extracted.

115. A case in the *Beaujon*, under M. Robert, of fracture of the left fibula, a ball having passed through it near its head, then backwards through the muscles of the calf, and escaping near the inner side of the leg. Suppuration followed, requiring several openings to give exit to matter. The fracture has united, and the patient was sufficiently well to be discharged from the hospital in September.

116 and 117. Two cases in the *Val de Grace*, under M. Baudens, of wounds of the leg without fracture, where phlegmonous erysipelas followed in both. They were treated with lumps of ice placed over them, and terminated favourably after the lapse of some weeks, both patients being cured.

118. A case in the *Hotel Dieu*, under M. Roux. A ball had entered the inner side of the middle of the leg, near the tibia, passing downwards and escaping at the ankle. The integument over the trajet of the ball sloughed, leaving a large wound as if inflicted by a sabre. It was a very long time healing.

119. A case in *St. Louis*, under M. Malgaigne, where a ball had passed through the left ankle joint from within, outwards. Inflammation of the joint followed, with suppuration, requiring, as usual, numerous incisions to let out matter. The patient is cured, but with anchylosis.

120. A case in the *Val de Grace*, under M. Beaudens. A ball had entered above the left ankle, fracturing the tibia, passing through the bones of the tarsus, and escaping at the inner margin of the foot. Inflammation and suppuration followed. Cold applications were used, and the leg was placed in a wooden case with three slides, and ultimately the patient was cured, without anchylosis.

121. A case in *St. Louis*, under M. Malgaigne. A ball had completely passed through the right ankle from right to left. A tumour of an encephaloid character developed itself over the external wound, which was extirpated with the knife on 31st July, the patient being under the influence of chloroform. His wounds are now nearly healed, and his general health is good.

122. A case in *La Charité*, under M. Velpeau. A ball had entered the inner side of left foot, passing across the tarsus in front of the astragalus and cuboid bones, and remaining lodged. It was extracted on the 21st July, by an opening made at the internal plantar edge of the sole of the foot. At the same time were extract-

cd portions of fractured bone. The cure has been very tedious, but now promises well. Another piece of bone was removed on 27th October.

123. A case in *La Charité*, also under M. Velpeau. A ball had entered the left foot over the middle of the tarsus, passing obliquely downwards and backwards, and escaping at the side of the sole in front of the heel. The foot became very much swelled, and the wounds began to suppurate; the patient was sent out of the hospital cured, in October.

Paris, 11th November, 1848.

Art. LXXVIII.—MEAN TEMPERATURE OF CUMBERLAND HOUSE, AND BEDFORD HOUSE, HUDSON'S BAY TERRITORY.

By David Thompson, Provincial Surveyor.

MEAN TEMPERATURE OF CUMBERLAND HOUSE.

Latitude, 53.56 45, *W.*; *Lon.* 102.13, *W*, *for* 1789 & '90.

	Mean	Max.	Min.
October	+39	+59	+15
November	+11	+31	—11
December	+ 5	+28	—21
January	— 5	+14	—31
February	+ 2	+36	—29
March	+ 6	—30	—29
April	+25	+51	— 1
May	+50	+75	+32
June	+59	+90	+34
July	+70	+90	+54
August	+60	+84	+37
September	+48	+72	+30
Mean	+31		

During winter, every clear night, the aurora from E. S. E. to North was seen, and often in zenith in the summer also seen, but faint and distant. At this place wheat, barley, and oats, come to maturity.

MEAN TEMPERATURE OF BEDFORD HOUSE, REINDEER'S LAKE.

Lat. 57.23 *W.*; *Lon.* 102.59, *W.*, 1795 *and* '96.

	Mean	Max.	Min.
October	+ 26	+54	+15
November	+ 1.5	+45	—37
December	— 18	+30	—36
January	— 19	+25	—50
February	—16.7	+15	—49
March	— 5	+44	—43
April	+11.5	+40	—30
May—21st day	+24.5	+50	— 7

The rest of the year passed in surveying various countries. On the 6th July, the above great lake opened by a gale of wind which sunk the ice. The aurora borealis was seen the whole winter.

On the 18th of May, the first wild-goose was seen: on the 20th of May, the snow was three feet deep in the woods: the ground frozen to the depth of 3½ feet.

Montreal, January 5, 1849.

Art. LXXIX.—MEAN RESULTS OF METEOROLOGICAL OBSERVATIONS AT HAMILTON, CANADA WEST, FOR THE YEAR 1848.

By Dr. Craigie, Hamilton, C. W.

1848. Months	Thermometer 9, A. M.	Thermometer 9, P. M.	Thermometer Mean	Thermometer Highest	Thermometer Lowest	Barometer Mean	Barometer Highest	Barometer Lowest	Weather Rainy days	Weather Days with slight showers	Weather Fair days	Years Mean temperature of
January	30.9	30.4	30.65	60	—1	29.635	30.22	29.05	4	7	20	1846.....50.215
February	29.62	30.2	20.19	55	+5	594	30.07	29.00	1	5	23	1847.....48.763
March	32.53	32.45	32.5	72	+6	642	29.95	29.19	4	6	21	1848.....49.293
April	46.2	45.9	46.05	84	+26	777	30.09	29.38	2	3	23	
May	61.42	58.16	59 79	91	++35	566	29.87	30.27	3	7	21	
June	70 82	68.2	69.52	100	++42	615	29.95	29.28	1	7	22	
July	70.32	68.8	69.61	94	++51	628	29.80	29.23	1	9	21	
August	73 58	72.516	73.048	96	++53	7065	29.88	29.33	2	5	24	
September	58.9	57.3	58.1	80	++33	623	29.93	28.99	4	8	18	
October	51.096	50.77	50 933	78	++32	677	30.13	29.22	3	6	22	
November	37.2	37.13	37.166	51	++15	694	30.09	28.90	5	6	19	
December	34.41	34.13	34.27	60	+13	649	30.12	29.05	5	9	17	
Mean temperature of year			19.295			29.763	Mean height		85	80	251	Of 3 years...49.224

PRACTICE OF MEDICINE AND PATHOLOGY.

Lecture on the Nature and Treatment of Cholera, considered with reference to its Analogy with Congestive Agues of Quotidian Type. By CHARLES W. BELL, M.D., K.L.S. (Read before the Medical Staff of the Manchester Royal Infirmary, and the Members of the Medical Profession in Manchester, November 3, 1848.) Continued from page 277.—Little now remains to be said with regard to the nature of cholera, if I have succeeded in convincing you that the disease does not exist essentially in the blood, but in disordered function of the nerves and blood-vessels, and if my arguments have carried any conviction of the truth of the following points—

1st. That an attack of cholera begins, and essentially consists, in disordered capillary attraction.

2d. That congestion is the effect of this condition, and the cause of the other symptoms, and of death.

3d. That the diarrhœa is a natural mode of relief to congestion, and thereby tends to the prolongation of life, and that being an exudation, it is not controllable by means applicable to excretion.

4th. That each period of twenty-four hours, in every attack of cholera (which extends beyond that period) is marked by—1st, A period of capillary disturbance. 2d, A period of congestion. 3d, A period of comparative capillary relaxation and partial reaction; and 4th, A period of renewed capillary disturbance and congestion, commencing at the expiration of twenty-four hours; not from the first purging, but from the first effect of the influence in capillary disturbance; and if the case proceed onwards without death or cure, that similar changes may be traced (often notwithstanding very active interference,) in the two subsequent periods of twenty-four hours each.

5th. Which I have rather asserted from observation, than attempted to assign any reason for, that the fourth day after the occurence of algide symptoms is not characterized by the return of a distinct stage of congestion, but that the disease then goes on to consecutive fever.

It was my original intention to have selected cases from published works, by writers of authority, to illustrate these points, and to show by examples how the natural stages of the disease may be mistaken for the effects of treatment; to illustrate the evils of bleeding from an artery, or of mistimed venesection, in producing collapse; the injurious influence of stimulants and opium; the torments induced by hot applications; the effect of calomel in causing consecutive fever, &c.; but this course might appear invidious, and I am satisfied would be unnecessary, for it appears to me that a very small proportion of well recorded cases of cholera will be found which do not bear on one or more of these five points, and to this test I most willingly submit the opinions which I have attempted to inculcate with regard to its nature and treatment, always providing, however, that due allowance be made in applying the test for the effects of any treatment that is consistent with the views here advocated.

Let us now proceed to consider some of the more practical points of the treatment of cholera, especially in reference to HEAT AND STIMULANTS.

When the body is cold and exhibits so great a want of energy in the cuticular capillaries, the most obvious means of removing this condition, which suggest themselves to every one, are external heat and diffusible stimulants; but as these are not at all times attended with the beneficial effects that might have been anticipated, it is necessary to examine the reason of this failure by enquiring into the particular nature of the object to be gained. Finding that the disturbance of vital action commences in the capillary circulation, and not in the heart, it would appear that to begin by stimulating the heart is to begin at the wrong end of the chain of actions, and that to fulfil the indication some means is required that will act by giving power to the capillary circulation, independently of the heart's action.

Unfortunately all the medicines usually classed as stimulants excite the capillary circulation only by exciting the heart's action; secondarily and slightly, by sympathy with the heart; but principally in consequence of the distension produced by the increased force with which blood is injected into these vessels; for to them, as to the heart, distension is the most direct means of exciting irritability and action; but if these vessels are not distended, capillary action is not directly excited by exciting the heart.

In the stage of congestion in cholera, we have endeavoured to shew that this increased propulsion of arterial blood does not take place, for the quantity of arterial blood in the system is diminished by the lungs opposing the free transmission of blood to the left side of the heart. We have shewn, that so soon as congestion commences, the heart becomes excited by its most powerful stimulus,—viz., distension of its cavities, at least on the right side, but that its action is opposed by the condition of the pulmonary vessels, and oppressed by the accumulation of venous blood from the extremities: it is evident, then, that to add stimulus to that which is already excited to the utmost in vain, can do no good, unless it can add power, but as it cannot do so in this condition, stimulus can only exhaust the irritability of the muscle, but not propel arterial blood in the quantity necessary to distend and excite the capillaries. If the powers of the heart have not been yet excited to the utmost, a stimulant may prove useful by urging it to still greater exertions to overcome the obstruction, but if already excited in vain, it is quite evident that additional excitement can only do harm, by producing exhaustion of muscular power. If, however, we have reason to believe, that by-and-by the obstruction to the heart's action by the capillaries of the lungs, and impulsion of blood upon it from the veins, will both gradually cease, at least in some degree, it will be evident, that if it only retain sufficient power, the heart will then be in a condition to restore the circulation to a more normal state, and therefore that our efforts should be directed to husband the power of the heart, instead of exhausting it; but the only means of doing so consistent with the maintenance of sufficient systemic circulation, is to endeavour by every possible means to promote capillary action, and so to diminish the labour of the heart by removing the obstruction to its efforts, not by subduing the excitement of the heart by opiates. The whole question, therefore, lies in the means of doing this. We have shewn that when opportunity offers,—that is while the powers of the heart are as yet unexhausted, and its irritability unimpaired,—this may be accomplished by freely opening the veins of both arms, thereby soliciting the blood from the extremities, and relieving the heart from part at least of its oppression *a tergo*, but both experience and reason have warned us against attempting this after the heart has been too long subjected to excitement. What then is our resource in this condition? This is exactly the point which the long history of disease that I have submitted to you is intended to illustrate,—viz., that we actually possess medicines which have the power of stimulating and fortifying the capillary circulation without first exciting the heart's action. Many of the preceding remarks have been directed to show that the action of antiperiodic medicines is of this nature, only that as the morbid effects evinced in periodical disease of different types are not identical, neither can the antidotal effect required from medicine be identical. I have endeavoured to point out that, although quinine alone does no good in this disease, the combination of quinine with iron has proved in my own experience to possess this power in quotidian ague and in cholera, in a very remarkable degree. I have found it check the vomiting and purging often in a few minutes, and gradually restore the circulation and warmth, and the secretions, without the assistance of any other applications, not, be it remarked, by exerting an astringent effect in moderating the diarrhœa, for it proved equally useful whether the symptom existed or not, but by removing the cause in the disorder of the capillary circulation.

But it is not enough to consider a question of so much importance as that of the use of heat and stimulants merely theoretically; I must, therefore, add my testimony to that of many authors against the use of diffusible stimuli where congestion is fully established. I have tried it carefully, and anxiously watched the result of each dose with my ear to the heart, and I can positively assert that each dose, in cases where congestion has reached to a dangerous degree, is attended with increase of all the symptoms, dangers, and sufferings attending congestion.

The first time I was myself attacked with cholera, the symptoms were at once arrested by a glass of brandy taken on the first occurence on the blueness, purging, and cramps, because congestion, being at once relieved by the diarrhœa, had not reached the point of oppressing the heart's action, and the stimulant aided the action of the heart to excite the capillaries; but on my second attack, the effect of this stimulant was very different, and excessively painful and suffocating, because not being relieved by diarrhœa, the congestion at once attacked the vital functions, and

I was obliged to have recourse to quinine and iron, which soon relieved the suffocation.

