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# CANADA

# MEDICAL JOURNAL.

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## ORIGINAL COMMUNICATIONS.

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*Granular Ophthalmia—Treatment of.* By D. MCGILLIVRAY, M. D.,  
Physician to the Ottawa General Protestant Hospital.

Granular conjunctivitis is a very troublesome and frequent disease, and often very obstinate to treatment. It is found more prevalent in some districts than in others. But why it so prevails in certain places is not well defined. I cannot say that it is peculiarly prevalent in this section of the country, as most of the cases that have come under my notice were immigrants from foreign countries or from other parts of Canada, excepting that portion of our floating population formed by the lumbermen, who frequently seek medical advice for conjunctival diseases. The disease consists in a roughened state of the mucus lining membrane of the lids, and more especially of the upper lids. The granular elevations are simply the conjunctival papillae in a hypertrophied condition from inflammation, and vary in degree and extent in different cases. In some they are very minute and scarcely appear to the naked eye, while in others they are as large as pinheads and cover the greater part of the conjunctiva or appear only in patches. They are most frequently found on the internal surface of the tarsi, while other parts of the membrane appear healthy or sometimes swollen and injected. On closely examining the everted lid, small greyish white bodies like sago grains appear on the palpebral conjunctiva. They appear in greater number at the retrotarsal fold; these are termed simple granulations, or according to Stelwagg, "granular trachoma," and are met with in the acute and chronic forms of the disease. This state of the eye is accompanied by a mucopurulent discharge which is very troublesome to the patient, vision is also frequently impaired from opacity and thickening of the cornea, the result of friction by the granulations; and if the disease is allowed to go on, especially in the acute form, loss of vision may follow

from ulceration of the cornea. To prevent this friction many plans have been adopted, such as attaching the upper lid to the eyebrow by means of stiches or adhesive plaster, and applying solutions of lunar caustic, sulphate of zinc, sulphate of copper, acetate of lead, &c., to the granulations, a method attended with pain and annoyance to the patient and sometimes with but little benefit.

While treating cases by the above methods, it occurred to me that friction in this disease might be prevented or at least greatly lessened on the same mechanical principle that it is overcome between other opposing surfaces, namely by oiling. Acting on this suggestion, I made a solution of cod-liver oil and alum sulph, half a grain of the latter to the ounce of the former, and applied the mixture by a camel hair pencil to the granulations, night and morning or oftener. After several weeks' trial the result exceeded my expectations, the opacity of the cornea and the granulations disappeared and the eye recovered its natural clearness. I have used this local treatment in several cases that have come under my care during the last two years, with equally good results. The *modus operandi* consists in the oil lubricating the granular surface and allowing it to slide smoothly over the cornea, while the alum astringes the granulations and makes them smaller, thus also helping to lessen the friction and consequently reducing the amount of mucopurulent discharge. I have no doubt that the cod-liver oil exercises a medicinal influence over the diseased conjunctiva owing to its chemical composition. In all the cases thus treated, tonics combined with alkalis and a generous diet, were prescribed. Total abstinence from alcoholic drinks should be strictly enjoined, as they invariably aggravate the disease; the eye should be kept well sheltered from wind and cold, and bathed several times a day with warm water, or the conjunctiva well washed of discharges by syringing with warm water three or four times a day, especially before applying the oil solution; I have found it beneficial to bandage a wet pad on the eye, as it absorbs discharges and prevents to some extent the movements of the lid. This mode of treatment was persevered in for several months in some cases, while other cases yielded to the remedy in a few weeks.

#### MEDICO-CHIRURGICAL SOCIETY OF MONTREAL.

MEETING HELD, APRIL 28TH, 1870.

The President G. W. CAMPBELL, A.M., M.D., in the chair. The minutes of the last meeting were read and confirmed. The President then introduced

Dr. HENRY HOWARD, Superintendent of the Provincial Lunatic

Asylum, at St. Johns, P.Q., who read the following paper on his system of "Ventilation."

MR. PRESIDENT AND GENTLEMEN,—The subject which I beg leave to bring before you this evening (my system of the ventilation of buildings) requires no proof of its actual necessity, or of how important it is in a sanitary point of view, you all know its importance just as well as I do. My system of ventilation I wish to be considered and discussed, by this Society, and that they would give their fair and candid opinion upon it without any prejudice. If you find it what I believe it to be, you will give me credit for it; if on the contrary you differ from me you will honestly give your reason why. Allow me for a moment to digress. I know there are many in this city, whom I fear are very ignorant of any system of ventilation, and when speaking on the subject begin to sneer at all systems. Men of such pretended knowledge, may (perhaps inadvertently) do a great deal of harm, without being able to offer any opinion, or suggestion, that would be of any benefit in a question of such vital importance. The majority of the medical and scientific men of the present day, have embarked in the subject of ventilation in order to secure some certain means of preserving the health of their fellow creatures. It is not the time through selfish motives, or egotism, to oppose instead of endeavouring to forward it. If such men are not capable by their own talents to add something to science, at least they should do so by encouraging all and every individual who is working hard for the sake of humanity. I believe, sir, it is a recognised fact that Dr. Parkes of the Royal Victoria Hospital, Netley, England, is one of the best authorities, in the present day, upon Hygiene, and I know that the high and responsible position that he holds, is due to his work on that subject. He certainly has grappled with the matter well, and has proved the errors of many old and new theories. So far as wind sewerage and foul gases are concerned, I have carefully studied his work and have adopted his theory: that foul air should be drawn off above, and not below the person, and that pure air should be admitted in a similar manner. That the great object in ventilation was to expel foul air as soon as possible after it was exhaled or generated, and admit pure air to take its place. That there should, in fact, be two distinct currents always going on in the place to be ventilated, and all this should be done without producing sensible draught.

Dr. Parkes in his work gives all the different means that had been invented to accomplish this end and concludes that they were all imperfect from one cause or another. To depend upon the wind was absurd, for we have no control over it, and when most wanted, very frequently there

was none; again it changes so often that tubes which a few minutes before were outlets, suddenly became inlets, and very frequently all the shafts would be inlets or outlets. Heat, steam, water, and horse power were all found too cumbersome, or expensive for general use; so all were given up in turn or left in their imperfect state, I suppose on the principle, better something than nothing, and that the very look of the thing made people believe that ventilation was going on. Observing these facts, I turned my attention to see if I could not invent some means to carry out Parkes' theory (independent of heat, steam, water, horse or man power). To ventilate my most miserable make shift of a building, crowded with the very worst class of lunatics, no man could have a better opportunity than I had of experimenting. My first set of experiments were to find out how impure gases were diffused through a chamber where there was no means of admitting pure air, nor any means of expelling foul. The result of these experiments was, that the foul gases were pretty equally diffused through the chamber, but that there was a greater quantity higher up, than lower down. From many such experiments I found that the room began to fill from above downwards. My next experiment was to see if I admitted air into the chamber upwards and inwards, (and this idea was my own) without any exit shaft, what would be the result. It was that I found the first effects of impure gas, about 9 ft. from the floor, but generally *all* my tumblers of lime water and leaded paper became affected in a more or less degree. My next experiments were the same as the last, with this addition, that I made an exit shaft through the roof of the Asylum, and put a moveable cowl upon the top of it. When this shaft acted as an exit, which was not always, I found that the pure air was below 7 ft. and near the floor; and that from 7 to 10 ft. was the greatest quantity of impure gases, though heated air was much higher, and while the air under 9 ft. was heated, yet there was but very little impurity in it. This experiment I tried very often, before I was satisfied with it, because as I have already said, sometimes my exit shaft acted as an inlet, at the very time I wanted it to act as an outlet.

From the foregoing experiments often repeated I came to the following conclusions.

1st. That the proper height at which to expel foul air was between 7 and 10 ft. from the floor.

2nd. That external air when admitted above the head inwards and upwards causes no sensible draught upon the person.

3rd. That this air when colder than the internal air fell down towards the floor and while passing through the heated and foul air, though it

became in some degree heated, was not impregnated, but very slightly effected with impure gases, it after descending kept floating up the impure gases; in fact there were two distinct currents, the foul gases and heated air *ascending*, the cold pure air *descending*. I know that the question has been asked "How can pure air pass through impure gas and not become impregnated with it?" The answer to this question is, that air is a mechanical mixture and not a chemical compound, and I do not say that if the foul gas is not drawn off, and that quickly, that the whole will not become *impregnated* and impure. But if, according to my system, the foul air is drawn off quickly, there is hardly time for the pure air to become even soiled. Here I wish it to be particularly understood that I do not say heated air is foul air, but that heated air and heated carbonic acid and sulphuretted hydrogen gases are lighter than pure cold air and consequently are floated up together, — the heated pure air, however, ascending much higher than the heated impure gases. In fact, the heated pure air will rush out through an open shaft if there is any draught, whereas the impure heated gas, as soon as it gets cold in the shaft, will fall down again unless there is a strong power to force it through.

The foregoing facts founded upon experiments, led me to put tubes through every part of the Asylum, all connecting with the main shaft that ran out through the roof of the house, and had a cowl upon it.

It was at this time that I invented an instrument for the admission of pure air into buildings and which for want of a better name I called a ventilator. One of these ventilators I placed in every window of the Asylum in the place of a pane of glass. You will ask me did these ventilators, exit tubes, and movable cowl purify the Asylum? I at once answer *No*. The state of the Asylum was very much improved indeed, but it was by no means perfect ventilation, simply because my exit shafts as often acted as inlets as they did outlets, and when there was no wind there was no draught in the shafts at all. I then invented an exhausting machine to take the place of the cowl, its movements however depended upon the wind; when it was working it did purify the Asylum perfectly, but when there was no wind it was of no use. It had one great advantage over the cowl, and that was, that I never had, with it, down draughts. I, at that time, frequently tried my experiments over again till I became perfectly certain that I had got the right mode of ventilation, if I could only succeed in having a motive power to work my exhauster, or foul air-expeller, independent of wind, steam, water or horse-power. I saw that the true principle was to have such a force as would suck up the foul gases independent of the pressure of the air

from below, in fact, that it should be pumped out, as water is from a well. I say I determined, if possible, to invent a foul air expeller, respecting which I would be certain that its action would be always equal, and independent of wind or weather. To accomplish this has caused me many a weary hour, and many a sleepless night, and more money than I am inclined to tell. Generally the fault was some miscalculation, or some mathematical error, and again the fault would be, want of a perfect machinist capable of carrying out my views. Often I felt inclined, and was advised to give it up; but I still persevered, and I now feel satisfied that success has crowned my efforts. The machine which hereafter I will describe takes a man six minutes to wind up, then it runs for twelve hours, in fact it runs 20 inches of cord an hour, so if you have sufficient fall for the weight, you can run it as many hours as you please.

It was visited by a number of scientific gentlemen from Montreal, some of whom are here present. They found that I had a main tube made of tin, air-tight, and 8 inches in diameter, running from one end of the Asylum to the other (130 ft.) which terminated in the transit shaft of the machine. Into this main tube there were 20 tin tubes from the different apartments of the Asylum to be ventilated; these were four-inch tubes, and I told the gentlemen present that I feared I was giving the machine more work than it could do. Some days afterwards, upon close examination, I found that all the tubes did not draw with the same force, and that the tubes furthest from the machine were those that were working best. After spending four days trying to find out the cause of this, I concluded that my four-inch tubes were too large and that the pressure of the air from below upwards was acting in some degree upon the shafts, when my object was that no air should be removed except by suction, like a pump sucking water out of a well. I removed the four-inch tubes and replaced them with one-inch tubes.