The case in which I had the best opportunity of carefully studying this effect of stimulants was that of the late Chief Physician of Persia, a native of that country, who had been educated at St. George's Hospital, London, and an intense admirer of calomel. His attack was not one of pure cholera in its usual form, with purging and vomiting, for he was one of the earliest sufferers from the disease, while the digestive organs were as yet seldom affected, and when it presented many of the characteristics of ague. I attended him as an intimate friend, night and day, although no persuasions could induce him to try either the medicines I recommended, or bleeding. He felt so bilious that he was sure nothing would do but calomel. His attack lasted for several days, in each of which, the cold stage that at first was followed by return of warmth, was more prolonged, till at length intermission ceased; the relief which was denied by the bowels was here effected by the skin, and the quantity of ice-cold watery exudation with which all the body but the breast was constantly bedewed, proved the rapidity with which the iced-water he was incessantly imbibing was both absorbed and exuded. He was much colder than I have ever found cholera patients in whom the characteristic diarrhœa existed. After resisting every proposal I made for treatment, he at length consented to try stimulants, for it appeared he was determined to die a martyr to medical prejudice, accordingly, I administered to him large doses of æther and alcohol. After each dose, on applying my ear to the heart, I heard a sudden increase of confusion in its action, a sort of indescribable churning sound, as if it would burst,—not as if it were getting rid of its contents, but rather constantly agitating the same portion of fluid,—and at each fresh effort of this sort, the icy exudation from the skin burst forth in redoubled quantity. I repeated this experiment until I had again and again proved the effects, and then, as in doing so, it was plain that more harm than good resulted, I left the case to nature, determined to watch the symptoms as they proceeded. It was not till the last day that the brain became materially affected, although, in the course of four or five days, I am certain not above half a pint of urine was secreted; he then became insensible, the pulse became imperceptible, and at night, being seized with fearful tetanic convulsions, which lasted for two or three hours, he died. The first case of cholera, with purging, occured on the day of his death in the next house, in a child, and was fatal in two or three hours.

This effect of stimulants in a congestive disease so very nearly allied to cholera, completely confirmed what I had formerly seen of the result of their exhibition in the mroe common form of cholera at home, insomuch that I have not since been tempted to repeat the experiment unless conjoined with bleeding, or in the very first moments of an attack, when there can be no doubt of their efficacy, or that of any other means of inducing an impression calculated to rouse the sympathetic through any single function, to a perception of its other natural stimuli.

A diffusible stimulant can, therefore, only do good while the heart possesses more power than has yet been called into action, or before it has yet become so completely oppressed as to be unable to respond effectively to the stimulus. The same reasoning might be applied to the question of using external heat as a stimulus, for though heat and pain are as powerful excitants to local action as brandy to the stomach, they unfortunately also act directly as stimulants of the heart's action. Difficult, however, as it may be to explain their effects, I feel bound, in conjunction with many others of great practical experience, to protest against the use of both, when the case has reached the stage of confirmed congestion; for though in the preceding period, external heat may be as useful as a diffusible stimulus, it is not less injurious in the second. On this subject I cannot do better than quote from one of our most recent authorities:—Mr. Parkes says, "Warm baths, vapour baths, and warmth applied in any way to the surface, never appeared to me to be of the slightest service in true cholera; the spasms were sometimes relieved, but the algide symptoms were almost invariably increased. The depressing effects of the warm bath were sometimes marked and unmistakeable; I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool, but not cold surface, and in five or ten minutes, have seen the same man carried from the bath, with a pulse almost imperceptible, and a cold and clammy skin."

These remarks perfectly coincide with my own experience of external heat. I have used the hot-air bath, so much recommended in this country, to such an extent as even to scorch the skin, without producing the slightest re-action, but apparently the worst effect, and have searched in vain for an excuse for the torture to which so many patients have been subjected by such attempts to restore warmth. These would not have been cruelty, whatever the sufferings, if really beneficial; but I am now convinced of the contrary, and that they are not only useless, but injurious. Without knowing more of the actions which took place in the systemic capillaries in health, and their changed operation in this disease, it is impossible to account for these injurious effects, unless by excitement of the heart, or on the principle which prevents us applying heat to a frozen limb, to which the condition of the circulation is somewhat analogous; practically I have come to the conclusion that external heat is always injurious when congestion is extreme. In my opinion *medio tutissimus ibis* in this respect, and that it is better to be content with a moderate temperature in the room, sufficient to promote any tendency to re-action, but not to force it.

The question, however, is one of no mean importance,—whether if we are not to yield to our natural impressions regarding the necessity of restoring warmth, we may not cede to the adjurations of the patient to indulge him with cold? This is a question which I am not bold enough to decide. There is I think much more evidence in favour of cold than of heat, yet both may be wrong, and according to my own experience both extremes are unnecessary. One friend writes to me from Tehran, that his practice of immersing the patient to the neck in cold water, till re-action took place, was very successful; another that he cured his patient with frictions with snow, in Erzeroom, where the cholera occurred in the winter season, and where the cold is intense. In Russia, and in Hamburgh, iced drinks have been much used. I am well acquainted with two Persian gentlemen, who were laid out for dead, in the cholera of 1829, but who recovered on being washed in the open court-yard, in a winter's night, as a preparation for interment, with water, to obtain which the ice had to be broken; and I was once called to the public burial ground of the city to see a dead man whose veins had begun to bleed while they were washing his body. The man, it appeared, had been attacked with one of those forms of disease already alluded to—a species of cholera without purging—had dropped down as struck with appoplexy; an attempt had been made to bleed him in both arms without succes; he was left for dead and carried off for interment, when what I have related occurred on his being deluged with cold water; unfortunately no one had presence of mind to bind up his arms and protect him from the cold, and he was really dead when I arrived. These are strong arguments for the use of cold, but may perhaps be met by equally convincing arguments against it. I look upon these as instances of powerful nervous impression made at a suitable stage of the disease, and that if the remarks formerly made of the *post-mortem* return of heat in the corpse be attended to, no such accidents can arise as those fearful cases sometimes mentioned of persons being buried alive. If one, apparently dead of cholera, become warm from the extremities upwards, no doubt of death can exist, and no chance of recovery; if this does not take place, or, on the contrary, if warmth proceed from the trunk to the extremities, there is a possibility that he may be alive.

I consider the question of the excessive external application of heat as fully determined in the negative in the congestive stage, but open to a certain degree in that which succeeds it, and that the question of cold applications is still *sub judice*, and subject to objection in the stage succeeding congestion, which, however, is so difficult to fix precisely, that both extremes are better avoided, especially as I am convinced that treatment conducted on other principles will almost always prove successful without either.

Now, with respect to the Diarrhœa,—enough has formerly been said on this subject to show that it has been considered rather as a means of relief to congestion, than in itself a source of danger, as tending to prolong life, rather than as the cause of death in cholera, insomuch, that even if we had it in our power to check it by a word, it is a word which we should hesitate to pronounce, unless we were assured that the cause rather than the effect would be influenced. If then it is to be considered in itself as rather a good than an evil, it must, nevertheless, be always looked upon as a sign of imminent peril to life, which, as its direct restraint would endanger immediate collapse, should the more

impress us with the necessity of removing its cause without delay; for when the disease is in the neighbourhood, and sudden evacuation of this kind occurs, even though not another symptom of cholera be present, the case is to be considered one of cholera. It is not a secretion from the bowels, for that is a vital action of the capillaries; this is the very reverse—an exudation, showing that the capillaries of the bowels have succumbed to the same cause of impaired action which we have throughout considered essential to the disease. The capillaries of the skin may yet perform their functions, the disorder may be almost local in the bowels, but it is essential, and were it possible by ligature, or any mechanical means, to arrest the exudation, without arresting its cause, it is scarcely possible to doubt that the effect would be congestion and oppression of the heart; that this would be attended with diminished circulation of arterial blood, and impaired powers in the whole system, which would then succumb to the anti-vital influence, and terminate in collapse. To what purpose then would we use astringents to check the diarrhœa? Have we not rather reason to congratulate ourselves that such means are for the most part wholly inoperative? In the perusal of cases it is impossible not to be struck with the complacency with which the authors seem to regard the effect of pills of lead and opium, chalk mixtures, catechu, &c., when administered at the very period of the disease, in which we should naturally expect to find congestion on the decline, (if the disease be really one of quotidian periodicity,) an effect which, if real, instead of apparent, would only be to be deplored. If, however, we check the diarrhœa, by removing the cause, restore their wonted action to the capillaries, and thereby prevent the effect of their disturbance, we cure, rather than restrain, the diarrhœa. Here, however, a difficulty meets us; the congested condition of the vessels, and their reverse current of blood, afford very obvious reasons for expecting very little effect from any medicine that can act only by absorption into the blood from the alimentary canal; nevertheless, experience proves that absorption does take place to a degree much greater than might have been anticipated. However great the amount of exudation of the fluid of the blood into the intestines, this occurs only at intervals, and paradoxical as it may appear, while the congestion continues unrelieved, there seems to be a great tendency to restore fluid to the blood, during these intervals. Sufficient evidence of the absorption of salts in solution, in the fluids of which the patient drinks with such avidity, is to be found in the fact, that tartar emetic produces its effects when given in this manner, and those I have so often witnessed from quinine and iron, given in weak solution, have been so immediate, as to excite my astonishment. Thirty drops of muriated tincture of iron, with a grain of quinine, in a wine-glassful of water, will often stop vomiting and purging that has resisted large doses of laudanum, and of solution of morphia. That so small a dose of this combination should ever prove effectual is singular enough, but I do not think it very wonderful that opium should rather increase than diminish these symptoms, if they are really mechanical effects of congestion, and the results of loss of irritability in the capillary vessels, (not of irritation in the viscera,) for opium will evidently tend to subdue whatever irritability may be left in the nervous system, and thereby give more scope to the anti-vital influence, instead of opposing it. Theoretically, such should be the effects of opium, and practically, I am very sure, that it more frequently tends to keep up the vomiting and purging, and the algide symptoms, than to cure them. If the symptoms proceed from partial paralysis, or loss of irritability of the sympathetic, nothing could be better calculated to augment them than opium, for in at least half the cases in which opium is prescribed in ordinary practice, its sole object is to subdue morbid irritibility of the sympathetic system. If, however, our reasoning upon the etiology of cholera has had any foundation at all, it goes to prove that the great object of cure is to maintain the vital sensibilities active, and to husband the power of the heart, and neither to exhaust irritability by undue excitement, nor to lull it into insensibility by opiates, for our hopes all depend on the integrity of these qualities, when the period of capillary relaxation arrives.

With respect to most medicines given in pill, I consider them nearly inert in the congestive stage of cholera, more especially those which are not soluble in water, but require the action of the secreted juices of the stomach for their solution, for we know that such secretion does not then take place. Calomel is fortunately one of this class, and in cholera passes through the bowels almost unchanged, and I am told on good authority may often be found at the bottom of the bed-pan in the form of a grey powder. With respect to the influence of calomel in this disease, I am fully satisfied that the most mistaken opinions prevail. We have already alluded to the effect of tartar emetic, which is said to be eminently curative, and on the principles of nervous impression, we can well conceive that it will be far from inactive, if it produces its characteristic effect, because that effect is a vital one of great influence on the whole of the sympathetic system, and were we destitute of safer and more appropriate medicines, it might be well worthy of trial. In like manner croton oil has been recommended: it may act like the diarrhœa of cholera, or venesection, in reducing the volume of the blood; but if it produce secretion, which is a vital action of the bowels in opposition to exudation, may operate as a stimulant to the vital energies of the sympathetic, and prove curative. (The danger of inducing syncope by these means is, however, too evident to require comment.) So also may any other medicine or means whatever cure, which produces a powerful impression on the nervous system, provided it do not act by injuriously stimulating the heart's action,—by impairing the force of capillary circulation, or the excitability of the nervous system,—or, in other words, if it be not calculated to produce the very effects which are caused by the exciting cause of the disease. Upon this principle Calomel, if it act as a purge, may prove beneficial, but if it affect the constitution, it can only be detrimental. This, I think, will become evident, if we consider the action of mercury but for a moment; its chief value is in subduing inflammation, but all our observations tend to show that the condition, both of the blood and of the capillaries in cholera, is diametrically opposed to that of inflammation. The medicine most useful in cholera and allied diseases,—viz., iron and quinine, are those most opposed to the effects of mercury, and most curative of them. All experience goes to prove that mercury, if an irritant, is at the same time a cause of positive weakness of capillary circulation, with the single exception, and that a doubtful one, of its effect upon the liver. There is no point in medicine on which I feel more entirely convinced, than that the constitutional effects of mercury are invariably injurious in cholera, and tend to produce consecutive fever. Soon after cholera appeared to have ceased, in 1843, I submitted two patients to a course of mercury for chancre at the same time,—one, a groom, died collapsed as soon as the gums became affected, though a very healthy powerful man; the other, the Persian High Treasurer, fell into the same condition, and was only saved by large and quickly-repeated doses of quinine and iron, and went through a severe attack of consecutive fever.

Mr. Parkes mentions, that two syphilitic patients, while under the influence of mercury, were attacked by cholera in his hospital, as proof that mercury is not prophylatic, I might mention several instances to prove it really a predisponent; but so many points of difference will suggest themselves in the condition of one laboring under cholera, from that in which we should be anxious to produce ptyalism, that it is unnecessary to pursue the subject farther.