The effect was most satisfactory, the suction immediately became equal in the twenty tubes, and much more powerful, drawing a much greater quantity of foul air than with the four-inch tubes. I by this discovered that the machine is fully capable of exhausting 20 one-inch tubes, 10 two-inch tubes, 5 four-inch tubes; so, to ventilate any building now is a mere matter of calculation. When I had this work perfect, I made the following experiment. In the women's day room, where there were two exit shafts and a ventilator in each window (that is four windows,) there were in the room 36 lunatics. The room is 31 by 39 ft. and 12 ft. high. I placed graduated tumblers of lime-water from the floor to the ceiling, and did the same with leaded paper, I

also placed three thermometers on the same post, one on the floor, one at 7 ft. and one up close to the ceiling; the exit tubes are 9 ft. from the ground. After leaving these for four hours, the following was the result. There was hardly any change in the lime-water, and papers, except between 7 and 10 ft., even here it was not very visible; the thermometers ranged thus, the one close to the floor  $70^{\circ}$ , the one 7 ft. from the floor  $74^{\circ}$ , and the one 12 ft.  $80^{\circ}$ . This experiment fully proved the pure state of the chamber. *Secondly.*—That where foul gases was to be found was between 7 and 9 ft. *Thirdly.*—That though the heat was greater at the ceiling it did not bring up with it the impure gases, but that the pure air floated it up to the height from which it was drawn off, viz: 9 ft.\* *Fourthly.*—That through the cold outside air admitted from above inwards and upwards, was to be found nearest to the floor. You may say what benefit is to be derived from the charcoal and small piece of cotton wadding in the ventilators: Is not the outside air pure enough? I answer you that in the first place, the cotton wadding breaks the force of the wind and obstructs the entrance of organized and disorganized matter; secondly, that the outside air is not always pure, and the carbonic acid gas is absorbed in the charcoal. As a proof of this, I made the following experiment on Saturday the 22nd of this month. I placed in the sleeping room of the men and in that of the women also (no one being in the rooms), a glass of lime-water. I also placed one in the sitting-room of the women (42 present) one in the sitting-room of the men and one on my gallery in the open air. After three hours the following was the result: in the sleeping rooms there was not the slightest sign of carbonate of lime; the water in the open air and sitting-rooms were alike,—just sufficient to see that there was a change in the water.

The exhauster expels 4,500 feet of air every hour. It will therefore entirely change the air in a building 90 ft. long by 60 ft. wide by 20 ft. high once a day.

If the exit shafts open 9 ft. from the ground of the occupied rooms, it will pump up 54,400 feet of foul air every 12 hours. The area of the inlet, that is, where the air enters the fan box, is 144 square inches, the area of the outlet, or where the air is thrown off from the fan is one-eighth of this, or 18 square inches. The circumference of the fan box is 11 inches, and the velocity of the fan is twice a second or 120 revolutions a minute. I think, gentlemen, you will agree with me that my experiments are conclusive and that I have carried out my theory to a practical result.

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\* According to Haswell's Tables and formula.

Dr. GEORGE W. CAMPBELL said it had afforded him much pleasure to listen to Dr. Howard's paper. The merit of his discovery was the proposal to pump out all the foul air.

Dr. TRENHOLME enquired if Dr. Howard knew the exact amount of foul air which he expelled in a given time.

Dr. HOWARD said he had given it to one of the best mathematicians in the County to work out, and his result was 54,000 feet every twelve hours.

Dr. TRENHOLME asked if when Dr. Howard reduced the size of his tubes from one to four inches, the fan revolved more rapidly, and if the same amount of foul air was expelled.

Dr. HOWARD said it did, and the foul air expelled was the same, upon the same principle that a similar quantity of water was sent through a small hose as a large one.

Dr. REDDY enquired the length of the exit tube.

Dr. HOWARD said its width was one-third of the whole circumference; its length—as you choose. There was always more foul air at the exit of the tube than at any other part.

Dr. DAVID asked if Dr. Howard could assign any reason why the foul air did not rise higher than 9 feet.

Dr. HOWARD could not assign any reason—but that it was a fact he had frequently proved.

Dr. CRAIK said Dr. Howard explained that the gasses were heavier than the heated air, which being lightest goes to the top—the foul gases go up till they become the same weight as the air, and then stop.

Dr. TRENHOLME would like very much to see Dr. Howard's experiments carried out in a large and lofty room.

Dr. R. PALMER HOWARD, said that Sulphuretted Hydrogen, and Carbonic Acid were heavier than atmospheric air, and when heated were still heavier, and that the position of the foul gases, as discovered by Dr. Henry Howard, were just such as we would naturally expect from well-known physical laws. If the room should be 20 feet or more high—the height at which most foul air would be found, would be more than 9 feet, but this did not affect the discovery, which he considered an invaluable one. The simplicity of the clock work was admirable, steam being very expensive. The revolution of the fan must produce a vacuum, and it was a mere matter of calculation as to the size of the receiver. The economy of cost and its certainty of producing a vacuum were points of merit in the invention.

Dr. G. W. CAMPBELL suggested the idea that in cities it might be cheap to drive the fan by water-power, a very small pipe would, he thought, answer the purpose.

Mr. ROBERT MITCHELL, (steam fitter, &c.,) said water was far too costly to be used for such a purpose, in fact, it was the most expensive method that could be adopted.

Dr. CRAIK desired further light on the subject. He felt somewhat uneasy as to how the heating of houses in winter would be affected, when this method of ventilation was adopted.

Dr. HOWARD replied that the past winter had been an exceedingly cold one, and so far as he had observed in the Asylum at St. Johns, the temperature of the building was not effected in the most remote degree; the ventilator was open all the time, and there was not the first complaint of cold. The Asylum was heated with wood.

Dr. CRAIK asked if he (Dr. Howard) thought that the same result would have been arrived at if the place had been heated by steam or hot air.

Dr. HOWARD said he had no experience upon which to base a reply, but he thought under circumstances such as were mentioned by Dr. Craik there might be a little loss, but not sufficient to make it of the least importance.

Dr. CRAIK enquired if Dr. Howard had tested for organic impurities in the gases. Condyl's fluid in tumblers would give the means of testing for them. These organic impurities were drifted by currents, but when these ended they gravitated downward.

Dr. FENWICK said this question had already been settled. Many impurities adhere to the walls of the room, and also to the bed clothing.

Dr. LAROCQUE asked if he had tested for the amount of vapor in the atmosphere.

The Society then adjourned.

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MEETING HELD 3RD JUNE, 1871.

The Vice-President, DR. PELLETIER, in the chair?

Dr. FRANCIS W. CAMPBELL read the following paper on "Vaccination and Re-Vaccination":

MR. PRESIDENT AND GENTLEMEN,—The fact that during the past winter and spring, Small Pox has been epidemic in all the large sea-port towns of the Mother Country, as well as in many of the continental sea-ports, has, naturally enough, excited our fears that with the opening of navigation it would make its appearance among us. The St. Lawrence being the great highway to the West, it was reasonable to expect that among the many thousands who would pass directly through or sojourn for a few days, there would be some, who bringing the seeds of infection with them from the other side of the Atlantic, would simply

arrive here in time to see it give ample evidence of its character. That in such expectations we have not been disappointed, the records of the Montreal General and the Toronto General Hospitals, bear witness. Emigrants in the first stage of the disease have entered both these institutions, within the last few weeks, and one case at each institution has proved fatal. With the prospect of this loathsome disease becoming epidemic among us, the attention of the public naturally turned to Vaccination, as their safeguard and preventative. "Present arms" was a command often heard I believe in most surgeries in Montreal, during the past three or four months. Having, since the 1st of January of this year, in my capacity as Public Vaccinator and private practitioner, vaccinated upwards of a thousand persons, my attention has been directed more than usual to the subjects of Vaccination and Re-Vaccination, more particularly the latter. I have, therefore, thought that a few remarks upon these subjects might not be devoid of interest to the members of this Society. I would confine myself entirely to the latter branch of my subject, did I not feel that the whole subject of Vaccination is one, concerning which the junior members of the profession know comparatively but little. The early history of Vaccination is full of interest and will well repay a moment's attention. Its literature is scanty, and even the one or two works that within the last five years have made their appearance, give but a brief summary of the many important reports which have been issued on this subject, during the last fifty years.

Cow Pox, or Variolæ Vaccinæ, is a disease having a specific eruption, of a vesicular character, and limited in its appearance (as a rule) to the udder and teats. The most common time for its appearance are the early months of spring, and the early part of the autumn. It seldom attacks other than milch cows. There is a period of incubation which in the natural disease is presumed to be about three days. Ceeley, however, says he believes it may be prolonged a day or two. The earliest symptoms noted are heat, swelling, with considerable tenderness, of the udder, soon after which hard papules the size of a pea commence to develop themselves. These gradually increase in size, and in about four days from their first appearance they begin to take on a vesicular form, with a central depression. The vesicle gradually increases in size, till all have attained their full growth, which varies from that of a pin's head to that of an English sixpence, and is reached in from ten to twelve days from their first appearance. In number they vary from two to twenty. Their colour varies according to the colour and texture of the animal's skin, but they always have a glistening metallic appearance. In

cases where the vesicle has been allowed to dry without removing lymph from it, Seaton says it takes on a brownish-black appearance. In several cases of the disease, which occurred among Mr. Stephen's herd of imported Alderneys some two years ago (and which I was privileged upon two or three occasions to visit, in company with Professor McEachern of the Veterinary School of this city, under whose care they were), the dry crust, several of which I obtained were all more or less clear, and resembling much the appearance of very fine Gum Arabic. Although I examined the udder and teats as closely as the irritable temper of the animals would permit, I failed to find any fully answering the description given by Seaton. This brief description will, I hope, enable the members of this Society, not previously familiar with it, to recognize the disease, should they at any time be on the look out for it, or should opportunity put a case in their way. The plate (*Plate I.*) I now exhibit is from Ceeley's report, and illustrates the Casual Cow Pox in the teats and udder of a black and white milch cow. The disease is at its acme, the skin of the animal being fair, a slight areola is visible around some of the vesicles, many of which have the blueish, metallic appearance already alluded to. It exhibits papulæ, vesicles with central crusts, un-acuminated and acuminated vesicles, imperfectly developed and broken vesicles. The large reddish crusts are due to rupture of the vesicles, from rough handling in milking, and are discharging blood, lymph and pus.

(*Plate II.*)—Figure 1, exhibits this appearance in a more marked degree, with the teat greatly swollen. It is taken from a cow in the second week of the disease. Figure 2 is the teat of a cow with fairer skin, exhibiting perfect cicatrices, cicatrices with secondary crusts, raw and imperfect cicatrices, and a crust still adherent on the base of the teat.

#### THE AFFINITY EXISTING BETWEEN HUMAN SMALL POX AND COW SMALL POX.

At the outset of his enquiry, Jenner believed that the Cow Pox originated from the disease in the horse, generally known by the name of *Grease*. This idea he lived to correct. It is now well-known that the horse as well as the cow is liable to a vesicular disease of a variolous nature, and that lymph taken from a horse, suffering from the Horse Pox, and inserted into man, will produce an affection in all respects like that derived from the cow and equally protective. The illustrious discover of Vaccination at the very outset styled the Cow Pox *Variolæ Vaccinæ*, and that he had good reason for adopting this pregnant desig-

nation, subsequent investigation has fully proven. Many of his contemporaries derided him for it, but with admirable sagacity he adhered to his own views. Let us for a few moments recur to the idea involved in this designation. It implies that one *Genus* of the inferior animals is liable to a disease of a kindred nature with that which attacks man, the latter, for the most part, being pestilential and fatal, the former nearly always mild and scarcely ever pestilential. In Jenner's time, the practice of inoculation had long been in use to diminish the mortality of human Variolæ. Casual observation had taught him, that an affection derived from the cow protected from subsequent attacks of that disease. This led Dr. Jenner to conclude that the protecting power arose from the impregnation of the constitution with a mild species of Small Pox, instead of the malignant sort, which is usually propagated from man to man, either naturally or by inoculation. This idea guided him through all his investigations and finally led him to his great discovery, viz: the successful transmission by inoculation of the mild and safe affection from the inferior animals, instead of the contagious and malignant Small Pox which had so long devastated mankind. His confidence in this practice, arose from his firm conviction that these two disorders, however different they might be in some particulars, were in reality identical. I need hardly say that this theory found many bitter opponents, the disease arising from the cow being imagined to be an antidote, not a safe and effectual substitute for a more malignant form. Up to the time of Jenner's death, the various controversies that raged, consisted in assertions and counter-assertions, as to the alleged virtue of the disease derived from the cow. Subsequently, however, this portion of the subject was warmly investigated, and in 1840 the Provincial Medical and Surgical Association of Great Britain through a Committee appointed the previous year, brought forward at their meeting held at Liverpool, a masterly report upon Vaccination, in which this section of the subject was fully entered upon. The report says "The light thrown upon the whole subject by these enquiries respecting the diseases of the inferior animals is very striking and peculiar and completes that chain of evidence which leads us to our great conclusion: It has been shown by unquestionable evidence, that cattle and other animals have for centuries been known to be affected with Small Pox, or Variola. This latter appellation has unhesitatingly been given to the disease, by every different writer who has seen it, by Dr. Layard in England, by Fracastorius, Ramazzini and others in Italy. The disease described by the authors above-named was of a malignant character and destroyed the cattle, almost as extensively as Small Pox did the

“ human race. It raged in England in 1745 and in 1770 and even as late as 1780; at this time Dr. Jenner was carrying on his investigations, and it was in this very year that Dr. Layard published his paper on the transactions of the Royal Society, in which he mentions that inoculation from the cow was successfully practised to mitigate the severity of the disease. It is believed to have been the remains of this violent Epizotic that Jenner found in Gloucestershire, and which being occasionally transferred to milker, secured them from subsequent Small Pox. These various epizotics may be traced in the most authentic Medical Records for several hundred years, and the connexion between them, and pestilences which have ravaged both man and the inferior animals, throughout the ages of the most remote antiquity, may be discerned with much greater accuracy than at first might be imagined. It is likewise worthy of remark that the countries where the disease has of late years been found either on cows or horses, are those where it has formerly been known to have existed among them in its most virulent form. \* \* \* \* \* We have further to observe than an affection of a precisely similar character was witnessed in a dairy in Gloucestershire in 1825. The Veterinary Surgeon who saw it, described it as a malignant case of Cow Pox, the whole skin from the base of the horns to the end of the tail, and down to the hoofs being covered with the disease. It killed the animal, and extended through all the cows in the dairy, between forty and fifty in number. This exactly agrees with the variolous epizotic described by Dr. Layard in 1780 which he said ‘ bore all the characteristic symptoms, crisis, and event of Small Pox.’” Mr. Gibson writing in the *transactions* of the Medical Society of Bombay says “ Small Pox carries off many persons annually, particularly in remote districts. \* \* \* \* \* *The same disease is at times very fatal among cattle, they become weak, and feverish, and are unable to eat in consequence of the pustular eruption on the lips, tongue and throat.*” All these facts I believe go to prove that the Vaccine disease is not a preventative of Small Pox, but in reality is the Small Pox itself. In order, however, to still further prove this assertion, it is necessary to show that human Small Pox can be communicated to the cow; in like manner as the disease of the latter has been communicated to man. The first fact I will mention occurred on this continent, and is detailed by Dr. Waterhouse of Cambridge in a letter to Dr. Jenner. He says “ at one of our periodical inoculations, which occur in New England once in every eight or nine years, several people drove their cows to an Hospital in order that they might have the benefit of their milk. These cows were milked by persons in all stages of Small

Pox; the consequence was, the cows had an eruptive disorder on their teats and udders, so like the Small Pox pustule that every one declared the cows had the Small Pox." Many unsuccessful efforts were made by various individuals in England to put this question beyond dispute, by directly inoculating the cow with human Small Pox. At the Veterinary College in Berlin in 1801 it is said to have been successfully accomplished for the first time. The Saltzburgh Medical Journal for 1807 says that Gassner had successfully inoculated the cow with variolous matter. Dr. McMichael, in 1828, in a paper read before the Royal College of Physicians, says "Vaccine matter having failed in Egypt, medical gentlemen were lead to institute certain experiments, by which it has been discovered that by inoculating a cow with Small Pox from the human body, fine active vaccine virus is produced." Professor Sunderland of Bremer, it is said about this time, succeeded in producing the disease on cows, by covering them with sheets, upon which persons labouring from Small Pox had lain. Notwithstanding that the proofs were slowly getting stronger, as to the unity of the two diseases, the profession as a body continued skeptical, (as I believe even to-day many are skeptical) until the experiments made by Mr. Ceeley of Aylesbury were made public. This gentleman read his paper on Variolæ Varionæ before the Provincial Medical Association of Great Britain in 1840, and up to the present day it is the authority on the subject. After many failures he succeeded in infecting two cows with the matter from human Small Pox and obtaining from them beautiful vesicles, from which source many hundred children were vaccinated, who exhibited all the phenomena of Vaccination in the most perfect form and complete degree. The Vaccination Section of the Provincial Association were furnished with some of this lymph by Mr. Ceeley, and they reported that it "produced the most regular vesicles, which in every respect corresponded with those so beautifully delineated by Dr. Jenner in his first publication."

(Plate IX.)—Represents the first successful variolation of the cow or inoculation from lymph of human Small Pox. The following is a brief outline of this experiment. Mr. Ceeley made seven punctures near the left *Labium Pudendi*, and inserted variolous matter. On the 9th day after he vaccinated the same cow on the right *Labium Pudendi*, with lymph of the fifth, sixth and seventh day from a child. On the tenth day after the insertion of the variolous matter one of the punctures on the left *Labium*, had assumed the form of a Vaccine vesicle. (Plate 9 shows the experiment on the tenth day, on left side is variolous

vesicle, on right side is seen Vaccine punctures of the previous day, when variolous punctures were in a doubtful condition.) From this variolous vesicle, he abstracted on the tenth day a considerable quantity of lymph.

(Plate X.)—Shows the experiment on the eleventh day; the variolous vesicle is duller from the abstraction of lymph, and the Vaccine punctures show signs (third day) of taking.

(Plate XI.)—Illustrate the experiment on the twelfth day.

(Plate XII.)—Shows the experiment on the thirteenth day. It will be noticed that both the variolous vesicle and the Vaccine vesicles continue to advance. This is the fifth day of the Vaccine vesicles, and from them lymph was taken and used successfully on both children and adults.

(Plate XIII.)—Is the fourteenth day and represents the variolous crust at its maximum of development with a large central crust. The Vaccine vesicles to-day (seventh) were at their greatest development.

(Plate XIV.)—Both description of vesicles are declining. This was the seventeenth day of the experiment. From this date both the variolous and Vaccine vesicles ran a nearly parallel course, so that on the twenty-sixth day of the former, and the 17th of the latter, the scabs of both appeared perfectly similar. To obviate any objections which might occur from the introduction of the Vaccine lymph as mentioned in the previous experiment, Mr. Ceeley inoculated another cow, on the *Lubium Pudendi*, with Small Pox matter of the seventh and eighth day.

(Plate XV.)—Represents the experiment on the fifth day.

(Plate XVI.)—Represents the experiment on the sixth day.

Lymph was to-day taken from the outer and lower vesicle on the right.

(Plate XVII.)—Eight day of the experiment, lymph was again taken from same vesicle as on sixth day.

(Plate XVIII.)—Ninth day; lower vesicles raw from injury in obtaining lymph.

(Plate XIX.)—Tenth day.

(Plate XX.)—Twelfth day.

(Plate XXI.)—Represents the experiment on the twenty-third day of the variolisation. This cow was subsequently inoculated both with variolous matter, and Vaccine matter, but no result followed, pretty conclusive evidence it seems to me of the unity of the two diseases. In neither of the experiments just briefly detailed, did Mr. Ceeley note any indisposition worthy of mention. The lymph that he obtained from both these animals was used upon a large number of persons. In some

it failed entirely, in many it produced papules destitute of lymph, and in very few were perfect vesicles formed. It was not until it had undergone four or five removes, that it produced good vesicles. Indeed it is impossible to read Ceeley's interesting paper detailing his various experiments, and his account of the phenomena exhibited by human subject vaccinated by him with the lymph generated by the variolation of the cow, without recognizing that we are reading over again the whole story of the inoculation of the human subject with primary cow poek. There was the same difficulty in making the vaccination take, the same occasional occurrence of imperfect results, the same retardation in successful cases of the papular and subsequent stages, and the same activity of local symptoms as are noticed when primary cow poek lymph is used.

(*Plate XXII and Plate XXIII.*)—Illustrate the accidental inoculation of an assistant of Mr. Ceeley, with variolous matter, who had been vaccinated and subsequently had modified Small Pox.

(*Plate XXIX.*)—Represents Vaccination of a child with lymph taken from a child who had been vaccinated with matter taken from a cow itself vaccinated with humanised lymph, (three upper vesicles) also two lower vesicles with variolous matter passed through the cow.

Before leaving the subject of the identity of the two diseases I will briefly mention a most interesting incident which occurred while Mr. Ceeley was engaged in his experiments. Eight milch cows had the cow poek induced in them by variolous effluvia. They, with two sturks, were turned to graze in a meadow, in which the clothes and bedding of a person who had died of malignant Small Pox, had previously been exposed night and day for a week and in which they were at one time left a portion of the day, when the cows were there. One of the cows was noticed at one time in the act of licking the bedding. Within 12 days after their admission to the meadow five of the milch cows and one sturk, simultaneously, or almost so, exhibited well-marked cow poek, and from these the disease communicated itself to the hands of several of the milkers. The simultaneousness of the attack, shows a common cause, and the period of latency points to variolous effluvia.

*Retro-Vaccination*, or inoculation of cows with vaccine lymph that has been passed through the human system an unlimited number of times, deserves a passing notice. At the present day many who have not examined the subject, are calling loudly for the adoption of this plan as a means of renewing what they are pleased to term, *the worn out vaccine*. The truth of the matter is, the successful performance of the operation is one of exceeding difficulty, and so far as I can gather no

benefit whatever attends the transmission. When vaccinated back to the human subject it produces results, though with less infectiveness, during the first two or three removes; in other words till it is again humanised, than the results of ordinary vaccination.

(Plates III, IV and V.)—Exhibit the appearances produced on the udder of the cow by humanised lymph.

As a summary of the preceeding pages I claim to have proved the following:—

1st. That cattle in many ages, and different countries, have been afflicted with Small Pox.

2nd. That this disease has existed among the inferior animals simultaneously with Small Pox in man.

3rd. That it appeared among the cattle in England at various times during the 17th century, and that even comparatively recently it has shown itself with considerable severity.

4th. That when the disease appears among the inferior animals in a malignant form, it produces by inoculation a disease of similar severity in man.

5th. That as man has received this affection from the cow, so likewise has it been produced *in* the cow from man.

6th. That the direct inoculation of the cow with human Small Pox, has produced a mild and mitigated disease, and that such disease reproduced by inoculation on man accords entirely in its character, in its progress, and in its protecting influence with the Variolæ Vaccinæ, as described by Dr. Jenner, thus irresistably proving his fundamental proposition, that Cow Pox and Small Pox are not dissimilar, but identical, and that the Vaccine disease is not the preventative, so to speak, of Small Pox, but the Small Pox itself.