There now remains, I think, but one material point in the treatment of cholera, viz., that connected with the use of Frictions and Rubefacients. Of these, it may be sufficient to say that their efficacy will depend upon the period at which they are applied, which is always difficult to decide, and on the degree of fatigue thereby occasioned to the patient, they partake much of the nature of heat and stimulants; but if applied without disturbing the patient, may perhaps be beneficial, although I have not myself very much confidence in them. Disturbance of the patient is a question of much more importance. In a report of the Commissioners of Public Health, much evidence is adduced by the examination of several medical men practising in London during the former visitation of cholera, as to the ill effects of removing cholera patients to temporary hospitals. There is very obvious reasons for desiring to remove them from the locality in which they have actually acquired the disease, because, undoubtedly, the cause exists there, and will continue to operate powerfully against the effects of remedial measures, and will also endanger the health of those who are in attendance, and if any of them happen to be attacked, it is sure to give apparent countenance to the popular belief in contagion; but if the disease have been acquired elsewhere than in the dwelling, no object is to be gained by removal unless to a very decidedly more healthful locality,

while on the other hand, the evidence is conclusive that danger often attends the practice. In considering the stages of cholera, it was remarked that there is a time when the patient appears rather indifferent than stupid—not asleep—for sleep never occurs in cholera, but as if resigned to fate rather than willing to make the slightest exertion. This condition marks that stage in which the tendency to *cessation of morbid inaction* exists rather than re-action of the powers which circulate the blood, that, namely, in which the influence that causes the disorder is so equally balanced against the resistance of the vital powers, that the slightest disturbance or source of exhaustion will turn the scale, and it is the nicest chance whether a stimulant will now rouse the vital powers to throw off the oppression, or by disturbing the circulation then in progress of partial recovery, renew the congestion. It is at this period that the greatest danger of syncope and collapse is incurred by an attempt at removal, and even frictions or anything that tends to disturb the patient may induce syncope and collapse; but sinapisms and blisters that can be applied without disturbance, may prove beneficial.

It is almost needless now to state the practice that I should myself pursue in this disease. If asked what I should do in a case of cholera, I should answer that, that depends very much on the stage in which I found my patient, but in all cases and in all stages I should certainly give a largely-diluted solution of some salt of iron and quinine, with a view to counteract the morbid inactivity of capillary circulation, and repeat it as often as the patient would drink of it till warmth was restored. If called to one of those cases of sudden collapse, with little vomiting and purging, reported to have frequently caused death in eight hours in Scotland, I should be satisfied that mechanical assistance was required to relieve actual congestion, and if I arrived sufficiently early would open the veins of both arms, trying to induce sneezing and coughing, or almost any powerful vital action, likely to give an impulse to the blood, and cause it to flow. If, however, I arrived too late to bleed, I should not hesitate to conjoin epsom salts with the mixture, however heterodox such practice may appear, because such cases are generally without purging, and purging is a safer and more direct source of relief than bleeding late in such a case; and I have followed this practice largely with success. I should neither give stimulants nor apply heat, should limit external applications to a blister or tincture of capsicum to the spine, be careful of disturbing my patient unnecessarily, and endeavour thus to carry him over the first twenty hours with life; if his condition was then such as to give little hope of his being able to sustain a fresh attack of congestion, I should risk every thing to obtain blood to such an amount that the colour should become florid and its motion free, and should continue the medicine till the secretions become fully re-established—till the hue of health returned to the complexion, and the lips and tongue had lost their pallor.

In all cases of consecutive fever my experience points to the use of the same remedies conjoined with salts and diminished quantity of quinine, which in excess, or even if not in very small quantity, tends to produce dryness of the tongue, but which I think necessary, nevertheless, for a time. Perseverence in this treatment, long after the secretions are restored, no doubt sometimes tends to produce a red, furred, and bilious tongue, very different from the clean, glazed, and dry red surface, or pale-edged darkly-coated appearance that preceded it, but then the fever has become billious, and may safely be treated with a smart dose of calomel, or what I find still better, sweating in the wet sheet of the hydropaths. The same treatment is appropriate to the prevailing form of remittant, commonly called low fever unpreceded by cholera or noticable collapse, and unaccompanied with maculæ or typhoid eruption, but characterized by the appearance of the tongue. If the spleen be tender on pressure, a short trial of sulphate of iron with sulphate of magnesia, without, or with not more than half a grain of quinine to each dose, with occasional application of leeches to the epigastrium, (in the forenoon,) will, I think, show that there is something in the treatment recommended, if nothing in the theory. *Post-mortem* examination in this type of fever constantly exhibits more or less softening of the spleen, a characteristic of tropical remittents, and in my opinion a positive indication for the exhibition of iron.

I must now conclude, but I confess I leave the subject with much reluctance, for there are many points of much practical importance left untouched; among others, the influence of the epidemic constitution while cholera prevails upon other diseases apparently unconnected with it, is full of interest.

Of these I cannot refrain from mentioning one example before concluding. While cholera prevailed I had two patients with pannus oculi, in which I had tried every possible local application in vain; one of them, however, had on one occasion a slight shivering fit when he visited me, and I gave him a mixture of quinine and iron, the pannus immediately began to diminish, and I tried the same medicine on the other, omitting local applications, and in both a cure was effected in the course of ten days. Similar effects were observed in a great variety of other diseases. I am very well aware, that in this laudation of a particular combination of drugs, I have subjected myself to the charge of empiricism, and in a certain degree I must acknowledge the charge to be just, for nothing could be more purely empiratical than the grounds upon which I was first induced to make trial of it, whatever my subsequent endeavours to explain its operation; but my object now has been only to recommend the combination of iron with quinine to your consideration, as a means well fitted to attain the end in view,—viz., to promote the oxygenation of the blood, to fortify capillary action and nervous irritability, to confirm the tendency to remission (whether apparent or too obscure to be noticable or presumeable,) and to prevent periodical return of venous congestion. I have endeavoured to shew that these are the primary objects requiring attention; relief of congestion by mechanical means the secondary one.

If I have over-rated the effect of the medicines recommended, I must be allowed to say that I have not been induced to do so by theory alone, for that which I have offered to you is solely the result of reflection after I had used it in many thousand cases effectually, but purely empirically. While the epidemic prevailed in Tehran, my court-yard was daily crowded with hundreds of poor wretches, praying for the love of God for a supply of the bitter water, or water of life as they called it. It was served out to them in pint-and-half mixtures of water, containing twelve grains of quinine, nine of sulphate of iron, and thirty drops of dilute sulphuric acid, with directions to take a coffee-cupfull for a dose; these orders, however, were not always attended to, and in some of the most remarkably rapid cures I witnessed, the whole was swallowed at a draught. One of these I particularly remember.

In visiting one of the princesses, the wife of the Chief Astrologer, I observed a wretched object, who had dragged himself to the tank in the outer court of the house; with difficulty I recognized him as the tutor of the family. He had been vomiting and purging, and agonized by cramps, and his thirst had impelled him to the position in which I found him. I immediately sent him a quart bottle of the mixture described, and I was never so much surprised as to see him in the afternoon at my house with his bottle, whither he had walked nearly a mile, to beg a fresh supply, having swallowed the whole at once, and felt immediately revived. I had previously known him as, both from religious prejudice and as an epicurean, an abhorrer of medicine; and this certainly was not nectar. Villainous though the compound may seem, it appears to be peculiarly grateful to the patient, by appeasing the thirst and sense of heat in the stomach, which even ice fails to allay, and I have seldom seen it vomited after the first dose or two. I have met with one or two instances in which it was found impossible to obtain blood from the veins, but where, shortly after a few doses of the medicine had been taken, bleeding was practised with success.

In the cases of simple diarrhœa without tenesmus now prevailing, I find the most immediate effect produced by a grain or two of quinine given with half a drachm of the muriated tincture of iron, in a wine-glassful of water. With respect to the cramps and spasms, I have been in the habit of looking upon them as mere symptoms, only to be obviated by removing the cause; it would, however, appear from the observation of others, that, apart from the more fatal symptoms of suppressed arterial circulation evinced in convulsions; the cramps in the legs, &c., are closely connected with the contracted condition of the colon, and I should therefore feel inclined to try the effect of assafœtida injections.

I have now, gentlemen, only to thank you for the attention with which you have listened to my remarks. The treatment which I chanced to adopt on purely empiratical grounds appeared to me more than usually successful, and I was led to enquire whether

there was any thing in the nature of the disease, or the remedies, that was explicable on known and acknowledged principles of physiology and therapeutics; the result was, that there existed a deficiency of facts by which to determine the question, and, till these were settled, it was impossible that all should agree on the treatment or deductions, at the same time it was evident that there were facts which, if generally admitted, would necessitate universal concurrence in a broad principle of treatment, to the exclusion of a vast amount of erratic practice, that might be termed a blind groping for facts on which to reason.

In my endeavour to ascertain what were the established facts, it appeared that beyond a rehearsal of symptoms there was not one universally admitted, save the constant existence of venous congestion,—contagion, the effect on the blood, the cause of death, the influence of particular treatment, were all subjects of hot contention, and the medical profession was divided into as many parties, and more, than there were sections in the materia medica. Under such circumstances it appeared that the weight of any single evidence could not possibly turn the scale of opinion, and that the only chance of arriving at a useful conclusion was to examine the disease in all its forms, as they relate to its cause, its course, its terminations, and its connection with other diseases. The result of this I have endeavoured to lay before you, in the reduction of the essential facts to six, not one of which is generally admitted, but if they be proved to be facts, the deductions from them are too evident to require comment. These which have previously been stated at length are shortly as follows:—

1st. That the cholera is the effect of impression on the sympathetic system,—that its cause does not primarily affect the blood, is incapable of reproduction in the body, and, therefore, incommunicable.

2nd. That its effects begin by impaired vitality and disordered functions of the capillary circulation.

3rd. That congestion of the thoracic and abdominal veins is the consequence of the above condition, and the cause of other symptoms.

4th. That exudation of the watery particles of the blood, whether through the mucous, or into the serous tissues, constitutes the natural mode of relief to this congestion.

5th. That cholera goes through certain diurnal stages, of which capillary disturbance is the first, venous congestion the second, more or less intermission or cessation of capillary disturbance the third.

6th. That the natural tendency of cholera is to go through these successive changes for three days, and then cease, or to run into consecutive or remittent congestive fever.

It appears to me that if these are facts (the establishment of which will depend on the observation and reading of every practical man,) a principle of treatment is inevitably established along with them, because they are facts on which the whole pathology of the disease may rest, and the whole course of treatment depend. If they prove otherwise, I have wasted your time and my own, in a feeble attempt to recommend a combination of quinine with iron, as only one of the numerous medicines said to be beneficial in cholera, but the action of which will then be totally inexplicable; and I shall owe you all much apology for inviting you to listen to the arguments by which I have attempted to substantiate as facts, what another process of reasoning may, perhaps, demonstrate as fallacies.—*Provincial Medical and Surgical Journal, London.*

New Diagnostic Sign of Intermittent Fever.—This sign, pointed out by M. R. Vonoya, consists in a peculiar condition of the mucous lining of the lower eye-lid. In a healthy subject this surface presents a more or less vivid red colour; but when intermittent fever has lasted some time in any subject, another appearance is observed, constituted by a pale crescentic line, the extremities of which correspond to the canthi of the eye, and its concave edge embracing the sclerotica; it is visible on lowering the lid and directing the subject to look upwards. This appearance is dispelled by the internal administration of febrifuge medicines and persists so long as the patient is exposed to a relapse.—*Medical Times.*

Lecture on the Asiatic Cholera, by George Ross, Esq., *Surgeon to the Western City Dispensary, and author of "Papers on Typhus Fever," and "Essays on the Processes of Digestion and Nutrition."*

It is with some diffidence that I approach a portion of my subject surrounded with so much perplexity,—upon which so many bold and contradictory opinions have been uttered, and so many plans of practice, orthodox and heretical, have been pursued. The modes of practice adopted have been divided into various classes,—for example, the specific, the pathological, the chemical, the rational, and many others. There has been a remarkable variety of rational plans of treatment, for, of course, each author appropriated this self-complacent title to his own practice, and distributed the others among his opponents. There is clearly, therefore, more than one rational plan, and the irrational systems are too numerous to relate. It is somewhat amusing to observe writers defining the rational system, and at the end of their dissertations to find that the plan proposed is exactly that they have employed. If there be modesty, there is at least great candour in this confession of the respect they entertain for their own skill. When Cobbett published his grammar, he declared that it was the very best that had ever been written, and maintained his declaration by adding, that he intended it should be the best; he wrote it in the best manner he could, he believed it to be the best, and he should be a liar if he said anything contrary to what he thought. So for the Rationalists; the only difference is, that they give the lie to one another. However, each of these authors must be judged by a jury—the profession—according to the evidence—the facts of his own practice. We must not be content to take his practice at his own valuation of it. The lunatic, when he was questioned as to the cause of his being in Bedlam, replied "that the world and he had a dispute; the world said he was mad, and he said that the world was mad; the world being more numerous, had a majority, and he was outvoted." Some of the Rationalists may possibly find themselves in the same situation.