The operation of Vaccination is one, apparently of so trivial a nature that it may seem hardly worth while to occupy even a few moments of your time in alluding to it, yet when we consider that so much depends upon its being properly done, it may justly assume importance in our eyes. Having, since 1862, occupied the office of Public Vaccinator for a considerable section of the city, and during that time vaccinated several thousand persons, I have had ample opportunity of trying the various methods for Vaccinating, as well as discriminating between the relative value of arm to arm Vaccination, the use of liquid lymph in capillary tubes, and the dry scab, or crust. First then allow me to say that the method I now invariably adopt is to make use of the *perfect* scab. In this matter there is room for the exercise of much caution, and it would be ill advised to use crusts from even healthy children

without having subjected them to the following easy examination. The scab should be held up to the light, and if it presents a mahogany colored semi-transparent centre you may put it aside for use, first having taken care to cut away from around it, the few drops of pus with epithelial scales that have dried with it. Enquiry should also be made to ascertain if the vesicle had ruptured in its course; if it has I would advise its rejection. In my early experience as a Vaccinator I found many scabs, which had in their course been accompanied with considerable constitutional disturbance, all but destitute of vaccine matter, being nearly perfectly clear when examined by the light test. Enquiry as to the cause, at once showed me that in, I think every case, there had been an early rupture of the vesicle with a copious discharge of clear liquid lymph. Having selected my crust I shave a portion of it down upon the back of a plate or saucer, and add a drop or two of *cold* water, and work it into a rosy solution. Taking a quantity of this solution I smear the portion of the arm I intend to vaccinate, freely with it, and then make what I term the tartan cut, viz.: a number of parallel scratches, with cross scratches. Care must be taken not to draw blood freely, for I find when this is done, the chances are decidedly against the success of the operation. If the child is pale I use a moderately blunt lancet. If the child is florid, with a fresh looking arm, I make the scratches with a fine needle. With this method, which is in many respects similar to the plan generally adopted by the profession in Montreal I have had very great success, and have succeeded in getting vesicles, filled with lymph, and ending in large dark brown mahogany crusts. The method of arm to arm Vaccination I have tried several times, but the vesicles and crusts which have followed have not shown any superiority over the first named method, while the difficulty attending it is such as to prevent its being adopted in this country—even anything like universally—for many years to come. The last method I will notice (although there are others) is that by means of liquid lymph preserved in capillary tubes. This is highly recommended by many in England, and during one whole year and a part of a second, I adopted it in my practice, but for constant use have now abandoned it, being satisfied that the results obtained are not one bit better than what I always obtain from a first-class vaccine crust. The collection of the lymph in capillary tubes is a matter of considerable trouble. According to Seaton, the vesicle should be punctured before the areola makes its appearance. As experience has taught me, no particular day, as a rule, can be relied upon for the areola to appear, it requires somewhat close watching to get the vesicle in the ripe condition which is

desirable. I need hardly say that in this country, where fees for Vaccination are so small, this is a serious drawback to this method. Why it is that the lymph is not to be used after the areola has made its appearance, has always seemed to me an anomaly, when the dry crust is so successful, and I have not been able, in all my reading, to find an explanation for it; but that the direction is a wise one, I have had proved in my practice. Some two years ago I Vaccinated a beautiful child in three places, and on the seventh day called, armed with a supply of tubes. To my annoyance I was told that only a couple of hours previous to my arrival, two of the vesicles had burst. On examining the arm, I found a light areola of about three quarters of an inch in depth surrounding the vesicles, while liquid lymph was apparently streaming down the arm in considerable quantity from the two broken vesicles. I at once set to work, and charged fully thirty tubes, which I freely distributed to my friends. Of those that I kept not one succeeded, and I believe the same remark applies to those I gave away. The only use I now make of this means of collecting lymph is the following:—I find it a matter of some difficulty to preserve throughout our hot summers (when Vaccination is not being carried on) the vaccine crusts. I, therefore, always about the middle of May, charge a few tubes—and with them in the fall I start afresh my Vaccinations. The great objection, however, to this method of Vaccination is, in my opinion, the enquiry which forces itself upon our attention—whether the rupture of the vesicle in so early a stage as the seventh or eight day—or even during any period of its course—does not affect the constitution as to its liability to a subsequent attack of Small Pox. In cases where this has occurred, and Vaccination again performed within a short period, the vesicles have gone through all their stages as if nothing had been done before. In such cases there can not be any doubt but that the first Vaccination was worthless. In other cases, a second Vaccination has been followed by an irregular and incomplete effect. The explanation of this is not difficult, I think, of solution, and depends doubtless upon the period when the rupture takes place, and the amount of lymph which has been discharged from it. It would be well, therefore, that those whose views may lead them to adopt the collection of liquid lymph in capillary tubes, should leave, at least, one and if possible two vesicles intact. Before leaving the subject of Vaccination I wish to allude to Bryce's test, many beautiful instances of which have occurred in my Vaccination practice. Mr. Bryce, of Edinburgh, very early in the present century, instituted many experiments to ascertain if the constitutional effect of the disease had taken place,

for he then stated, what is very generally believed now, that you may have a fair local affection, without the system being involved. Mr. Bryce argued that if the vaccine had not produced a constitutional effect, fresh lymph introduced upon the fourth or fifth day, would produce effects which would go through the ordinary periods of action, while, if the system was pre-occupied by the vaccine matter, the secondary vaccination would rapidly gain upon the primary till they were both equal, and finish their various stages together. I quote one case occurring quite recently in my practice. "Albert H——, Cadieux Street, was vaccinated by me in four places on the morning of Saturday, May 6th, 1871; on the afternoon of the 10th, I called and was informed the vaccination had not taken. On examining the arm it was difficult for me to decide whether to vaccinate again or to wait a day or two, but, as the mother was anxious for me to repeat the operation without delay, I vaccinated the child in four more places upon the same arm. I called on the 12th and found distinct evidence of the original marks having taken, and made considerable progress, while a faint flush in the neighbourhood of the last four cuts showed me that the whole eight were going to succeed. Fearful that so many vesicles would entail a severe constitutional disturbance I visited the child daily, and watched their progress. On the twelfth day from the original vaccination, the whole eight vesicles were quite equal in size, the areola around the first four being slightly greater in extent than around the four last. On the twenty-sixth day the whole eight crusts, nearly of a size, fell off. The constitutional disturbance was not more than would have followed an ordinary double vaccination.

Bryce recommended that his test should be systematically employed. This is too much to expect, and altogether unnecessary; but there may, now and again, occur cases where it may be well to employ it, and when it occurs, as it has done to me,—accidentally,—it is interesting to watch it.

As regards the vaccine lymph losing its protective power by successive transmissions, the matter may be answered by the following quotation from Seaton's work issued some three years ago. He says:—"The lymph now in use throughout the Stations of the National Vaccine establishments is, if not exclusively, nearly all of Jenner's original stock, and from daily observation I can affirm that it has not lost anything of its infective power, and that the vesicles produced by it correspond accurately in their character and course with Jenner's description."

## RE-VACCINATION.

I now come to the last, but not the least, important branch of my subject, re-vaccination. The time I have occupied, however, being already so great, I feel that I will have to condense much that I would desire to say upon it. It is only within the last fifteen years that the profession have at all regarded it with favour; but I think I may now safely say that there are few medical men who do not advise its adoption, although there is considerable difference of opinion as to the best time, or rather the age, when it is *necessary* to have it done. So far as I can judge, I think the majority decide in favour of puberty, unless there might be urgent reasons, such as the presence of, or a threatened epidemic of Small Pox, where all over seven or eight years had better be done. During the last few months I have re-vaccinated a very large number, and have met with a very large amount of success. Fully sixty per cent. of my re-vaccinations took immediately—a large proportion of those upon whom I repeated the re-vaccination a second time took admirably, and even among those who failed upon the first and second re-vaccination, some took when applied a third time. Altogether, I do not at all over-estimate the results, when I say that fully eighty per cent. of my re-vaccinations were successful, and their ages varied from seven years up to thirty years. The amount of success varied, however, considerably, and at twelve years, as a rule, I got the best results, in the appearance of large, healthy crusts; after twelve years I was not conscious of any increase in the size of the vesicle, or in the amount of constitutional disturbance. There is considerable difference in my experience in the appearances of the primary and secondary crusts. The former are usually thick and round, with even edges, while the latter are very often as large as an English shilling—very thin—irregular in shape, and with a very irregular edge. I have made enquiry as to the amount of constitutional disturbance that accompanies the successful re-vaccination, and believe that, as a rule, it is greater than in a child with primary vaccination. With the exception of having used the crusts obtained from one very successful case of re-vaccination—in about thirty cases—all my re-vaccinations were made from the crusts of primary infantile vaccination. I always had a prejudice against the employment of secondary lymph, and was induced to make use of it from the following circumstances:—About the middle of March I re-vaccinated a strong young man from the Eastern Townships, who was in attendance at the Military School. The marks of the original vaccination were plainly visible, though

small, and somewhat smooth in appearance. I inserted lymph in three places, which produced three of the largest vesicles I ever saw, either on child or adult, with an excessive amount of constitutional disturbance, confining him to bed two days, and to the house two days more—the local phenomena being equally well marked. It so happened that he resided in the house of a family I attended, and nothing would do them but that the whole family should be vaccinated from this young man. I thought the opportunity of testing secondary lymph an admirable one, being at their own request, and accordingly re-vaccinated three children, from five to twelve, the mother, aged thirty, and a maiden lady residing in the house, who also expressed a similar desire. I had not made the insertions more than two or three minutes, when my attention was directed to a raised white lump, like that of hives, about half an inch in diameter, which surrounded the cuts, and which, in turn, was surrounded by a well marked red areola, at least an inch in diameter. I confess that this appearance somewhat alarmed me, and I, therefore, all but daily watched the progress of these cases. Nothing unusual occurred—they ran a normal course, producing good secondary crusts, with considerable constitutional effect. In brief, I may say I used the whole of the three crusts obtained from this young man, and in every case where it was employed, there was the same local phenomena immediately visible. The success I obtained from its use was quite equal to that of any primary matter. At this time I was not aware that secondary lymph had been used in England; but in the *Lancet* of April 29, of this year, the following forms part of the proceedings of the Obstetrical Society of London at its meeting on the 5th of April:—

“ Dr. Branton showed a wax model of a successful vaccination performed with the lymph from a secondary vaccination.”

“ Mr. Eartees considered that the most obvious criticism to be that though occasionally lymph from a secondary vaccination might produce a good vesicle, still it could not in general be depended upon.”

“ Mr. Wilkinson said he had several times vaccinated successfully with secondary lymph.”

“ Mr. Scott, had from necessity vaccinated a gentleman with secondary lymph producing perfect vesicles. As he was much exposed to contagion, Mr. Scott re-vaccinated him in three weeks with primary lymph and again vesication took place, but less perfectly.”

I need hardly say that my consenting to employ, and afterwards for a time continuing to use secondary lymph, was simply for experiment, and that primary lymph should invariably be employed.

The *Lancet* of January 21st, 1871, says:—There can be but one opinion as to the wisdom, and even the necessity of it. (Rev.) No sensible man will rely upon his primary vaccination, unless he lives in an unusually protected position. Only fanatics of the anti-vaccination order now talk as if the first vaccination were vaunted as a permanent protection against Small Pox. The profession should set forth the extreme importance of re-vaccination. The effect of the first vaccination clearly tends to wear out. From the early part of the century cases of Small Pox, after vaccination, have been increasing, and now amount to four-fifths of the cases. . . . But here is the point—no cases are reported after re-vaccination. Re-vaccination, performed carefully, with fresh lymph, and plenty of it, will protect from the disease *absolutely*. At least such is the experience at the London Small Pox Hospital, where for the last 30 years the nurses have escaped the disease, and they were re-vaccinated before entering upon their duties, and I believe that the same remark applies to the other Small Pox Hospitals in England.

I have thus condensed into a few pages my own experience in the operation of re-vaccination, as well as a brief summary of the opinion of the profession upon the subject. Its importance and efficacy is denied by few, and I am certain would not be doubted by any, were they to investigate the subject, as it deserves. Indeed it will be news I am sure to some even in this Society to know, that the vaccination or re-vaccination of persons who have had Small Pox, has not only been recommended within the past few months, but actually carried into practice. Several cases have lately been recorded, where very fine vesicles have been produced in persons who have had Small Pox. The present epidemic in England has shown that secondary attacks of Small Pox have proved fatal, and experience shows that vaccination and re-vaccination together confer even greater safety than does an attack of Small Pox. This fact is, it appears to me another strong argument as to the unity of the two diseases, but perhaps the strongest of all was detailed by Mr. Henry Lee at the meeting of the Royal Medical and Chirurgical Society of London on the 9th of the last month. He mentioned that variolous matter had been taken from a Small Pox patient and cow pock alone produced.

#### SYPHILIS AND VACCINATION.

The transmissibility of syphilis and other diseases by means of the vaccine virus has been the great argument of the anti-vaccinators, but till the present time, no instance of this disaster have been proved, as

occurring in Great Britain. Within the last month, however, Mr. Jonathan Hutchison has brought forward at the Royal Medical and Chirurgical Society some eight or nine well authenticated cases, where syphilis had been so transmitted, and at the present moment a Committee of that Society are engaged in investigating the facts detailed by Mr. Hutchison. Seaton in his last work denies that syphilis is so conveyed; but even although a few isolated cases of apparent transmission should be brought forward, they would not weigh the weight of a feather in the scale compared to the lives that have been saved through the instrumentality of Jenner and his wonderful discovery of vaccination.

Gentlemen, I am done, I hope I have not been too tedious, but the vastness of the subject, with its very great importance, is my excuse for the length of my paper.

Dr. FENWICK mentioned that in the Proceedings of the Surgical Society of Ireland, cases of Small Pox were reported which proved fatal even after successful re-vaccination. The rule that Small Pox never followed a successful re-vaccination, as mentioned by Dr. Campbell was, therefore, not absolute, although in large Small Pox Hospitals it has apparently conferred almost complete immunity from the disease, none of the physicians, nurses, or other attendants having contracted it.

Dr. SCOTT enquired why vaccination should be performed at four different points. He had had thirty years' experience and had never used but one place to vaccinate and had been perfectly satisfied with the results. He believed that scratching so many places gave rise to much more constitutional and local disturbance than was at all necessary or advisable. He had used the secondary scab several times but it had never taken.

Dr. GODFREY knew a few cases in Montreal where persons had had Small Pox twice and in one case even three times; if this be the case we may readily conceive that there must also be exceptions to the absolute rule of protection by re-vaccination. He had this year met with some obstinate cases of skin disease from vaccinations practised at the hands of others. Had never met with this before this year—believed he has seen even pyæmia result and he thought it was this, especially, which had led to the fierce opposition to the Vaccination Acts which had been manifested in Great Britain.

Dr. FRASER said that with reference to the identity of the two diseases, his mind was not made up; he was, therefore, not prepared to express an opinion upon that point. With reference to the best method or plan of vaccinating, he had tried nearly all that had been recommended and finally had returned to the use of the dried scab, as recommended by Dr. Campbell, which he considered was the one which would

give most satisfaction. Vaccination from arm to arm was very good when it could be done, but it was frequently inconvenient for the parties to arrange to bring the children together; it should be done on the 8th day. With regard to *selection of a scab* for use, the Doctor recommended the rejection of a scab from any child which showed the slightest sign of skin eruption, and stated his belief that disease might be communicated by means of vaccination. Alluding to the recent cases of supposed syphilis acquired from vaccination, the Doctor said that Mr. Hutchison had so long paid particular attention to the subject of infantile hereditary syphilis that his evidence on this point must be considered of great value and not lightly to be put aside. Dr. Fraser further recommended the rejection of a scab from a child which presented any of the appearances characteristic of the scrofulous diathesis, and instanced a case in which he found that in using the crusts from any of the members of a certain family there was produced a large irregular pustule and a number of small pustules, enlarged glands, and perhaps some other form of skin eruption. Here, therefore, there was evidently something wrong with the matter itself, and from the same effects having been produced from *all* the members of this family we may fairly assume that the fault lay in the existence of some constitutional peculiarity, probably scrofulous. Of course, no medical man would dream of using a scab from a child presenting evidences of syphilitic disease. Some have asserted that this affection could not be communicated except a small quantity of blood was transferred to the arm together with the vaccine matter, but whether this be so or not, it is practically too fine a distinction to bear tampering with. For many years past he has had no difficulty in preserving a supply of scabs in a small, wide-mouthed bottle well corked. Last year his supply failed, and he got three scabs from the Medical Department of the Privy Council, and found them to answer admirably. The Doctor further remarked upon the usefulness of re-vaccination, showing that as we can never tell what persons really are protected by the previous vaccination, it may be our duty to re-perform the operation upon all.

Dr. REDDY remarked upon the occasional occurrence of a peculiar eruption during the course of the vaccine fever, and showed how annoying such cases might sometimes be. The eruption first appeared about the seventh or eighth day, was at first papular then vesicular; it disappeared about the fourteenth day.

Dr. TRENHOLME had a case sometime ago of a child in which there appeared high fever and a papular eruption, to the great alarm of the parents. Believed this was from decomposition of the matter acting

there as a poison, and thought it arose from the fact that the matter, after wetting, had lain for nearly a week between the glasses; he now always took the precaution of carefully washing the glasses just before using them. Dr. Trenholme then exhibited a fine vaccine crust which was the result of the vaccination of a young child directly from a heifer which was suffering from cow-pock.

Dr. GODFREY mentioned that he had preserved scabs in *Glycerine* and found them quite perfect after the lapse of two years.

Dr. FRASER questioned Dr. Trenholme's idea that the eruption described by him was the result of septicemia; such an appearance was not characteristic of pyæmia and none of the other symptoms of this grave affection were present.

Dr. BELL said that in the United States army they preserved vaccine crusts by imbedding them in fine white wax. In this manner he had seen crusts kept a very long time.

Dr. F. W. CAMPBELL, in replying to the debate, said he felt much gratified at the remarks which had fallen from the various speakers, but he regretted that the subject of the identity of Cow Pox and Human Small Pox, which he believed he had proven, had not called forth any discussion. With regard to Dr. Fenwick's calling in question the possibility of Small Pox succeeding a successful re-vaccination, he would say that while it was possible, it was not probable, and the words he had made use of were copied from a recent number of the *London Lancet*. Dr. Scott had doubted the advantage of vaccinating in four different places, but he (Dr. Campbell) thought that it was the duty of every medical man to vaccinate in at least three if not four places, when the weight of evidence clearly proved that those who had three or four good marks were vastly more protected than those who had only one or two. In Great Britain, public vaccinators were compelled to make four distinct insertions of vaccine matter. Dr. Reddy had alluded to the appearance in a few cases of a papular eruption. In his (Dr. Campbell's) experience this eruption was exceedingly common, and he believed it to be simply the Cow Pox, matter having thoroughly entered the system was thus eliminated by peculiarity of constitution. He believed it was quite possible to communicate disease by means of vaccination, but thought that a little caution would prevent its occurrence. He was certain that the benefits which the world had received from vaccination could not for a single moment be counterbalanced by even the accumulation of all the asserted cases of transmitted disease.

The Society then adjourned.

## PERISCOPIC DEPARTMENT.

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 Surgery.

## ON THE REMOVAL OF NÆVOID GROWTHS.

Mr. James F. West, F.R.C.S., senior surgeon to the Queen's Hospital, and Professor of Anatomy in Queen's College, Birmingham, writes to the *Lancet* on this subject :

Each case must be treated on its own merits. Thus there are certain cases in which the setting up of adhesive inflammation, and the consequent obliteration of the vessels supplying them by the injection of the perchloride of iron, by vaccination, or the introduction of heated wires into them may be advantageously employed. But this principle cannot be carried out in many nævi of the face, as a large, dense cicatrix is thereby produced, which is often very unsightly. The simple application of collodion, or of pressure by elastic pads, may cure in slight cases.

The destruction of nævi by caustics, again, is attended by uncertain results, and the consequent cicatrices are often deep and ugly, from the impossibility of our gauging the distance to which the caustics— as chloride of zinc, nitric acid, etc.—ought to penetrate the tissues.

The ablation of erectile tumors is probably the most perfectly reliable means of treatment, and this may be accomplished either by enucleation, the ligature, the knife or the *écraseur*.

Piecemeal excision or enucleation is often attended with great loss of blood, even where the adjacent arterial trunks have been compressed as completely as possible; and the little patients who are the ordinary subjects of nævi, bear hemorrhage badly. A comparatively trifling loss often proves so serious to the patient that the attacking of large subcutaneous nævi by this process would hardly be justifiable.

The same difficulty meets us in the use of the knife; and I cannot doubt that the older surgeons were just in laying it down as a rule that, in removing nævi, it was always proper to cut wide of the tumor, and on no account to cut into its mass.

The introduction of either hare-lip pins or of ligatures frequently fails to cure; the latter are especially unreliable with venous nævi of large size, owing to their becoming loose, even though the skin around the growths may not have been included in them. The parts daily diminish in size, so that ligatures have to be again and again applied to ensure the entire destruction of the tumor. Moreover, ligatures

often set up troublesome ulceration at the base of the nævi, from which occasionally severe hemorrhage takes place.

The advantages which, in my experience, the *écraseur* offers are, that hemorrhage is avoided—an important element in all operations, but particularly so with children, and that you have a linear cicatrix and a comparatively small wound; and thereby prevent or diminish the deformity which, by other operative procedures, will almost of necessity be produced. Chassaignac, also, claims for it that less inflammatory action and less suppuration attend its use than that of the knife; and, consequently, that the wounds resulting therefrom heal more readily, and are less likely to be followed by pyæmia. On these latter points I will not now offer an opinion; but as to the smallness of the resulting cicatrix—a great desideratum in all operations about the face—and as to the freedom from hemorrhage, even when dealing with large growths of this kind, I am quite decided.

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#### TREATMENT OF CHILBLAINS.

Mr. Fergus recommends sulphurous acid in this affection. It should be applied with a camel's hair brush, or by means of a spray producer. One application of this usually effects a cure. The acid should be used pure. A good wash for hands or feet affected with chilblains is sulphurous acid, 3 parts; glycerine, 1 part; and water, 1 part. The acid will be found particularly useful in the irritating, tormenting stage of chilblains.—*Clinn. Med. Repertory.*

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#### CONGENITAL OCCLUSION OF THE RIMA GLOTTIDIS.

Dr. Louis Elsberg, of New York (*Trans. Am. Med. Assoc.*), presented to the Am. Med. Association a wax model of an interesting case of congenital occlusion of the rima glottidis, occurring in a young lady, 17 years of age. There is, as far as he has been able to discover, but a single other case of this character on record, and this was not published until after the present case had come under his observation. The latter case, a boy 11 years of age, was seen by Dr. Zurhelle, of Aix-la-Chapelle, in 1869, and published in the *Berliner Klinische Wochenschrift.*

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#### IS IMPERFORATE HYMEN EVER HEREDITARY ?

In answering this query affirmatively, Dr. Horatio Yates, Surgeon to the Kingston General Hospital, Canada (*Braithwaite's Retrospect*), mentions a family where two sisters and two children by a brother's wife had imperforate hymens. He regards these cases as too much to be called a coincidence, but clearly an example of an hereditary peculiarity.

## ON CATARACT.

Haynes Walton, Esq., Surgeon to St. Mary's Hospital (*Braithwaite's Retrospect*), does not consider advanced age in itself an objection to operating if nothing else forbid. The best attainable results have followed extraction after ninety, in the hands of Sir W. Lawrence. The late Mr. Scott did the same operation on a female between ninety and one hundred. Dr. Walton has several times performed it to his complete satisfaction, after the eightieth year, and once as late as the eighty-sixth. Two cases of success are published after the patients have turned one hundred years each. Of course, after the seventieth year a person is less favourable for the ordeal of extraction, from a variety of circumstances and from contingencies inseparable from long life.