The opinion, however confident, which medical men express of the mode of treatment which they have pursued, is utterly valueless, unless the facts of their practice lend it corroboration. I have been amused by reading the heroic assertions that have been published in favour of particular systems; and, on comparing the mortality under the vaunted system with that under other modes, have been surprised to find, that the fatality preponderated on the side of the most successful practice! This is the testimony of facts against opinions, and shows the self-deception to which men yield in matters of therapeutics.

I have endeavoured to avoid these errors. By collating all the facts that appeared to me to be credible and unexceptionable—positive in themselves and complete in their relations—by arranging and tabulating them, I have attempted to discover the most successful treatment in cholera; and if I am not thus prepared to recommend a system cleared from all error, I am, at least, qualified to point out injurious practice. Some of the alleged facts may be erroneous—I admit it; all are not cases of cholera that are so called. Bad diagnosis is a source of error that will always tend to render calculations on medical practice uncertain; but, then, it is an error likely to occur in every set of cases, and we may expect that the errors of one register will counteract the errors of another. I have, also, done all I was able by discrimination and selection to avoid this liability to error, and have excluded all cases from my Tables which did not appear, according to the reports, to exhibit well marked and decided symptoms of the Asiatic disease. I have acted in this manner invariably, and without the slighest bias in reference to the results of treatment.

When special reports have not accompanied the cases, I have received them in the aggregate as genuine cases upon the authority of the authors. I shall, however, feel it necessary, in commenting on the various tables, to call attention to particular sets of cases, of whose correctness I may entertain any suspicions. Whenever the results by the same mode of practice in different hands are very strongly contrasted, I shall advert also to a possible source of error in the reports, so that we may be able to come to a definite and well-grounded opinion without prejudice of persons or bias of authority.

The following Table exhibits the results of the treatment of 1821 cases occurring in England, exclusive of the city of London, of which I have collected the returns. This and the succeeding Table are arranged according to the increasing rate per cent. of mortality for facility of reference:—

Comparative Treatment and Mortality of Cholera in London.

TREATMENT.	Number of Cases.	Deaths.	Rate per Cent.	Remarks.
Salines with Cold Water,	81	7	8.5	A return from Greville Street Dispensary.
Tartar Emetic,.....	21	4	19	A return from Droitwich Lunatic Asylum.
Salt Emetic and Cold Water,.....	607	112	29.29	
Calomel alone,.....	331	107	31	Including Dr Ayre and Dr. Peacock's Cases.
Blood-letting, with Calomel and Opium,.....	210	117	56	
Stimulants alone,.....	167	96	57	
Do., with Calomel and Opium,.....	356	214	60	
Venous Injection,.....	48	39	80	Injection of Saline Solutions.

I will now add another Table, based upon a Table framed by Mr. F. De Grave, for the city of London.

City of London.

TREATMENT.	Number of Cases.	Deaths.	Rate Per Cent.
Combination of salts used at Greville-st. Hosptl.,	26	8	30.77
Calomel,.....	75	35	46.66
Miscellaneous,.....	17	8	47.06
Calomel and Opium,.....	196	112	57.14
Opium,.....	81	47	58
Stimulants,.....	63	42	66.66
Combination of Salts proposed by Dr. Stevens,...	25	22	88
Venous Injection,.....	20	18	90

Salines.—A short comment on the foregoing Tables may serve to bring out the facts in more distinct relief. By both Tables we find that the saline treatment as practised at Greville-street was highly successful, and, as a contrast with the received mode of treatment by calomel and opium, and stimulants, I may state that, at Greville-street, the mortality by the latter system was as high as 78 per cent, and in the City, according to the above table, it rose to a very high figure. The formula employed at Greville-street was—Carb. soda, ℥ ij.; sodii chlorid., ℥ ij.; potass. chlorat., gr. viij.; aq. q. s. pro haust. This draught was given about every quarter of an hour. The patient was also placed in a hot saline bath at 120°, in which 14 lbs. of salt were dissolved. Cold water was drank *ad libitum*.

Although I have been unable to collect a greater number of trustworthy facts in reference to the employment of salines, yet I have observed, in the course of my researches, an almost universal opinion in favour of their efficacy. Of course those who find in this practice a confirmation of a theory, speak in raptures of its merits; others, less sanguine, admit the success of this kind of practice, but doubt its specific influence over the disease. A few treat them as useless or injurious. I do not desire to reconcile these opinions at the present moment; my business is with the facts, and whether the benefit conferred was active or passive is of minor consequence.

Tartar Emetic.—There is hardly any remedy that has been spoken of with greater approbation that tartar emetic. In the Droitwich Lunatic Asylum, 21 cases were treated with an emetic of 3 grains of tartarized antimony at the commencement, followed up with 1 grain of opium every hour. Twenty-six other cases were treated there with calomel and opium and stimulants, of whom 17 died, or in the ratio of 66 per cent.: there could be no doubt, therefore, that stimulation was bad. A Mr. Littleton, of Saltash, was in the habit of dividing half a drachm of tartar emetic into 5 grain doses, and giving one every twelve minutes until vomiting had ceased. He then administered 40 or 50 grains of calomel. He highly applauds the tartar emetic plan, but unfortunately he has not tabulated his cases, which appear to have been very numerous; his testimony, therefore, has lost all its direct value. Many other writers have spoken in favourable terms of the use of tartar emetic.

Salt Emetic and Cold Water.—The cases in the Table are collected from a variety of sources:—Dr. Pidduck's Report on the cases at the St. Giles's Dispensary; Dr. Venables' cases at Wick; Mr. Benman's, of Covent-garden; Dr. Hoskings', of Guernsey; Mr. Godrich's, of Chelsea, &c. Dr Hoskings state that, of 62 cases treated on this plan, only 16 died; whilst, under stimulants and opiates, the same number died out of 37 cases. Mr. Godrich treated 12 cases in this way and all died; he, however, adds that they were all hopeless cases before trial. With the exception of these cases, reported by Mr. Godrich, the mortality under this treatment was remarkably low in all other instances, as the table proves. The dose is about two table-spoonfuls of common salt, in from four to eight ounces of cold water, every quarter of an hour until vomiting is produced; then cold water, in large draughts, to allay the insatiable thirst and heat at stomach.

Seeing the low rate of mortality which followed this practice, it would appear that the benefit, both in these cases and in those treated by the Greville-street plan, was mainly owing to the common salt. At any rate, we are authorized in concluding that common salt is one of the most useful of the saline remedies. The addition of chlorate of potash, in the Greville-street prescription, would seem to be judicious, and attended with success. It is found to be useful in other cognate diseases in which the vitality is low. We must not overlook the fact, that large quantities of cold water were permitted to be drank by the patients under the saline plans; and, although I have not collected any cases showing the results under the simple cold-water plan, yet there is reason to believe that it is of great utility.

Calomel.—The most discrepant opinions are expressed relative to the treatment of cholera by calomel. Dr. Ayre's and Dr. Peacock's cases are included in the 331 cases cited in the Table; but, if these cases had been omitted, the results of the treatment by calomel would have been 62·9 per cent. instead of 31 per cent. How comes it that the calomel treatment, in the hands of these gentlemen, was so highly successful? Each of these gentlemen adopted the same plan, viz: the administration of one of two grains of calomel, with from one to five drops of laudanum, every five, ten, or fifteen minutes. Dr. Ayre omitted the laudanum when 60 or 80 drops had been given. Under this system Dr. Peacock states that he did not lose a single case out of 30; and the mortality among 191 cases treated by Dr. Ayre was not more than 18 per cent. In a recent communication to a contemporary journal, Dr. Ayre cites the number of his cases as 219, the deaths 43; being a mortality of 19·6 per cent. This mortality is remarkably low.

On the other hand, small doses of calomel, in conjunction with port wine, were given by Mr. Banner, of Liverpool, and the mortality was 52·6 per cent. At the Cholera Hospital, Holbeck, small doses of calomel, with large doses of brandy, were at first given and disapproved; afterwards the reverse plan was adopted, with more success;—nevertheless the mortality was 53 per cent. These latter cases, and many others that might be cited, prove no more than this, in comparison with the cases treated by Dr. Ayre, that the stimulants with which the calomel was associated were positively injurious to the patients.

Other gentlemen recommend calomel in large doses,—say, on the average, half a drachm, repeated every hour or two, combined or not with opium; but the mortality under this plan was not so low as to be considered an indication of satisfactory treatment. Dr. Searle has strenuously advocated the use of large doses of calomel; and my friend Mr. Bird, who, during the years 1831-2, had extensive opportunities of observing the epidemic at Merthyr Tydvil in Wales, has also expressed to me his conviction in their efficacy. Dr. Blackmore in a recent communication to the *Provincial Journal*, says, that calomel alone induces serious remote effects. In the city of London we find that when calomel alone was given, the mortality was only 46 66 per cent.; but, when calomel was combined with opium, it was as 57.14 per cent. The stimulant system resulted in a still higher mortality. It would appear, therefore, that opium and stimulants were decidedly prejudicial. There is evidence, then, to conclude that calomel is most efficacious *when uncombined with any other remedy*; and the question may now be asked, whether the plan of treatment employed by Dr. Ayre was accompanied by so low a

mortality by virtue of its own merits, or in consequence of defective diagnosis. Some discussion took place in the year 1832, when Dr. Ayre first promulged his plan, and reported his cases, about the accuracy of his statements, and when doctors differ upon the correctness of diagnosis, who shall decide? At this distance of time I certainly cannot undertake to pronounce an opinion. Nevertheless, this suspicion damages the evidence. Granting that Dr. Ayre's cases were genuine cases of the disease, does the calomel exercise a positive influence over the malady, or is it wholly inert? I should not have thought of following out the inquiry in this close manner, if Dr. Ayre had not himself invited examination by repeatedly publishing his opinion that, during the stage of collapse not a particle of the calomel was absorbed, and yet that its efficacy was peculiarly shown during this stage. He mentions, in a communication to the Secretary of the Central Board of Health, the case of a woman, 91 years of age, among others, who recovered from the stage of collapse by calomel, and yet informs us, in the next sentence, that "during the period of the collapse no absorption of it takes place." If this be true, what merit was due to the calomel for this woman's recovery? Either Dr. Ayre's patients recovered from the stage of collapse by the aid of the *vis medicatrix naturæ* alone, and no thanks to the calomel, or his explanation of its action is unsound. How can a medicine cure that has never entered the system? It is plain, that if the patient has rallied from the state of collapse without or in despite of medical aid, that the claim set up for calomel as a curative remedy is invalid. Dr. Ayre's theory proceeds upon the necessity of exciting a flow of bile, and yet he informs us this result cannot be expected in cases of collapse until the patient has emerged from that state, and is, therefore, virtually cured. I would defend Dr. Ayre from his own theory, and be content with the facts he has stated, if I could be quite satisfied that they were facts. I think that the evidence he has brought forward demands attention, and the institution of new sets of experiments with the view of testing the efficacy of his plan of treatment. I really believe that if not altogether so successful as he states, it will be found far more useful—less dangerous, perhaps, I should rather say—than opiates, stimulants, and other very active remedies. Dr. Ayre allowed his patients to take as much cold water as they desired, and he objected to stimulants. Much of his success might have been owing to this auxiliary remedy.

The treatment of cholera by the administration of large doses of calomel has been also recommended. I have had some experience of this mode of practice in typhus fever, and in the early stages of that disease have found it to exercise the greatest efficacy. In cholera it is said to be equally beneficial, but the facts, as observed in England, do not support the assertion, so far at least as they have been accurately reported. I am reluctant to give too much prominence to opinions, even though emanating from judicious men. Calomel, in one or two scruple doses, acts as a sedative, and tends to arrest the vomiting that is so trying to the patient. Some authors say that the vomiting is a curative process; I shall refer to this hereafter. I have relieved obstinate vomiting in other cases by a large dose of calomel; but it is a dangerous weapon when fever is absent, and occasionally produces excessive salivation. Calomel in large doses does not purge. There can be no doubt that the administration of calomel, in small or large doses, induces a very different therapeutic action; but I do not so clearly see that this difference can obtain when small doses of calomel are so frequently repeated, in accordance with Dr. Ayre's plan, as to throw into the stomach, within an hour, as much as would be introduced if given in a single dose. Small doses may be conceived to have a better chance of absorption; but Dr. Ayre says, that during collapse they are not absorbed. I doubt this, for I have observed, that in typhus fever, at least 24 hours elapsed after the administration of a large dose of calomel, before any change could be observed. An immediate effect should not be expected in cholera. On the whole, it appears to me, that the evidence respecting the utility of calomel in this disease, is very equivocal; and that it is still questionable, whether or not it exercises its specific action to control the disease. Unless as a specific it has no claims; and it is upon this ground alone, so far as I can ascertain, that it has been employed. I have great doubts of its beneficial influence, though I would not discourage further trials, and should be glad to have my doubts removed. It is said, that calomel in large doses, has been wonderfully efficacious in India; but what are the facts; the subject requires examination. In India, small doses are useless, and large doses cure; in England, large doses are injurious, and small doses cure! Surely a man may be forgiven if he venture to doubt. During the anterior and subsequent febrile stages, which frequently characterise the disease in England, calomel would doubtless be found of use. Under these circumstances, the disease should be treated by the ordinary rules of practice.