## MEDICATED BOUGIES IN GONORRHŒA.

Dr. Schuster states that tannin mixed with glycerine at first forms a soft waxy substance, which soon becomes hard and brown, and melts in a moist atmosphere at the temperature of the body. Dr. Schuster has formed small pencils of this compound, which he inserts into the urethra of patients suffering from gonorrhœa. He has found the treatment by means of caustic injections (the abortive method) frequently fail, and that it occasionally produces violent pain, inflammation and hemorrhage. On the other hand, the treatment with slightly astringent solutions cures the gonorrhœa within a period varying from four to seven weeks, but is often followed by a troublesome gleet. The tannin-glycerine rods employed by Dr. Schuster are from three to four inches in length, well rounded at the extremities, and consist of thirty grains of tannin, one grain of powdered opium, and a sufficient quantity of glycerine to form a pastille. These rods are hard in winter and soft in summer. Before their introduction they should be dipped in warm water. They are to be left for from five to ten minutes in the urethra, and then withdrawn. As a rule they produce no pain. If, however, they be left in for an hour, or over night, more or less pain is caused, and this appears to be due to a combination occurring between the tannin and the mucus or pus, which becomes hard and acts like a foreign body. Dr. Schuster has had no case of orchitis following the use of these pencils, though he has thought it advisable to recommend the employment of a suspensory bandage, nor has he noticed any irritation of the bladder or prostate. In case of gleet, a rod may be left in for a few minutes, and a speedy cure usually results.—*Movimento Medico-Chirurgico di Napoli*—*National Med. Jour.*

## THE EARLY SYMPTOMS OF SPINAL DISEASE.

In an article in the *New York Medical Journal*, October, 1870, by Dr. James R. Wood, of New York, on Pott's Disease of the Spine, he calls attention to the following premonitory symptoms:—

The early symptoms of curvatures of the spine are sometimes very obscure, and it is often difficult, more especially for those not familiar with its characteristic features, to form a correct and satisfactory diagnosis. A physician may, and many even those of high standing in the profession do, pass through a series of years of professional service without ever having a single case of the kind come under their observation; and as there are other diseases with symptoms somewhat analogous to this in its early stages, it is often confounded with them, and treatment instituted upon a false theory and pursued until an excorvation of the spine settles beyond all controversy the character of the disease.

The disease sometimes comes on insidiously; but, in a large majority of cases the attack is sudden and severe, the pains occurring spasmodically, and are provoked by mental as well as physical causes, such as fright, mirth, or violent passion. The pain is usually quite remote from the seat of disease, and may be experienced anywhere in the course of the nerves leading from it, or at their extremities. Consequently, it may be in the side, chest, stomach, bowels, or hips and lower limbs, just according to the locality of the disease. That is one reason why those not familiar with its symptoms (and no one, however great his experience, is infallible on this point) are so liable to be misled, and to confound it with other affections more commonly met with in those regions where the pain exists. The disease is, therefore, often mistaken for neuralgia of the chest, stomach, bowels, lumbago, nephritis, rheumatism, and various other affections; and it was stated in the history given of one case that came under my care, that the child had been treated two years for worms, the treatment continuing some time after the discovery of the curvature, so firm was the belief that worms were the prime and only cause of its illness. A patient will be frequently seized with a paroxysm while playing about the house, when it will throw itself suddenly upon the floor, and refuse to be taken up or handled while the pain continues. When the pain has ceased, the patient will rise and return again to its amusements, as though nothing special had occurred.

This introduction to a sad sequel frequently occurs in the night. A child, which, but a few hours before was put into bed, apparently in perfect health, wakes suddenly out of sleep, in wild agitation, shrieking and crying vociferously. The family is aroused, and the cause of this

sudden alarm carefully sought for, and not unfrequently the solution is found in that very popular theory "an attack of worms;" or, perhaps, a frightful dream. But, in doubt as to which is the real cause, there is administered, both some favourite nostrum as a vermifuge, and a liberal amount of solace. Eventually the pain subsides, the child drops to sleep, and the ever-watchful and anxious mother, full of faith in the infallibility of the remedy, retires again to rest, with the delusive hope that the relief is final and complete. But this is often only the commencement of a series of similar attacks which follow at various intervals, by day as well as night, and, result in protracted sickness and extreme suffering, from which the patient sometimes finds relief only in death. These paroxysms of pain are peculiarly liable to occur whenever the subject first wakes out of sleep; especially so, unless moved and handled with the utmost care and caution, which is not always the case when entrusted entirely to the care of servants and nurses.

Cases occasionally occur in which there is, from the first, but little or no pain experienced, the position of the patient when standing, sitting, or walking furnishing perhaps the only tangible evidence of the existence of the disease. The pain, however, is often so marked and peculiar, as to render the character of the disease nearly unmistakable, even without the appearance of the knuckle to complete the evidence and render it conclusive.

The position and habits of the patient are often such as to attract early attention. He inclines to stoop or lean to one side, and sometimes does both, instinctively thrusting his hands into his pockets, or resting them upon the thighs to give support to the back. He avails himself of whatever object of support may chance to be within his reach upon which to lean, and relieve the affected part from the superincumbent weight of the body. The position is modified somewhat, according to the locality of the disease. When situated in the lumbar region it is, usually, more erect than natural—the shoulders are elevated, and the head thrown backward. If the disease is situated in the cervical region, there is a constant inclination to support the head with the hand placed under the chin, or to throw it backward or to one side, resting it upon the shoulder.

Locomotion, from weakness of the back, is usually performed in a shuffling manner; and not unfrequently does a child, previous to the development of other symptoms, manifest a tendency to trip and fall, for which he is often reprimanded before the cause is understood. A complete loss of power of the lower extremities generally in those cases succeeds this tendency sooner or later, and locomotion for a longer or shorter period is suspended.

Pain and tenderness, upon pressure over the affected region, are very seldom experienced by the patient; but, tapping on it or jarring the spine in any way, is commonly attended with more or less suffering, and a sponge wet in hot or cold water, and applied to it, usually produces pain. Any disagreeable impression imparted suddenly to the back, causing a violent contraction of the spinal muscles, has a similar effect. Placing the patient upon his stomach across a narrow bench or stool, is one of the various methods that may assist, in addition to those above mentioned, in forming a correct diagnosis where doubt in a case exists. In this and various other positions of the body the patient will usually evince more or less suffering, even before any degree of excurvation of the spine is detected.

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INTESTINAL OBSTRUCTION : AMUSSAT'S OPERATION :  
RECOVERY.

Thomas B. Bott, M.D., Surgeon to the Dispensary, Bury, publishes in the *British Medical Journal*, for November 19, 1870, the following case:—

James Walkden, aged 28, brickmaker, in the summer of 1868, whilst climbing a bank, fell back, and the handle of his spade, which was upright in the ground, came into collision with the parts about the anus. He had severe pain at the time, and was driven at once to stool. No blood or anything abnormal was evacuated. He suffered pain in walking for many days. Subsequently, he occasionally had gripings in the lower parts of the abdomen, which caused him to go to stool at once.

Three or four weeks before coming under my care, he noticed that his motions were narrow—not much thicker than a tobacco-pipe stem. He generally went to stool twice a day; but he was rather irregular as to time.

Early in February, 1870, he was seized with violent pain in the bowels. The bowels were not evacuated. He called in the parish surgeon, who gave him purgatives. He had a motion somewhat resembling barm on Saturday, February 12th, 1870. The bowels had not again operated; but the pain increased.

I was called to him on the evening of Thursday, February 17th, 1870. He lay supine. His face was rather shrunken and anxious. He had no sickness now. He suffered from a feeling of tension of the abdominal parietes. The abdomen was very hard to the touch, but not tender on pressure; it was tympanitic, more or less, except in the hypogastric and iliac regions. The left iliac region was especially dull,

swollen, and hard. The rectum was empty. I was unable to reach any stricture or other obstruction with the finger. The parts on the anterior wall of the rectum were very tense—evidently the result of the pressure caused by the liquids and fluids in the bowel. The feeling to the touch was that of a distended bladder; but the introduction of the catheter showed that this viscus was almost empty. At last two moveable hard lumps could be felt on the anterior wall and to the right side. I ordered small doses of tincture of hyoscyamus in camphor-water, and directed the abdomen to be fomented with a hot decoction of poppies, and an enema of soup and water to be given.

Feb. 18th. The enema came back *alone*, immediately after being given. I administered enemata myself—first of soap-water, then of turpentine suspended in water by means of the yolk of an egg. No fæces accompanied the expulsion of these clysters.

On the 19th, my father saw the case with me. Bougies of various sizes, down to that of a No. 8 catheter, were introduced. They all passed about six inches, and were arrested opposite the promontory of the sacrum. I could not detect anything like the hard surroundings of stricture; there was simply no passage. Enemata of tepid water, and subsequently of tobacco-water, were given, without a favourable indication being obtained. Dr. Adam Fletcher saw the case with me on the 20th, and endeavoured to pass bougies. The remedies of the previous day were repeated.

On the morning of the 21st, I inflated the rectum with air by means of bellows, and obtained considerable forcing power by this means; but there was no indication of any satisfactory result. He was wasting in the body. The eyeballs were sunken; the features pinched, and expressive of pain and anxiety.

All our means for obtaining a passage *per vias naturales* being exhausted, nothing could now be done to save the man from a painful death but colotomy. He and his friends were anxious that he should live, and consented to have the operation performed. At 3 p.m., therefore, whilst he was under the influence of chloroform, assisted by my father (Mr. Bott), Dr. A. Fletcher, and Mr. John Parks, I performed Amussat's operation. I made an incision over the left loin, according to the directions given by Mr. Erichsen in his book on Surgery. Having cut through the tissues and arrived at the fatty areolar tissue next to the bowel, I found it to bulge somewhat into the wound. It was of a reddish tinge. We made the bowel fast by sutures attached to the upper and lower edges of the wound. On opening the viscus between the sutures, liquid fæces gushed forth to a distance of upwards

of two feet, sometimes accompanied, and sometimes intermitted, by flatus. Altogether about as much as would fill a chamber-pot was evacuated. When the flow ceased, we washed the wound with glycerine of carbolic acid (B. Ph.). We then fastened the edges of the bowel to the integument at the edges of the wound. Whilst doing this, we noticed that the peritoneal surface of the gut was exposed in front. Some serous fluid poured out from the opening in the peritoneum. We brought the edges of the wound together by means of three stitches and a strip or two of plaster; applied a pledget of lint to the wound, and a thicker piece over it; and over all put a bandage. He was removed, while still unconscious, to bed. He was in that state, with a good pulse, at 4.30 p.m., at which time I left him. I ordered him to have a grain of opium every four hours, to keep the bowels quiet.

7.30 p.m. He had severe griping and cutting pain in the hypogastric region. He felt inclined to relieve the bladder, but was unable. The catheter was used, but the bladder was empty. There was considerable oozing of blood from the wound. Some fæces and flatus escaped from the opening in the bowel. The wound was dressed with lint soaked in dilute oil of carbolic acid, in the proportion of one to eight. A bandage was placed round the abdomen. He was ordered to take beef-tea frequently. I gave him thirty drops of laudanum, and ordered a grain of opium to be taken every hour.

Feb. 22nd, 10.30 a.m. He became easy soon after my visit last night, and went to sleep. He passed urine once during the night. Pulse 140, weak. He looked pale and haggard. The tongue was dry in the centre. He had no pain on moderate pressure over the abdomen. The wound was dressed with lint dipped in Condy's fluid. His soiled flannel waistcoat and sheet were removed, without giving him pain.

Feb. 23rd. He had a good night. Pulse 140. The wound was dressed, and clean clothes put on him. At 5 p.m., he was ordered to take one grain of opium three times a day, and one when any pain came on. He was also ordered egg and milk, alternated with beef-tea.

Feb. 28th. The stitches were all undone. The bowel had receded to the bottom of the wound. Dr. A. Fletcher saw the case with me, and suggested that we should prevent the escape of fæces, except when the wound was dressed. We placed a plug of lint in the opening in the bowel, and packed the remainder of the wound with lint dipped in carbolized oil (one in twenty.)