Opiates and Stimulants.— Any one who has taken the pains to review the history of the epidemic cholera, and carefully to compare the opinions that have been expressed in reference to its treatment, cannot hesitate to condemn the employment of either of these remedies for the cure of this terrible disease. By a reference to the foregoing tables, we find a gradually increasing rate of mortality under these several plans with their various combinations, and it would seem that the mortality, when calomel was employed, was augmented in due ratio as it was combined first with opium and then with stimulants. Under the stimulating system the mortality was highest, with the exception, perhaps, of the routine combination of calomel and opium and stimulants, when the mortality was literally murderous.

Venous Injection.—Of venous injection I shall merely, at present, say that its results are not sufficiently propitious to warrant any confidence in its remedial agency.—*Medical Times.*

On the Acidity and Alkalinity of certain of the Human Fluids in the state of Health and Disease. By M. ANDRAL.—In their physiological conditions, each of the humours of the body presents a certain degree of acidity or alkalinity; and the spontaneous transformation of a naturally acid fluid into an alkaline one, or *vice versa*, never takes place in the healthy organism. The utmost that can occur in this respect, is the rendering the fluid temporarily neutral by great dilution, as in the case of excessive perspiration—the water then being abstracted from the blood in larger proportion than the other principles. However this may be in health, the opinion is very generally entertained that in disease such chemical change in the humours does often take place; and the object of this paper is to investigate its accuracy.

Of all the fluids of the economy, the *serum of the blood* is the most decidedly alkaline; and whatever the nature of the disease or its duration, in which M. Andral has examined this fluid, he has never found the intensity of this reaction sensibly vary. Vogel quotes a case of metro-peritonitis from Scherer, in which the serum of the blood is said to be perfectly neutral, but adds, that he himself had never met with anything similar. If blood is examined after death, any acidity then found is the result of decomposition, and not the effect of disease. In examining the condition of fluids formed from the blood, it should be borne in mind that upon the same surfaces liquids possessed of different reactions may be found; so that the accidental predominance of one of these fluids may easily be mistaken for a change in the reaction of another. Thus the *sweat is acid*, but the *sebaceous matter is alkaline*. In the very various conditions in health and disease, under which M. Andral has examined the sweat, he has found it generally acid, sometimes from dilution neutral, never alkaline; out at the same time, at some parts of the skin, where sebaceous follicles abound, as the axilla and other hairy parts, an alkaline reaction may exist. It is evident, then, that the sweat is not a simple escape of the serum of the blood, charged with certain of its principles, for then it would be alkaline; and if the skin be irritated by blisters and the like, the fluid consequently effused will be found decidedly alkaline. So is the fluid found in herpes, eczema, pemphigus, &c., vesicular diseases preceded by more or less congestion of the skin; and it is remarkable that the contents of *sudamina*, which unlike these are preceded by no congestion, are acid, being also destitute of albumen, which is found in the others. Although sudamina are usually accompanied by excessive sweating, cases of typhoid fever are sometimes met with where this is not the case.

Still more remarkable is the difference of the reactions of the various fluids found on *mucous membranes*, giving rise to considerable chance of error. Throughout their whole extent they furnish, like the skin, an acid principle, which exists in the transparent fluid, destitute of globules, which they normally separate from the blood; but when this fluid is replaced by one of an opaque appearance and containing globules, secreted under the influence of acute or chronic inflammation, the reaction becomes decidedly alkaline. Few amimal fluids are so strongly alkaline as that furnished in coryza; and in bronchitis the acid and alkaline are not unfrequently found together, and yet remaining quite distinct in their transparent and opaque forms. The mucous membrane of the *mouth* and *tongue*, too, offers varieties of conditions. Examined in the morning, before food is taken, in the vast majority of cases, the fluid covering it is acid, but examined later in the day this is found to be alkaline. In the first case, it is due to the presence of the mucus; in the latter to that of the saliva. The acidity of the mouth is then no indication of a morbid condition of the stomach, occurring as it does in the healthiest persons, and in every variety of disease, and being distinct in proportion to the length of time food has been abstained from, and the secretion of the salivary glands has remained unexcited. The mucous membrane of the *stomach*, examined after death, generally furnishes an acid, sometimes a neutral, but never an alkaline reaction; and this whether it yet contains the remains of food, or whether digestion has been long suspended. How are we to reconcile this with the results of experiments which declare the fluids of the organ to be alkaline, save when stimulated by the presence of food or foreign bodies? This is not the case in *man;* for in the most opposite forms of disease the author has found acidity; and the great majority of matters rejected during life manifest the acid reaction. It is not rare to find this also in the duodenum and upper portions of small intestine; although these are often rendered alkaline by the arrival of the fluids from the liver and pancreas. Throughout the large intestine there is always marked alkalinity. The *tears* and *saliva* have always been found by M. Andral alkaline; and he believes that when this latter fluid has been said to be otherwise, that of the mucous membrane has been mistaken for it. Thus, in the very cases furnishing an acid reaction, if we, by means of a sapid body, excite the flow of the saliva, we immediately find this alkaline. "And thus falls to the ground one of the principal arguments which has been adduced in support of the theory which regards glucosuria as resulting from the acidification of the blood or other humours of the economy."

In a state of health, *urine* which has not remained too long in the bladder, and is examined soon after voiding, is always acid, although such acidity may become much enfeebled, or even neutralized, if very abundant drinks be taken without corresponding diaphoresis. Circumstances may render the urine temporarily alkaline, as the taking alkalies, or the prolonged use of exclusively herbivorous aliment. The privation of food, however long, does not remove the acidity of the urine; but it is a curious fact, that in some convalescents we find the urine become temporarily alkaline when they commence a better diet. Nor does disease render the urine alkaline. Multiplied as have been the author's observations upon this point, he has never met with a case in which the urine, from the influence of the disease itself, left the kidneys in an alkaline state; and he feels convinced that the statements which have been made to the contrary are founded in error. It has been said that diseases of the spinal marrow have this effect; but, in fact, the urine never becomes alkaline in these, until the mucous membrane of the bladder is diseased. It is not then an alteration of secretion, but a purely chemical one; the urine becoming decomposed and ammonical, from coming in contact with pus and other morbid products. *Pus*, whatever its source, is always alkaline, consisting as it does of the serum of the blood, amidst which special globules are developed; and this, as well as other morbid secretions, never becomes acid, except after long exposure to the air.

The immutability of the secretion of the acid and alkaline principles of the animal fluids is then a law of both their physiological and pathological conditions; and it must be a very important one, seeing that it persists without any exception, save one of a very temporary character, in respect to the influence of alimentary substances in the urine. —*Brit. and For. Med.-Chirurg. Rev.*, October, 1848, from *Gaz. Médicale*, 1848, No. 28.

SURGERY.

Impaction of a Halfpenny in the Pharynx for Eight Months. —A boy, aged one year and eight months, came under Dr. Ward's care, June 23rd, when his breathing was so loud and stridulous that it resounded through the hall in which he was waiting. As soon as Dr. Ward saw him, the child began to cry so convulsively, and was seized with such violent coughing, that a close examination of his throat was impossible. He was pale and emaciated, and seemed decidedly phthisical. The glands of the neck were somewhat enlarged, and the chest sounded well on percussion. His mother observed that he was quite well and hearty till March 3rd, when she supposed he swallowed a halfpenny with which he was playing, as he began to choke immediately, and the coin could not be found afterwards, and from that moment his breath became stridulous. She was then in Coventry barracks, and she took him to the regimental surgeon, who, thinking it an attack of irritation from teething, merely gave him some castor oil. At this time, besides the dyspnœa, he was constantly dribbling a thick mucus, and he could only suck one mouthful of milk at a time, being forced to withdraw from the breast with each effort of swallowing. The mucus was so profuse as almost to choke him; and these symptoms, with an increasing cough, continued for three months, till a short time before he came under Dr. Ward's care, when the dribbling had almost ceased. The mother next took him to the Coventry Hospital, where the case was again considered to be laryngismus from teething, and was treated accordingly. Dr. Ward concluded that the bronchial glands were effected with tuberculosis, as well as those of the neck, and, pressing on the recurrent, were causing the stridulous breathing. He therefore prescribed an iodine liniment, and the syrup of iodide of iron. Under this treatment the child rapidly improved, with occasional relapses, and thus seemed to confirm his diagnosis, when, on October 25th, his mother brought him, looking comparatively well, and produced the halfpenny, which she said he had taken out of his mouth and put into his father's hand after a severe fit of coughing, the day before. There is now, however, considerable hoarseness, when he cries or coughs, the latter symptom not having ceased with the cause. The coin was much worn and corroded, and covered with a layer of dried mucus.—*London Pathological Society in Medical Gazette.*

Employment of Collodion in Fistulous Openings.—Dr. Yvonneau, of Blois, relates the following case:—A little girl, five years old, was brought to me, whose right cheek was totally perforated very near the commissure of the lips, in consequence of an abscess which had burst externally, and which had, very neglectfully, not been opened early within the mouth. Both the upper and the lower jaw were attacked by caries, and the cheek had formed adhesions with the gums. The part had the shape of a funnel, the apex formed by a fistulous opening, about half an inch in diameter, throug hwhich the saliva and the liquid taken into the mouth were dribbling. The continual loss of saliva gave the child an inordinate appetite, in spite of which it was wasting very much. The breath was also extremely fœtid. I operated on the 11th of Oct., in the following manner:—After having rendered the child insensible by chloroform, freed the adhesions, and removed a portion of the lower jaw which was detached, I then included the fistulous opening in two semicircular incisions, and brought the lips of the

wound together. These were kept in contact by a twisted suture upon four needles and strips of adhesive plaster, and a roller around the chin finally contributed in keeping the parts steady. The whole of the dressings were, however, very soon so much soaked with saliva that they would have had to be changed every two or three hours to give a chance of keeping them clean. The third day after the operation I perceived that portions of the liquid which the child drank found their way through the fistula as previously; the parts had ulcerated upon the needle, and the fistulous opening was larger than before. Puzzled and annoyed at this failure, I bethought myself of using collodion, which I had employed before with benefit in the place of dextrine for applying bandages to fractures. I obtained perfect stillness in the child by chloroform, and the margin of the wound being exactly brought together by an assistant, I placed strips of very good adhesive plaister across it in an imbricated manner, which included the chin, the cheek, and the ala nasi, and by means of a camel-hair pencil I covered the whole with a layer of collodion. I expected thereby to render the plaster not only thoroughly adherent, but impermeable to the saliva. Three days afterwards I was obliged to remove the dressings, as the child had amused herself by pulling off a strip, and in doing so I was very agreeably surprised to find the wound covered with granulations, and the fistulous opening reduced to a line or two in diameter. Full of expectation, I re-applied the dressings in the same manner as above. I did not interfere with it for the five following days, when I cut away the apparatus, and found, instead of the ugly foramen, a clean linear cicatrix, which will be almost effaced by time. The collodion has evidently been of very great service here, for not only did it prevent the imbibition of the dressings from capillary attraction externally, but also effectually prevented the saliva from coming in contact with the external part of the wound, and thereby favoured the formation of granulations in the latter. It is not improbable that strips of adhesive plaster thus applied will very conveniently replace the needles, in the operation for hare-lip!—*Union Médicale* and *Lancet*, *Dec.* 23, 1848.

Means of Recognizing the Sensibility of the Retina in Certain Cases.—M. Cuthier when he wishes to ascertain whether the retina retains its sensibility, compresses the eyeball at one of its angles; if the luminous ring appears at the opposite corner, he decides that the sensibility of the retina is perfect, if it does not appear, the contrary is presumed. M. Cutheir has never observed this effect of internal pressure in amaurosis, but it is present in cataract and other defects of vision depending upon diseases of the transparent media of the eye.—*Annales d'Oculiste*, Avril, 1848.

On the employment of Gutta Percha in the Treatment of Strictures of the Urethra. By Henry J. Bigelow, M. D., one of the Surgeons of the Massachusetts General Hospital. This method consists essentially in the use of gutta percha in taking the impression of a stricture.*

There is, in general, no difficulty in the treatment of a stricture near the orifice of the urethra. On the other hand, a contraction of the canal far back towards the perineum often presents serious difficulties. The introduction of an instrument is then sometimes impracticable, or requires a tedious and very careful manipulation. It is plain that one great difficulty exists in the inability on the part of the surgeon to ascertain the precise character of the lesion; the geography of the part to be traversed by the bougie. It is well known that this contraction is susceptible of variation. It is abrupt or gradual, concentric or lateral, straight, angular, curved or spiral, smooth or knobbed, long or short, and finally partial or exaggerated; and against all these varieties the principal weapon in the hands of the surgeon is the bougie. This instrument, with little available variety, either in its material or conformation, is a point attenuated or obtuse, urged by a force applied at perhaps six inches distance; and is expected to thread its way through the complicated and winding labyrinth which often constitutes a stricture. Fortunately, the healthy canal traversed by the bougie generally so directs it, that when the contraction is not great, the point enters its orifice after more or less manipulation. Yet it will be conceded that this manipulation, however delicate and skilful, is often, and of necessity, only a series of tentative thrusts or offers, made in the dark in the hope of ultimately discovering and traversing some interval or interstice, should such exist.

Other circumstances, such as the density and character of the opposing tissue, and the necessity of employing or of avoiding protracted pressure, complicate the problem.

The common method, it is true, is often quite effectual and satisfactory; especially in the ordinary run of cases of simple or partial contraction. Yet there is something gross in it. It is wanting in the nicer modifications of art which should characterize surgical manipulation when they do not interfere with its simplicity. Nor are the results of this process always satisfactory; especially when the case is difficult, or the operator inexpert. It will soon be shown that false passage is very common in connection with old stricture; simply because the propelled instrument, finding no natural canal, has made one for itself. Or, as not unfrequently occurs, when the urine dribbles away, no canal can be detected and no instrument of dilatation passed.

These dificulties are not new. Different methods have been devised to bring the part to be operated upon more directly in contact with the tube to see it. Ducamp insisted upon the great advantage of impressions in wax, as conveying an idea of the conformation of a stricture, and contrived hollow tubes, containing centric bougies sliding out like a telescope at one side of the distorted canal.

Whoever has tried this wax has probably found, that however good the impression received in the interior may be, it is lost, either when the material is extricated from the stricture or subsequently from the canal. It is of questionable utility in this point of view. Besides the wax is soft and liable to break; and lastly, when moulded to the canal, it is itself of no use in dilating it, and another instrument of corresponding outline must be arranged for this purpose.

The advantages of gutta percha, are first, that it is probably the only material in the world capable of receiving an acute impression at a temperature quite comfortable to the skin, and at the same time of retaining it entirely, at about the actual temperature of the body; then becoming hard and resisting, besides being exceedingly tough, even in attenuated filaments. It follows, that upon being withdrawn from the urethra, it presents a perfect impression of the most minute inequalities of the callus against which it has impinged.

In the second place, it may be used when thus moulded as a dilator of the stricture; and it can be made to enter with unerring certainty any of its orifices.

A few words will suffice to describe the method I have adopted in employing these bougies. A medium size answers a good purpose, unless there be strictures anterior to the one to be treated, in which case a small calibre is sometimes requisite. Let the bougie be oiled and the tip passed to and fro rapidly in the edge of the flame of a candle, until it is so warm that the nail will indent it; the mass will remain plastic after the surface has ceased to be hot, and may be rapidly passed down to the stricture, being very smooth and

* The use of gutta percha bougies is not new; it is attributed to a physician at Singapore; but I have neither seen nor heard any allusion to their being employed to take impressions of strictures, which, so far as I can judge, constitutes their chief if not their only value. I have received within a day or two, through the politeness of Mr. Burnett, apothecary in Tremont Row, a sample of bougies with which he has been furnished by the Gutta Percha Company in New York, excellent in appearance, but which I have not had an opportunity of testing.

pliable. If it be pressed against the stricture for a minute with a force eqiuvalent to an ounce or two of weight, and then left to cool during the succeeding three or four minutes, it will present, when slowly and carefully disengaged from the stricture, a firm and unyielding impression of the most minute inequality and indentations of the callous surface. The tip may be cut off and preserved, furnishing, with others, a complete history of the conformation of the stricture under treatment.

If water be employed to heat the gum, it will be found that the steam from the surface will soften the rod for the length of an inch or more; rendering it liable to curl up against the stricture, as small elastic bougies are apt to do. The tip alone should be softened. On the other hand care should be taken not to burn the gum: its texture and ability to harden are thus destroyed, and a piece may be left in the stricture. Such a case occurred to me. A plug was thus left in a small stricture, causing retention of urine during eighteen hours; when the orifice having become dilated, the plug was forced out by the urine; which then flowed more freely than for many months before.

Pure gutta percha softens most readily and cools with least elasticity and shrinking. It is therefore far better for impressions than when adulterated, as is common, with caoutchouc. But when pure, a little oiling and use soon raises a a fur upon its surface; so that it is probable that some compound will answer better for mere bougies.

I have hitherto made these rods from pure gum, of the thickness of sole leather, cut into square strips, plunged into boiling water, and rolled between two boards, care being taken to prevent twisting.

When the bougie is imbedded in the stricture, let its head, or external end be warmed and flattened in a vertical or transverse direction with reference to the pubis, and it will indicate, when withdrawn, the position of the inequalities in regard to the periphery of the canal.

Suppose now that the impression, as is frequently the case, is forked. Examination of the extremities often indicates which is the true passage and which the false. Let the false extremity be carefully shaved off and the bougie returned into the urethra, its flattened head maintaining its relative position to pubis. It forms a conical bougie of the best description, exactly adapted to the form of the true passage, which it inevitably enters. Impressions also record and especially direct the progress of a cutting instrument, as seen in the annexed sketches.

The general pathology of stricture is not here discussed; but it will be quite obvious that there are cases of irritable and inflammatory stricture in which this method of dilatation, as well as all other active mechanical treatment, would be inappropriate. Nor are the relative merits of dilatation, incision and cauterization here considered. Each is occasionally a valuable resource; the progress of all is incalculably aided by the knowledge derived from impressions; while the first by the far most valuable mode of treatment is considerably accelerated by the actual employment of the gutta percha.

[The author gives some cases in elucidation of his proposed practice which it is unnecessary to copy.]—*Boston Med. and Surgical Journal.*

Influence of Ætherization on the Mortality of Surgical operations.—In the April number of the *Monthly Journal*, Dr. Simpson has published tables showing the influence of ætherization on operations. From these it appears that of 230 primary amputations of the thigh, leg, and arm, performed in the British Hospitals *without the use of æther*, there were 88 deaths, or 38 per cent.; of 388 secondary operations, 95 deaths, or 24 per cent.

Of 302 amputations of the thigh, leg, and arm, *under ætherization*, 73 were primary, and 25 deaths followed, or 34 per cent; 229 were secondary, 46 deaths, or 20 per cent. only.

In amputations of the thigh alone the difference is more marked, the per centage being as follows:—*Without æther*, in the Parisian Hospitals, 62 in 100; in the British Hospitals, 38 in 100. *Under ætherization*, 25 in 100.—*Monthly Journal.*

MATERIA MEDICA AND CHEMISTRY.

Local application of Chloroform in Lumbago.—Three cases of this disease are detailed by M. Moreau, in which immediate and permanent relief was obtained by the application to the loins of a piece of lint on which some chloroform had been poured. Oiled silk ought be laid above the lint, to prevent the evaporation of the chloroform. In a few minutes the patient complains of a burning heat in the part, which becomes red, and occasionally vesicles are formed; at the same time the rheumatic pain disappears. The author thinks that the cure cannot be attributed solely to the counter-irritation, as in one of the cases recorded sinapisms had been previously employed, without success. He supposes the chloroform to reach by imbibition the cutaneous and superficial muscular nerves, on which it exerts its anæsthetic power.—*L'Union Méd., Oct.* 21, 1848.

In a case of pelvic tumour, where the patient suffered from severe pains of the inferior extremities, probably in consequence of the nerves being compressed in traversing the pelvis, the usual means of affording relief having failed of success, M. Legroux determined to try the local application of chloroform, A sponge containing chloroform was placed in the foot of a large boot of wax-cloth, constructed for the purpose, so that the vapour only came into contact with the skin. A feeling of warmth, prickling, and numbness, was soon experienced. The application was continued for several hours, when complete anæsthesia was established, and the neuralgic pains had entirely ceased. The absence of pain continued several days, and the same treatment was equally successful on its return.—*Ib., October* 31, 1848, *and Monthly Journal*

Quinine a prophylactic of Puerperal Fever.—During an epidemic of puerperal fever, which occurred in the hospital of Rouen in 1843, the thought occurred to Dr. Leudet of ascertaining, by experiment, whether quinine possessed the power of enabling the economy to resist the contagion of this disease. From the 21st September, 1843, to the 8th January, 1844, eighty-three women were delivered in the Hotel Dieu of Rouen; in nine of these women to whom quinine was given, not one case of puerperal fever occurred, while of the remaining seventy-four, who received no special treatment, twenty-one suffered from the disease. In two later epidemics, its utility was subjected to a more extensive trial. From the 8th of July to the 9th of August, 1845, of twenty-six cases of delivery, fifteen were treated with quinine; one only of these was attacked, while of the remaining eleven, eight had puerperal fever. Lastly, during an epidemic which prevailed in Rouen, from the 19th of March to the 21st of April, 1846, there were thirty-six deliveries. Quinine was prescribed to seventeen women, only one of whom had fever, while of the nineteen who were submitted to no special treatment, eleven were attacked with the disease.

Dr. Leudet begins the prophylactic treatment about four hours after delivery, by the administration of five grains of quinine, which dose is repeated twice during the same day, at intervals of five hours. On the second day the same doses are given; but on the third day they are diminished to three grains thrice daily, and are so continued for four days more.

This method is adapted for the more common form of epidemic, where the fever does not present itself for three or four days after delivery. When it appears during parturition, or immediately after it, Dr. Leudet advises that the use of quinine should being with the first symptoms of labour.—*Thèses de Paris*, 1847, *from Monthly Journal.*

Effects of Quinine in Large Doses.—M. Briquet, prevented a memoir entitled "Experimental Researches on the properties of Quinine in Large Doses, and its Therapeutical Uses." By large doses, the author means from fifteen to thirty grains.

In his researches, the author studies in succession the effects of large doses of quinine on the principal organs of the economy as for instance on the organs of circulation, on the nervous centres, organs of respiration, digestion, generation, &c

According to M. Briquet, the organs of circulation present two especial modifications; the first, which affects the number of pulsations of the heart, shews that these may be diminished from eight to forty beats in the course of a few days. The force of the pulse he has studied with the aid of the hæmadynometer of M. Poiseuille, applied to the carotid at the same time that quinine is injected into the jugular veins. He has thus ascertained that the force may be diminished in all degrees from one-tenth to complete annihilation, according to the doses injected, and that thirty grains is sufficient to cause immediate death. He has therefore concluded that quinine exercises a directly debilitating effect on the heart's action.

The brain and its dependencies exhibited different phenomena according as the quinine reached the brain directly or indirectly. When injected into the carotid arteries, it produced excitement, and in some cases convulsions followed by prostration, but if injected into the aorta, there was less excitement, and a more rapidly sedative effect.

The respiratory organs were not sensibly influenced; the digestive organs were slightly injected when a moderate dose had been given, and were vividly inflamed when large doses had been given several days in succession.

M. Briquet has never known abortion produced by quinine, as has been maintained by some.

From the study of the physiological action of quinine, the author deduces his views of its therapeutical value. Thus he supposes that from its power of reducing the pulse, it may be useful in the pyrexiæ in rheumatism, gout, erysipelas, and other diseases accompanied by high action. The action on the brain may be taken advantage of, in cerebral neurosis, periodic neuralgia, &c,

Children and young people are readily influenced by quinine, and its absorption is rapid, but they can bear larger doses than adults. Bleeding augments the susceptibility to the effects of quinine in a notable manner. After some further remarks on the action of opium and coffee, when given in conjunction with quinine, the author draws the general conclusion:—

1. That quinine depresses nervous power, especially that of organic life.

2 That it is a direct excitant of the organs with which it comes into contact, in this respect resembling æther and chloroform.—*Prov. Med. and Surg. Journal.*

MISCELLANEOUS.

GENERAL AND MEDICAL INTELLIGENCE.