March 1st. There had been no escape of fæces, and he had a good night; I dressed the wound, after removing the plug and allowing the fæces to escape. It looked healthier. He was ordered to have a mixture of laudanum and hydrocyanic acid every four hours.

March 4th. The wound had been dressed twice a day; it was granulating nicely. Pulse 118. The tongue, which had hitherto been dry, furred, and cracked, was much improved. There was a copious flow of liquid *fæces* to-day. I dressed the wound night and morning, cleaning it first by very dilute Condy's fluid, projected from an elastic bag with a nozzle attached. He took three or four pints of milk daily, besides chicken-broth, beef-tea, and eggs occasionally.

March 6th. He passed *per anum* some hard, buff, feculent substance, preceded by bloody mucus and softened mucous membrane.

On March 10th, he was passing large stools through the wound twice a day.

On April 4th, he passed *per anum* a stool four inches long and half an inch in diameter, of a light yellow colour—in fact, of the same colour as the *fæces* passing by the wound. From its appearance, it was evident that the *fæcal* material had passed in very small pieces through the stricture, and accumulated in the rectum to the size mentioned. It was accompanied by a considerable amount of mucus. The granulations of the wound were on a level with the surface. As the weather was fine, he spent some hours of the mid-day in the neighbouring fields. He passed flatus *per anum* occasionally—about once a week.

April 21st. He passed a stool *per anum*.

May 20th. He passed some blood *per anum*. He had been wearing for some time, in the hole communicating with the bowel, a gutta-percha plug about the thickness of the little finger, with a broad flattish disk about three inches by two at its external extremity. It was mushroom-shaped, and was kept in its place by a bandage. It effectually prevented the escape of *fæces* for twelve hours. If a longer time elapsed before the wound was dressed, the action of the bowels forced the thin *fæces* by the sides of the plug to the surface.

June 3rd. He passed small quantities of yellowish mucus, tinged slightly externally with blood, about once in from fourteen to twenty-one days. I examined the rectum with the finger. I could reach no stricture. The hard rounded tumors on the anterior wall had gone. No hardness or thickening of the wall of the rectum could be detected. He had occasional griping pains in the left iliac region.

On July 30th, he passed *per anum* a reddish yellow mass three inches by one inch and one-tenth in dimensions, somewhat resembling a piece of softened muscle. A thin slice under the microscope appeared to consist of blood and mucus, with a few crystals.

October 1st. He had had no evacuation *per anum* since the last report.

REMARKS.—This case has been quite as successful as such cases can generally be expected to be. The man's life is prolonged for an indefinite period. The most potent prejudice, both with patient and surgeon, against this operation, is a fear lest the former should, if the operation succeed, become a nuisance both to himself and to his neighbours. This man does not seem to be an annoyance either to himself or to others. He has very good control over the evacuation of the contents of the intestine; except that, if he do not allow their escape twice in twenty-four hours, he feels uncomfortable; there is a little griping and some fluid oozes out by the side of the plug. Sometimes, as during a recent attack of diarrhoea, it is necessary to remove the plug oftener. He seems happy, and is gaining flesh. With the experience of this case before me, I would certainly perform colotomy if similar conditions, or any conditions in which this operation was the only means of saving the life of the patient, should present themselves. The operation is no more difficult than that for strangulated hernia; and, if the peritoneum is not injured, it is surely not more dangerous. In this case, although the peritoneal sac was opened, and although, as the escape of serous fluid showed, there was some inflammation of the peritoneum prior to the operation, yet no serious disturbance followed. The irritation caused by the exposure of a mucous surface to the air was well allayed in this case by the administration of hydrocyanic acid.

If I had to perform the operation again, I would, before making an incision, draw a line with pen and ink on the skin parallel with the spine, and halfway between the spine and the left anterior superior iliac spinous process. This would be a guide to the locality of the colon, which I should expect to find behind it and within an inch of it.

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## Medicine.

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### TREATMENT OF CHOREA BY ETHER SPRAY TO THE SPINE.

Dr. John Rose (*Lancet*, December 10, 1870, p. 813) reports three cases of rapid recovery from obstinate chorea by the anæsthetic ether spray. It was applied along the spine for four or five minutes at each time, and effected a cure after fifteen sittings. In obstinate cases he proposes to shave the occiput, and apply the spray there as well as to the spine.

## A NEW TEST FOR HYSTERIA.

A French work just issued by Dr. Chairon, chief medical officer to the Vesinet Asylum, entitled "Clinical Studies on Hysteria," announces the discovery by him of a new pathognomonic sign of hysteria, which, should it be confirmed by experience, will prove to be a valuable contribution to medicine. Since Dr. Chairon has become connected with the Institution he has passed under view 26,000 female patients, amongst whom were a great many cases of hysteria. He says that he has ascertained that in every one of them the commencement of the affection has been marked by a special sign—insensibility of the epiglottis.

The determination of this symptom, which is constantly present, is very simple. It is sufficient to introduce gently the finger into the mouth, so as not to frighten the patient, and place it on the base of the tongue. It will be found that the epiglottis may be touched, displaced and scratched with the nail without producing the least regurgitation. When this system exists there will be found invariably a congestion of one or both ovaries, usually of the left.

Singular as this proposition is, the author proceeds to prove its exactitude, and has with that object, quoted a great number of cases collected at Vesinet.

## MAMMARY ABSCESS AND ITS REMEDY.

BY JOSEPH R. BECK, M.D., Lancaster, Ohio.

Among all the troubles incident to, or connected with, child-bearing there is none so prolific of bad results, both to the medical attendant and to the parturient patient, as a mammary abscess. The occurrence of such an abscess in his patient has lost many a physician his reputation in a whole family, and very frequently not only in the immediate family of the sufferer, but even in the whole of a wealthy and influential connection. The attention of the profession cannot be too strongly directed to this fact, that the occurrence of a mammary abscess in a patient recently delivered, and still under observation, is generally attributed to the neglect of the physician in charge. This may appear to be a wholesale accusation, but my opinion is based upon close observation, and seems to me to be fully sustained by the facts in the case.

It is not the purpose of this article to treat of the symptoms of this disorder, nor to enter upon a discussion as to the relative merits of different plans of treatment, but simply to give the views of *one* observer upon the mode of effectually preventing any abscess of the mammary gland from troubling either the patient or the obstetrician.

The symptoms of inflammation of the gland under consideration are well known to the profession. Whenever these arise, every effort should be made to arrest the secretion of milk; this will relieve the mother, and not necessarily interfere with the well-being of the child, which, if proper care be taken of it, will generally be found to thrive upon good and pure cow's milk, with the occasional addition of a small quantity of lime-water.

The treatment, therefore, is to be begun as soon as there are any symptoms that mammary abscess is likely to occur. I have found the following prescription of service: Alcoholic Extract of Belladonna, four drachms; Glycerine a sufficient quantity to mix them to the consistence of a moderately thin paste. This is to be spread in a medium thick layer with a spatula, over and upon both mammary glands, from the sternum to the axilla. Cover with a cloth dipped in olive-oil, and this in turn with oiled silk. Allow the dressing to remain undisturbed during a variable period of from two to three or four weeks, inasmuch as it can be worn by the patient for any length of time without inconvenience.

The argument in the case is directed, of course, to threatening abscesses; but all will at once recognize the appropriateness of the treatment in cases of still-born children, where it is certainly desirable to arrest the secretion of the milk at once. In these cases apply the remedy within an hour or two after the birth of the child. I have never known this treatment to fail of its desired effects, where it was used in time.—*Medical Times, Philadelphia.*

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#### RAW BEEF IN ANÆMIA.

James S. Bailey, M.D., Albany, N.Y., writes: In the anæmia of young girls just verging into womanhood, there is nothing that so speedily relieves this condition as lean raw beef.

There is usually an aversion to any food, at this period, of a solid nature; but upon the contrary, a craving for pastry, pickles and knick-nacks, &c.

Upon many occasions I have insisted strenuously upon a change in the mode of living in this respect, and placed patients, if not upon raw beef, upon beef underdone, for a diet, and have invariably experienced marked improvement. It imparts tone to the organs of digestion; by it the blood made is improved in quality.

I am quite certain, in tardy menstruation, if instead of administering ferruginous mixtures, should the diet alone be changed as suggested, and the meals served regularly, with an abundance of exercise in the

open air, many females, instead of dying prematurely, would be restored to usefulness.

Many women acquire the habit of abstaining from food at breakfast, and soon, if food is taken, the stomach rebels. Experience has proved that the healthy stomach requires a supply of food every six hours during wakefulness. Any departure from this established law produces derangement in digestion, and the system must necessarily suffer the penalty for this infringement upon the laws of health.

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## Midwifery.

### LEAVING OPEN THE ABDOMINAL CAVITY AFTER OPERATION.

In the proceedings of the Gynecological Society of Boston, reported in the April number of the Society's Journal, there is a case of extirpation of the uterus, in which this plan was tried.

The operation was performed before the war, by the late Dr. Baker, of Knoxville, assisted by Dr. Boyd. The patient recovered, and still remains well. A portion of the abdominal wound was purposely left open after the operation, freely exposing the peritoneal cavity to the external atmosphere; it was believed by the surgeons in charge that to this fact was owing the patient's recovery. The colored people in Tennessee, when spying sows, leave the abdominal cavity open after the operation, and it is said that the mortality is very much less than when the cavity is closed.

This is the only case in which surgeons have ventured on such a proceeding, and there will probably be great hesitation in following the example, though the Society had little to say against it. The great aim of ovariologists has hitherto been, in every way possible, whether by closely set sutures, bandaging, or carbolic acid paste, to hermetically seal the external wound.

In dealing with the abdominal cavity, however, Dr. Storer, in remarking on the above case, stated he had often questioned whether it were well to close in so thoroughly, what under other circumstances, would seem to afford the surest invitation to peritonitis and septicæmia.

He added that there could be no doubt that exposure of the peritoneum to the atmospheric air for a long time might be permitted without subsequent ill-effect. He had frequently been compelled to do this during a period of from one to four hours, for the purpose of providing against hæmorrhage from broken adhesions, during abdominal sections, and he thought it was well worthy of research whether Dr. Baker's course was not based upon a wise common sense.

# Materia Medica and Chemistry.

## THE ACTION OF MERCURY ON THE LIVER.

In a paper read before the Medico-Chirurgical Society of Edinburgh (*Edin. Med. Jour.*, April, 1871) Dr. Thos. R. Fraser reviews the whole subject of the cholagogue action of mercury. After considering various doctrines as to the nature of this action, he says that all that has been actually demonstrated is the increased flow of bile after the administration of mercury. Dr. Fraser believes that this is supported by the following arguments:

1. Certain characters of the alvine dejections imply an absence or diminution of bile, and these characters are present in various diseases.
2. In many of these diseases mercury restores the alvine dejections to their normal condition, or produces in them, as well as in normal dejections, certain characteristic appearances.
3. The characteristic appearances caused in the alvine dejections by the administration of mercury are due to the presence of bile constituents.

The appearances caused by mercury cannot be due, as suggested by Murchison, to the merely purgative action of the drug, whereby the intestinal contents are so rapidly carried to the rectum that the modification and absorption of their bile constituents which occur in normal digestion are prevented, because a similar effect is not found to follow the use of other purgatives, and because, in constipation, where the passage of the intestinal contents is undoubtedly retarded, there need not be clay-colored stools, while on the other hand, in diarrhœa the stools may be pale or clay-colored.

These views are opposed to those recently promulgated by the Edinburgh committee, and Dr. Fraser is disposed to believe that, in the vivisections made by the committee, several nerves ramifying in the substance of the common duct, and in the surrounding tissues, were necessarily divided, and that it may be that by these nerves an influence is conveyed to the liver by the action of mercury.

## ON TINCTURE OF HYOSCYAMUS.

By M. DONOVAN, Esq.