The *St. Johns News* states, that the small pox is raging fearfully in that city, there being over 1300 cases under treatment.—Eleven medical journals are published in Italy.—Dr. Clutterbuck lately stated to the Medical Society of London, that he had witnessed most beneficial effects from chloroform in cholera. It relieved the spasms, and composed the patient.—Death has been busy among eminent medical men. Samuel Cooper, Professor of Surgery in University College, and well known for his Surgical works, died on the 3d December last, aged 68 years.—On the 5th December died Dr. Clendinning, in his 51st year.—At Brighton, Thomas Callaway, in the 58th year of his age, for many years one of the Assistant Surgeons of Guy's Hospital; and at Dublin, on November 11, Dr. Renney, aged 92, Director General and Chief of the Army Medical Department in Ireland.—It is stated that Dr. Chomel, who has one of the largest practices in Paris, had, one day lately, not a single patient to visit.—An ink, containing iodine, has been lately fraudulently used; all traces of writing by it disappear in a few days.—In Paris, the ratio of indigent, to the population, is as 1 to 12, and of 24,000 deaths, 9,000 have died in the hospitals.—There are, in Paris, 15 hospitals, comprising, altogether, 7,174 beds, and receiving, annually, 90,000 patients. There are four large hospitals, and seven infirmaries for 8,000 old and infirm of both sexes. More than 100,000 receive assistance at their own houses, and about 25,000 children are found abandoned by their parents. The hospitals are of two kinds, general and special. In the first, only, are treated acute diseases, wounds, &c; the diseases admitted into the second require special treatment. There are ten general hospitals, and seven special. The names of the general hospitals, with the number of beds which they respectively contain, are as follow:—Hotel Dieu, 810; Sainte Marguerite, 300; La Pitiè, 621; La Charité, 494; St. Antoine, 320; Necker, 329; Cochin, 125; Beaujon, 438; Bonsecours, 323; De la Republique, 600. The following are the special hospitals, with the number of their beds:—Saint Louis, 825; Du Midi, 300; De Loursine, 300; Des Enfans Trouvés, 600; Maison d'Accouchement, 514; Maison de Chirurgiens, 120; Maison de Santé, 150: in all, 7,174 beds.—*La Gazette Medicale.*—It is said that a good deal of sickness exists among the emigrants in California.—The fees of the Sardinian physicians are fixed by law. Nine pence is the charge for a short visit. In China a salary is paid to the physician while the patient remains in a state of health.—A Mr. Yates died in Manchester, Ohio, the other day, aged 113 years.—*Cholera.*—This fell disease still continues its ravages in Great Britain. In Ireland it has appeared in Derry and Donaghadee, subsequent to its first appearance in Belfast, imported from Edinburgh. Up to 18th January, 131 cases had occurred in the latter city, of which 61 died, 45 cured, and remainder under treatment. In Scotland it has appeared in Edinburgh, Paisley, Barrhead, Haddington, Dumbarton, Greenock, Carnbrae, Prestwick-Toll, Saltcoats, Steventon, Dalry, Kilmarnock, Kilwinning, Stirling, Peebles, and Doune. In Glasgow, the interments from cholera alone, amounted to 100 per day. In England, although the disease is not extinguished, still the progress is by no means in the same proportion. We fear, that as soon as the winter is over, it will break out with increased malignity. In Russia, from official documents, the malignity and mortality appear to have been fearful. During the period of *seven months*, from April to Nov., 1848, the total number attacked was 1,643,282, and of this number 650,720 died. On this Continent the disease has appeared in five places. Cases have occurred in New York, Memphis, Cincinnati, Mobile and New Orleans, raging epidemically only in the latter city. The disease is, meanwhile, arrested on this Continent.—We take the following from the *Southern Med. and Surg. Jour.* of Jan. 1849. We would like some of our city homœpaths to answer it: *Wonderful Effects of "Calcarea Carbonica" Homœopathically used.*—"A young lad, aged fifteen, extremely psoric, had remained exceedingly small and thin; his limbs were very slight, and his head too large for the rest of his body. He suffered from violent headaches when making any mental exertion; in his childhood he had suffered from feebleness of the limbs; he was very timid, especially at night; he could not bear to be left alone in the dark. Two doses of *Calcarea at forty-five day's interval*, after one dose of *Sulphur*, brought about such a favorable change in his constitution, that, in *six months*, his height, which had hitherto increased only from six to eight lines per annum, gained *four inches*; his limbs, the hands and feet in particular, had become large and strong, like those of a young man who would grow to the ordinary height." A smart boy, that, and up to—*chalk!* Only think of it. The next case by the same writer, is one of *cyanosis* in a girl seven years old, who "presented all the appearance of abnormal permeability of the ductus arteriosus." * * * * * * "*A globule of the 30th dilution of calcarea effected a radical cure in six weeks, probably by restoring the abnormal part to its proper state!*" Think of it again. Infinitely less than the ten thousand millionth part of the duodecillionth of a grain of chalk, will make a boy grow four inches in six months, or close up an open ductus arteriosus in six weeks!! The good book asks us in a very positive way, "who by taking thought, can add one cubit to his stature?" Answer: We can't by taking *thought*, but we can by taking *chalk and brimstone!* Reader of ours, do not imagine that we have taken the above cases from the renowned works of the famous Baron Munchausen, or the true histories of Lemuel Gulliver, Esq.; they are truly quoted from an address by the learned Dr. Croserio, before the Société Hahnemannienne, in the Sept. No. of the American Journal of Homœopathy, published in New York City. The same astute philosopher, in closing his address, gives a last advice, (we should suppose he couldn't long survive it!) "to exercise prudence in its administration," for, says he, "this remedy

(calcarea carbonica,) is one of the most energetic, and in spite of its peculiar adaptation to infantile disease, we should be very circumspect in our doses, especially at that period of life and in old age, for even at the end of *six weeks* it often produces very violent primitive symptoms, which might be attended with danger, if the doses given were too strong." Further on he says, "with respect to the duration of the action of calcarea, it is very long. When it is *very homœopathic* we may look for salutary effects *for six weeks and longer!*" Gentle reader, if you have rickets, scrofula, lupus, neuralgia, chorea, headache, cyanosis, "big head," or any other of the numerous affections for which calcarea is homœopathically administered, take a globule of the 30th dilution, and if six weeks afterward, you have a troublesome borborygmus, a twinge of the toothache, or a "crick in the back," go and make your *affidavit* that the chalk did it, and you will contribute to that great mass of evidence upon which homœopathy, as a system is built. For ourselves, we can't understand how any man in his sense can swallow and believe such nonsense. It strikes us, that some, at least, of the converts to, and advocates of, this system of moonshine would be much benefitted by some 'very homœopathic' article, which would produce what an old quack in Springfield declares he can cure—"*information on the brain!*"—*Ohio Medical Jour.*

THE
British American Journal.

MONTREAL, MARCH 1, 1849.

PROCEEDINGS ON MEDICAL AFFAIRS IN THE LEGISLATIVE ASSEMBLY.

Since our last issue, medical matters are assuming a definite shape. The petition of B. H. Charlebois and others, has been presented, and a committee has been appointed by the House, consisting of the Honble. L. J. Papineau and Mr. M'Connell, and Drs. Taché, Beaubien, Fortier, Boutillier, and Davignon. The following questions have been submitted by the committee to a number of medical gentlemen in this city, Quebec, and the country districts, with the request to furnish answers in writing at their earliest convenience.

Question 1. What is your opinion with respect to the law now in force regulating the practice of medicine?

2. Do you consider that law calculated to meet the wants of the medical profession?

3. What are the defects in the present law?

4. Do you think that law requires amendments, or that a new law is necessary?

5. Do the Governors of the College established under that law represent the medical profession?

6. Will you have the goodness to state your ideas with respect to the organization of the profession, and the legislative provisions necessary to meet its present wants?

7. Do you not think the law now in force might remain in operation as regards the members of the present college, after depriving that law of its effect on the whole of the profession?

The matters involved in the above being of the utmost importance to a very large and respectable part of the community, we sincerely hope that the committee will print the evidence which may be submitted to them.

A petition to the House, praying for an act of incorporation for the profession of Upper Canada, has been presented by, we believe, Mr. Sherwood. We have not yet seen the bill which is to follow the petition.

Our dear friends, the Thompsonians, are again in the field, and have presented a petition praying for an act to legalize their practice. Among the names appended, we notice that of a minister of the Methodist persuasion in this city, and also that of a druggist, who pretends, and that not slightly, to the countenance and favour of the profession here. The names of these parties are at the service of any gentleman who wishes them. Our profession deserves better at the hand of its sister profession than to see its ministers crusading against it, in support of quackery, and that of the worst description. The youth of the druggist is the only palliation for the indiscretion which he has committed. Under other circumstances we would have mentioned him by name.

Inspector General of Hospitals.—A few months ago it was currently rumoured that the ministry intended the appointment of an officer for the purpose of exercising a general supervision over all medical institutions in the Province. We think that the profession and the public generally would approve of such a step; and the circumstances which have transpired at the Toronto Lunatic Asylum, would not only seem to indicate the strong necessity of such an appointment, but would have justified it. The necessary investigations into the management of that institution, which would appear to have been for a long time exceedingly bad, could have been conducted with the utmost facility. In consequence of the expense, Dr. Park has been denied a hearing by a commission, and, as his only recourse, upon a petition presented to the Legislative Assembly, a committee of investigation has been appointed. We await their decision in the matter, and look forward to it with interest.

Sanitary Measures for the City.—It is now about two months since we directed attention to the imperious necessity which exists for the adoption of general sanitary precautions, in anticipation of the advent of cholera. The Corporation has not made one single move towards the attainment, or in pursuance of so desirable an end. If the disease is obedient to the same laws which have characterised it in its march in 1832 and 1834, we have no reasonable grounds for not expecting a visitation of it next summer, or, in all probability, this spring. The filth of yards is scattered about in

all directions, to become, with the heat of our vernal or summer sun, pestilential foci. Why such a practice is permitted at any time is to us a mystery, how much more so when the city is threatened with a desolating plague. The facility of placing such materials on the ice opposite the town, to be carried off with its breaking up, is equally as great, as placing them where they are now tolerated. Sanitary measures have been adopted in Quebec, Toronto, and, we believe, also in Kingston. These cities are all under surveillance—medical men being members of their Corporations. This matter being one of every body's business in our Corporation, is undertaken by no one; and it will not be until the "wolf is at the door," that any activity is likely to be displayed, or the slightest means taken to secure the maintenance of the general health, by the adoption of any sanitary precaution.

BOOKS, &c., RECEIVED.

Lee's Clinical Midwifery. Philadelphia, Lea & Blanchard. 1849.

Lecture on Obstetrics and the Diseases of Women and Children, by G. S. Bedford, M.D., Prof. New York. 1849.

Essays on Infant Therapeutics, &c., by J. B. Beck, M.D. New York. 1849.

Summary of the Transactions of the College of Physicians of Philadelphia, Sept., 1848, to January, 1849.

Our usual Exchanges, mainly through the parcel of Messrs. Wood & Co.

TO CORRESPONDENTS.

Letters have been received from Dr. Douglas, Quebec; Gilbert, Hatley; Wright, Dublin; Gibb, London; Messrs. Wood & Co., New York; Captain Lefroy, Toronto; Dr. Beck, Albany—the missing number has been sent. The following communications have come to hand:—Transatlantic Letters, by Dr. Wright; Case of Cystosarcomatous Tumour in the Abdomen, by Dr. Crawford; Case of Lacerated Wound within the Orbit, by Dr. Griffin, Quebec.

Desirous of rendering the periscopic department as full as possible, and to compensate for deficiencies in this respect, from the crowd of original matter in past numbers, we have concluded upon omitting a series of bibliographical notices, with other Editorial matter, for this issue. Were our space double, we could more than fill it.

TO SUBSCRIBERS.

Mr. Wood is on a tour in the Upper Province, collecting for this Journal, in addition to Mr. Wadsworth.

MONTHLY METEOROLOGICAL REGISTER AT MONTREAL FOR JANUARY, 1849.

Date.	Thermometer.				Barometer.				Winds.			Weather.		
	7 A.M.	3 P.M.	10 P.M.	Mean.	7 A.M.	3 P.M.	10 P.M.	Mean	7 A.M.	Noon.	6 P.M.	7 A.M.	3 P.M.	10 P M
1,	+ 5	+ 8	+ 3	+ 6.5	30.11	30.03	29.93	30.02	W	W N W	W N W	Fair	Fair	o'erc'st
2,	—10	" 5	— 5	— 2.5	29.99	29.86	29.80	29.88	W	W	W	Fair	Fair	Fair
3,	— 2	" 7	+ 2	+ 2.5	29.72	29.85	29.57	29.71	W	N W	N W	Fair	Fair	Fair
4,	— 1	" 3	" 4	" 1.–	29.56	29 43	29.38	29.46	W N W	W N W	W	Fair	Cloudy	Fair
5,	+ 6	" 17	" 18	" 11.5	29.33	29.29	29.35	29.32	W	W	W	Snow	Snow	Fair
6,	" 16	" 22	" 14	" 19.–	29.61	29 73	29.82	29.72	W	W by S	W by S	Fair	o'erc'st	Fair
7,	" 9	" 15	" 3	" 12.–	29.93	30.09	30.02	29.98	W	W	W	Fair	Fair	Fair
8,	" 2	" 13	" 10	" 7.5	30.01	29.85	29.84	29.91	W	W	W	o'erc'st	o'erc'st	Fair
9,	" 0	" 7	— 4	" 3.5	29.85	29.87	29 90	29.87	N W	N	N	Fair	Fair	Fair
10,	— 7	" 3	— 7	— 2.–	29.99	30.02	30.12	30.04	N	N W	N W	Fair	Fair	Fair
11,	— 8	" 3	— 1	— 2.5	30.27	30.37	30.46	30.37	N W	N W	W	Fair	Fair	Fair
12,	— 5	" 14	+10	+ 4.5	30.50	30.46	30.27	30 41	W S W	W S W	W by S	Fair	Fair	o'erc'st
13,	+13	" 29	" 30	" 21.–	30.07	29.79	29.70	29.85	W by S	S	S	Fair	Fair	o'erc'st
14,	" 36	" 36	" 25	" 36.–	29.43	29.53	29.93	29.63	S W	S W	W	Rain	Fair	Cloudy
15,	" 15	" 18	" 22	" 16.5	29.99	29.30	29.22	29.50	E	E N E	E by N	Cloudy	Snow	Snow
16,	" 20	" 25	" 17	" 22.5	29.75	29.74	29.79	29.76	W N W	N W	W	Snow	Fair	Fair
17,	" 23	" 30	" 2	" 26.5	29.44	29.38	29.78	29.53	S	W by N	W	o'erc'st	Cloudy	Fair
18,	—12	— 1	—10	— 6.5	30.06	30.23	30.51	30.27	N W	N W	N N W	Fair	Fair	Fair
19,	—15	+ 1	— 4	— 7.–	30.72	30.64	30.52	30.63	W N W	W by N	W by N	Fair	Fair	Fair
20,	+ 6	" 25	+24	+15.5	30.33	29.93	29.87	30.04	S	S W	S W	Fair	Fair	Fair
21,	" 27	" 15	— 3	" 21.–	30.02	30.04	30.17	30.08	W N W	W N W	W N W	Fair	Fair	Fair
22,	—11	" 8	+17	— 1.5	30.22	29 94	29.70	29.95	N E	N E	N by W	Fair	Fair	Snow
23,	+18	" 26	" 21	+22.–	29.78	29.88	29.75	29.80	N N W	N N W	N W	Fair	Fair	o'erc'st
24,	" 29	" 36	" 29	" 32.5	29.84	29.85	29.85	29.85	S	S S W	W	Fair	o'erc'st	Snow
25,	" 29	" 35	" 30	" 32.–	29.80	29.68	29.50	29.66	S W	S W	N	Snow	Sleet	Rain
26,	" 30	" 37	" 10	" 33.5	29.25	29.17	29.64	29.35	N	S W	W	Rain	Snow	Fair
27,	" 4	" 11	" 5	" 7.5	30.08	30.16	30.24	30.16	W	W	W	Fair	Fair	Fair
28,	— 3	" 13	" 9	" 5.–	30.35	30.21	30.02	31.19	N W by W	W N W	W N W	Fair	Fair	Snow
29,	+12	" 21	" 30	" 16.5	29.89	29.56	29.45	29.63	N W	N	N	Fair	Snow	Snow
30,	" 14	" 13	— 3	" 13.5	29.65	29 90	30.09	29.88	N by W	N W	W	Fair	Fair	Fair
31,	—15	" 6	— 5	— 4.5	30.35	30.36	30.34	30.35	W	W	W	Fair	Fair	Fair

Therm. { Max. Temp., +37° on the 26th
Min. " —15° " 19th and 31st
Mean of the Month, +11.7

Barometer, { Maximum, 30.72 In. on the 19th
Minimum, 29.17 " 26th
Mean of Month, 29.897 Inches.

MONTHLY METEOROLOGICAL REGISTER AT H. M. MAGNETICAL OBSERVATORY, TORONTO, C. W.,—JANUARY, 1849.

Latitude 43°. 39′.4. N. Longitude 79°. 21′.5. W. Elevation above Lake Ontario, 108 Feet.—(For the Brit. Amer. Jour. of Med. and Phys. Science.)

Day.	Barometer at Temp. of 32°.				Temperature of the Air.				Tension of Vapour.				Humidity of the Air.				Wind.			Snow in. on surf.	Weather.
	7 A.M.	3 P.M.	10 P.M.	Mean of 24 h.	7 A.M.	3 P.M.	10P.M.	Mean of 24h	7A M	3 P.M	10P.M	Mean of 24h	7A.M	3P.M	10P.M	Mean of 24h	7 A.M.	3 P.M.	10 P. M.		
1,	29.958	29.734	29.836	29.851	17.5°	17.0°	11.0°	14.7	.071	.082	.043	.067	.70	.82	.62	.73	E. N. E.	E. N. E.	N.E. by N.	—	O'rcast all day: snowing from 4-9 p.m
2,	30.078	30.032	30.008	30.038	4.2	6.6	4.2	1.1	.029	.049	.024	.031	.73	.75	.58	.72	N. N. E.	W. by S.	Calm.	3.5	Generally clr, a few light c'ds dispersed
3,	29.884	29.753	29.666	29.768	8.6	16.2	0.7	8.5	.051	.057	.041	.053	.74	.59	.83	.75	Calm.	W. N. W.	W. N. W.	—	Cl'ded till 1 p.m.: mostly clcr fr 2 p.m.
4,	29.554	29.428	29.407	29.460	10.8	22.3	22.0	18.2	.059	.099	.101	.086	.77	.81	.83	.81	Calm.	N. W.	N.W.by W	—	O'ct all day: Sl't snow fr 10 am to 5 pm
5,	29.438	29.499	29.561	29.494	17.0	23.4	24.4	21.5	.069	.093	.110	.090	.68	.72	.82	.74	N.W.by W	N. N. W.	N. N. W.	1.0	Generally clouded: clear spaces
6,	29.702	29.793	29.880	29.783	19.4	21.0	15.8	18.7	.099	.083	.077	.085	.91	.71	.82	.79	N by W	N. by W.	N. by W.	—	Cl'd all day: particles of s. fal'g occas'ly
7,	29.919	29.860	—	—	13.0	17.8	—	—	.074	.089	—	—	.88	.86	—	—	Calm.	Calm.	——	not ap	Overcast all day
8,	29.700	29.625	29.640	29.657	20.8	26.6	21.8	23.4	.093	.138	.102	.108	.81	.94	.84	.83	N. E. by E	S.	Calm.	—	O't till 9 pm.: Snowing fr 9 am to 5 pm
9,	29.708	29.738	29.830	29.756	15.0	11.5	4.0	10.3	.065	.051	.052	.058	.71	.69	.90	.77	N. N. W.	N.	N. by E.	1.2	Cl'd till 1 pm.: rem'der of day lt dl'd cl'd
10,	29.979	30.059	30.150	30 059	3.7	2.0	2.6	1.5	.027	.040	.028	.032	.68	.78	.70	.70	N.W.by N.	N. by W.	N. by E.	—	Partially cl'ded am: clear fr 3 pm: fine
11,	30.280	30.280	30.313	30.289	14.2	8.4	0.6	2.1	.016	.050	.022	.028	.69	.74	.47	.59	Calm.	Calm.	N. by W.	—	Gen'ly clear, a few light clouds pm, fine
12,	30.240	30.054	29.908	30.067	3.4	23.8	24.2	17.1	.042	.083	.102	.079	.68	.64	.76	.73	N. N. E	S. S. E.	Calm.	—	Mostly clouded, clr spaces occasionally
13,	29.700	29.582	29.445	29.574	33.4	36.0	35.8	34.9	.136	.175	.171	.160	.71	.83	.82	.78	S.	S.W.by S	S. W.	—	Densely o'cast, slight rain 5 to 9 pm
14,	29.596	29.781	—	—	31.1	28.4	—	—	.134	.139	—	—	.77	.89	—	—	W.S.W.	W.	——	*	Gen. clr, fine, faint auroral arch N 7 pm
15,	29.566	29.203	29.558	29.440	24.0	34.0	22 8	26.8	.108	.140	.102	.118	.82	.71	.81	.79	N. E.	W. S. W.	W. by S.	0.5	Densely cl'ded, snow and sleet till 10 am
16,	29.738	29.797	29.636	29.716	17.4	21.9	20.6	20.1	.086	.065	.081	.084	.86	.56	.71	.75	S.W.by W	W. S. W.	Calm.	—	Am cl'd, pm clear & unclouded, fine
17,	29.425	29.628	29.905	29.641	26.4	22.0	10.5	20.4	.104	.107	.064	.098	.72	.88	.85	.78	W. by S.	N. W.	N. W.	—	Light cl'ds occasionally, m'stly clr, fine
18,	30.104	30.229	30.401	30.239	5.0	16.2	12.0	10.9	.047	.058	.053	.055	.79	.61	.66	.72	Calm.	W. N. W.	W.	—	Gen o'st, particles of snow occasionally
19,	30.492	30.377	30.204	30.354	8.4	19.2	16.8	14.5	.055	.065	.069	.062	.79	.60	.69	.69	N. N. E.	S.W.by S.	W. by S.	not ap	Mostly o'cst till 1 pm, detached c'ds pm
20,	29.930	29.738	29.822	29.837	14.2	27.4	28.0	23.4	.069	.116	.110	.100	.76	.76	.71	.75	Calm.	S.W.by W	W. by S.	—	O'cast all day, particles of s. 1 to 3 pm
21,	29.914	29.968	—	—	28 0	22.0	—	—	.122	.105	—	—	.78	.87	—	—	W. N. W.	N. N. W.	——	not ap	Dn'ly cl'd all day, particles of snow pm
22,	29.999	29.845	29.767	29.870	11.0	23.5	22.0	18.6	.061	.080	.090	.079	.80	.62	.74	.74	Calm.	W.S.W.	W. S. W.	—	O'cst till 9 pm, auroral light 10 & 11 pm
23,	29.803	29.758	29.717	29.762	18.4	29.1	28.6	25 1	.087	.105	.104	.099	.83	.65	.64	.71	W. S. W.	S.W.by S.	W. by S.	—	Partially clouded
24,	29.724	29.652	29.687	29.688	30.8	32.7	32.3	31.8	.133	.159	.158	.151	.78	.86	.87	.84	W. S. W.	S. W.	Calm.	—	Den'ly cl'd all day, snowing fr 1-4 pm
25,	29.633	29.361	29.197	29.398	33.4	36.9	35.6	35.6	.167	.190	.187	.181	.88	.87	.90	.87	Calm.	E. by N.	Calm.	2.5†	Den'ly cl'd all day, moderate r. m't day
26,	29.074	29.550	29.903	29.493	38.0	26.4	21.4	28.9	.193	.109	.087	.132	.85	.75	.74	.78	W. by S.	N. W.	N.W.	‡	Den'ly cl'd, sl't r'n am, particles sw. pm
27,	30.015	30.066	30.061	30.042	19.3	24.2	24.0	22.5	.088	.084	.085	.086	.81	.64	.64	.70	Calm.	Calm.	Calm.	not ap	Densely clouded all day.
28,	29.952	29.770	—	—	27.1	29.6	—	—	.113	.139	—	—	.75	.84	—	—	S. by E.	S.E. by S.	——	—	Clouded, slight snow pm
29,	29.581	29.421	29.475	29.495	30.0	35.6	32.3	32.6	.141	.175	.151	.158	.84	.84	.82	.85	Calm.	Calm.	N.	0.2	D'ly cl'd, slt s 6-7 am, slt r'n fr 6-10 pm
30,	29.725	29.884	30.012	29.859	22.8	13.8	9.0	15.3	.102	.054	.039	.070	.82	.64	.57	.74	N. N. W.	N.W.by N.	N. by W.	0.3*	Den'ly cl'd, snowing from 6-16 am
31,	30.185	30.126	29.872	30.050	0.0	12.4	17.6	10.0	.034	.053	.070	.055	.72	.66	.69	.72	N.	N.E.by N.	S. E.	—	G'ly cl'd, sn'ing fr 2 pm till 7 am Feb 1
Mean	29.823	29.786	29.810	29.803	15.7	21.9	18.0	18.49	.083	.095	0.086	0.089	.77	.73	.74	.75	5.33 miles	8.50 miles	5.89 miles.	9.2	

Highest Barometer, 30.502 on 19th, at 9 a.m. } Monthly
Lowest do. 29.016 on 26th, at 6 a.m. } Range1.456
Highest Temperature, 39°.5 on 26th, at 6 a.m. } Monthly
Lowest do. .. 14°.2 on 11th, at 7 a.m. } Range 53.7
Mean Max. Therm., 21.53—Mean Min. Therm., 13.08.
Mean Daily Range, .. 11°.45
Extreme Daily Range, 31° .2 from am to 9 pm, on 12th
Highest Barometer observed at Toronto since 1840, 19th 8h 40m am, 30.508. Corrected to 32°.

Proportion of Wind from each Quarter—
From N. 2099.9 miles.
W. 2607.9 "
S. 1067.5 "
E. 581.9 "
Mean velocity of the Wind, 6.71 miles per hour.
Maximum velocity, 24.6 miles from 5 to 6 pm, on 15th
Most Windy Day, 15th.—Mean veloc per hour, 14. 66 miles
Least do. 27th. " " 1.55

Temperature for January.

Year.	Mean.	Max.	Min.	Range.
1840, ...	17.8°	36.7°	18.6°	55.3°
1841, ...	25.6	42.3	6.4	48.7
1842, ...	27.8	49.4	1.9	47.5
1843, ...	23.76	55.4	1.8	53.6
1844, ...	20.67	45.3	7.2	52.5
1845, ...	26.68	45.7	0.2	45.9
1846, ...	26.10	44.0	1.3	45.3
1847, ...	22.87	42.9	2.9	45.8
1848, ...	27.41	51.1	11.4	62.5
1849, ...	18.49	39.5	14.2	53.7

	Rain.		Wind.		Snow.		
	No. Days.	Inches.	Winds.	Calms	Mean force.	Days.	Inch.
1840, ...	4	1.395				11	0.0
1841, ...	2	2.150	178	134	0.36	14	0.0
1842, ...	5	2.170	216	96	0.78	9	0.0
1843, ...	6	4.295	420	204	0.69	12	14.2
1844, ...	7	3.005	450	197	0.70	11	24.9
1845, ...	5	Imper'ct	456	192	0.70	9	22.7
1846, ...	5	2.335	463	185	0.55	10	6.0
1847, ...	5	2.135	493	126	1.09	5	7.5
1848, ...	7	2.245	439	185	0.60	8	7.1
1849, ...	4	1.175	250	74	0.55	10	9.2

Rain in inches on surface—*On 14th, 0.235; †25th, not applicable; ‡26th, 0.820; *30th, 0.120.
The several daily means are derived from two eight hourly series, viz: 6 am, 2 and 10 pm, and 7 am, 3 and 11 pm.
Magnetic Disturbances in January—None.

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