Some years since I published, through the medium of the MEDICAL PRESS, an account of trials made on myself and others, with a view to discover what dose of tincture of hyoscyamus should be given in order

to produce its sedative effects. The experiment was made on several persons, beginning with a drachm dose, increasing it to six drachms, and in my own case to one ounce, of the tincture of the *Dublin Pharmacopœia*. In no case were any effects observed beyond dryness of the throat and fauces. The experiments were made with tinctures prepared from the dried leaves of garden-grown plants, from wild plants collected in a mountainous district of North-Wales, and from the same leaves dried and undried.

I was under the impression that some of the plants employed in making the tinctures on which I experimented were in the second year of their growth, but the trials now to be described have convinced me that none of them could have been more than one year old. At that time I was not acquainted with the means which I have since discovered of testing the age of the plant.

I satisfied myself by these experiments that tincture of hyoscyamus prepared, as I believe it generally is in this country, from leaves of one year's growth are all but powerless. I was strengthened in this opinion by finding that M. Hertz has given upwards of fifteen grains of the extract, most probably made from the plant in its first year, without any sensible effect.

Mr. Houlton had long before affirmed the inertness of the one year old plant, and the activity of that of two years' old.

In order to come to some determination on this subject I adopted means of procuring a tincture certainly made from the latter, and from trials with it soon convinced myself that it was an article of very different value from a tincture of the one year old plant, and that all my former experiments must have been made with the latter, although I was led to believe that, in some of them, the plant of two years' growth had been used.

My first trial was on myself. I took one drachm, and for an hour or two felt no effect beyond dryness of the mouth. On a subsequent occasion I took two drachms, and in two hours had proof that I had taken a sufficiency. My sensations were indescribable: one was a feeling of uncertainty of my steps in walking, although they were really quite steady, and a slight sensation of giddiness. This trial convinced me that I had taken as full a dose as prudence would permit. To a lady who suffered from headache I gave, at her own request, one drachm of this tincture. In about two hours she felt so overcome by sleepiness that she could scarcely keep her eyes open; the headache was, however, greatly relieved. On another occasion she took a similar dose, and,

being in bed, she soon fell into a "delightful sleep," and, on awaking, found that the headache was almost gone; but she complained of dryness of the fauces and throat, although on the first occasion she did not experience either of these effects. Some months after the same lady suffered from headache, and did not receive any benefit from a similar dose; nor did another person experience any relief from toothache nor any other effect beyond slight dryness of the fauces, which soon passed off.

Convinced by the foregoing considerations that the medicinal properties of hyoscyamus reside exclusively in the plant of two years old, and that the plant of one year's growth is therefore useless, I sought to discover an easy test by which the age of the plant from which a given tincture had been prepared could be determined. The following has at least the advantage of simplicity: add a little of the tincture to a glass of water; if the mixture become slightly milky, the tincture was made from a two years' old plant; if it remain transparent, the plant was in its first year.

The *British Pharmacopœia* gives no information as to what shall be the age of the hyoscyamus from which the tincture is to be made; it is, therefore, a matter of chance whether it will have any effect or be powerless. Given in the dose of twenty or thirty drops, as is sometimes done, it is hard to believe it can have any effect in either case.—*Dublin Medical Press.*

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#### ICE PER RECTUM IN CHLOROFORM NARCOSIS.

Dr. Baillie says (*L'Opinion Médicale*) that nothing is better in the narcosis of Chloroform than the introduction of a morsel of ice into the rectum. A slight pressure on the sphincter relaxes it, the ice slips in, and immediately a deep inspiration is produced. This is the prelude to natural respiration and a restoration of the cardiac functions. He also recommends the remedy being tried on children born apparently dead.

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#### ARNICA IN PNEUMONIA.

Mr. C. C. Balding recommends strongly (*London Lancet*) the use of arnica (min. x strong tincture every two or three hours) in pneumonia. The pulse should be reduced by it to 60 or 70, and descends at times as low as 40 per minute. The relief is immediate and marked.

## IODIDE OF IRON AS A REMEDY IN INCONTINENCE OF URINE.

In the *Medical Times and Gazette*, Dr. John Barclay, after a very long list of the "constitutional, moral, mechanical, and specific" remedies and methods of treatment in this disease, says, "I have tried several of the above remedies, and before I stumbled upon the syrup of the iodide of iron, found atropia or belladonna by far the most certain and trustworthy. Tincture of iron is much employed, but after frequent and persevering trials with it I have been always disappointed. During the past two and a-half years twenty cases of incontinence of urine have been treated by me. The medicine invariably prescribed has been syrup of the iodide of iron alone, and, so far as I know, there have been no failures. I have notes of all the cases, but only eleven in the completed state, since the other nine, who came from a distance, did not return to say what was the result. The probability is that they were cured, otherwise they would not have been got rid of so easily. At all events, the eleven who did report themselves, or who were continually under observation, were all cured, the improvement in several of the cases following so closely upon the administration of the remedy, as to leave no doubt that the good effect was due to the syrup. Dr. Manson, of Banff, and Dr. Smith, of Kinnairdy, have both found the medicine equally satisfactory. Dr. Smith says that he tried it, only a fortnight ago, on a boy, who for a long time had been a sad martyr both to diurnal and nocturnal incontinence and who had resisted all other remedies, but who, upon giving him the iodide, was in two or three days almost well." The doses given were from fifteen minims to half a fluidrachm three times a day, according to age.

## SOLUTION OF SANTONIN.

Dr. John Harley (*The Practitioner*, Feb. 1871) gives the following formula for a solution of this ordinarily so insoluble remedy:

R Santonini, in pulvere, gr. xij  
 Sodæ bicarbonatis, gr. xx;  
 Aquæ destillatæ, oz. iij.

Put the soda and water into a flask, keep the fluid near the boiling point, adding, as it disappears, about two grains of the santonin at a time, until the whole is dissolved. Solution is effected in about half an hour, during which time the water is reduced to oz. ij. If need be, reduce by boiling to this bulk, when oz. i will contain a full dose,—six grains of santonin. If an alkaline reaction be objectionable, neutralize with acetic acid.

# Canada Medical Journal.

MONTREAL, JUNE, 1871.

## THE HOWARD SYSTEM OF VENTILATION.

The subject of ventilation of public buildings has long engaged the attention of sanitarians; and of the various methods proposed, all present some objectionable point, so that perfection has not, so far, crowned the efforts of those who have devoted their energies in this direction.

Dr. Henry Howard of St. Johns, the worthy superintendent of the Provincial Lunatic Asylum, of the Province of Quebec, has been forced to enter the lists in sheer self-defence. A more wretched building does not exist on this Continent, possessing the name of a lunatic asylum, than the institution over which he is forced to preside. The building was a few years ago used as a court-house, although we believe it was not originally built for that purpose. At the time of the Trent difficulty, when the Imperial Government in their wisdom sent an additional military force to this country, the old barrack at St. Johns, which had been selected by the Canadian Government as a temporary lunatic asylum, was re-occupied for military purposes by the Imperial authorities, Dr. Howard was forced to relinquish that site, and the present building which he occupies was temporarily given for his use, in lieu of the more extensive and commodious buildings known as the St. Johns barracks. We have repeatedly entered a strong protest against the present system, as more expensive than would be the erection of a new building; expensive in every respect, as it is utterly impossible to adapt such a building for the purposes of a lunatic asylum, besides which the constant outlay on an old building, the alterations necessary, the patching and repairing, in the long run costs more than a new establishment. But what should weigh more in the counsels of those whose business it is to provide such establishments is the fact that there is no chance of benefit being derived by the unfortunates confined there. As to the scientific treatment of the insane under such circumstances, it is out of the question. There is, therefore, every reason on the score of economy, if on no other grounds, for a change in the present system. The sooner the Government of Quebec adopt the idea of

change in this respect, the better for the country at large, and the sooner will be removed the stigma of possessing a building in which afflicted human beings are housed which is hardly suitable for animals of the porcine species.

We say that Dr. Howard in sheer self-defence was forced to seek some efficient means of improving the air of his Asylum. From personal inspection we can bear testimony to the success of his efforts. When some five years since we visited his Asylum, we were simply horrified with the condition of things. Improvement certainly has followed on his exertions, and we have reason to know that these experiments have been attended with considerable outlay. We presume that the expense of these various trials or experiments have been borne by himself, as we all know that in Government enterprises great care is adopted to prevent unnecessary outlay, every farthing has to be satisfactorily accounted for, and the expenditure of large sums of money for mere experimental purposes would not be allowed. We visited the Asylum at St. Johns in April last, and rooms and dormitories which were in 1865 pestiferous, were in 1871 sweet and endurable. No more positive evidence, therefore, can be adduced of the success of Dr. Howard's method of ventilation than the results observed in his own Asylum.

Dr. Howard's system consists in the admission of fresh air at the upper part of a room; the air passes along the ceiling and falls as spray to the floor; in its transit it must to a certain extent mix with the foul air of the apartment, but only to a slight degree, while the latter being of higher temperature, is floated to the upper strata, and is carried off by an upward shaft at the ceiling. This shaft is in connection with an air-tight receiver, having within it a fan of four blades; this is kept constantly revolving by means of a clock-work arrangement and weight; to the receiver is attached a discharge tube, which passes out out above the roof of the building. The revolution of the fan produces a vacuum, and must draw the air through the tube which opens at the ceiling of the apartment to be ventilated; this air is then forced through the discharge tube, and is rapidly diffused or carried off by the wind.

From experiments instituted by Dr. Howard, it would appear that the largest quantity of foul air in a room twenty feet high, when there is defective ventilation, is about two feet from the ceiling; a room of the same height yielded a large proportion of foul air ten feet from the floor, but there was almost perfect absence of impurities at the ceiling and at the floor. From these facts it would appear that a discharge tube would be more efficient if opened at two feet from the ceiling in

an apartment of twenty feet in height, and one foot in a room of ten feet altitude.

We need hardly allude to the urgency of ventilation. To secure a healthful condition of the body, constant change of the air of an apartment is an absolute necessity. The emanations from our bodies are such as to render any apartment in time highly poisonous, so much so, as to preclude the continuance of life. Decay is constantly going on in our bodies, and an arrest of these changes is inconsistent with the continuance of life. The problem consists in getting rid of these effete particles after they have been separated from the living mass. Hence the urgency of adopting some rational system of ventilation. More especially is this urgently necessary in public buildings, schools, gaols, lunatic asylums, churches, and other buildings where large numbers of persons are met together.

The Court House of our City has been long known to be very deficient in ventilation. So much so that on more than one occasion the Judges on the Bench have been obliged to relinquish their official duties through indisposition. The Quebec Government have wisely determined to endeavour to improve the present state of that building, and we believe that Dr. Howard has in hand the contemplated improvement. If he succeeds as well as he has in the Lunatic Asylum at St. Johns, it will go far to secure the confidence of the public in his invention, and will, we trust, lead to the adoption by other ill-ventilated public buildings throughout the country of the Howard system of ventilation.

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#### MEDICAL COMFORTS FOR THE MILITIA.

Many of our readers are aware that the Government of Canada deemed it expedient to form camps in different parts of the Dominion, and a special sum was voted for militia purposes. We believe that as a rule the experiment has succeeded, and although it is hard to please everybody, yet the larger mass of the volunteers were satisfied, and convinced that everything was done to make them comfortable while under canvas. We speak from personal knowledge, as we ourselves were on duty at the camp at Laprairie. The object we have in writing is to call attention to the Medical Field Companion, or whatever it may be called, which was issued to each Regiment. In our opinion it contained all that is requisite for service of the kind undertaken by the volunteers.

We have heard of some grumbling, and one daily paper compared it to a spice box, and styled the gentleman who was entrusted with the

getting up of the box, the Old Woman of Montreal. This as a *jeu d'esprit* is all very well, but it only shows the utter ignorance of the writer.

According to the regulations issued by authority, every volunteer before going into the field should be subjected to medical inspection. The object of this being to reject all who are physically incapable of bearing the fatigue of a campaign, and who would be an encumbrance to an army on the march. Another requirement being to send into hospital all men who may be taken seriously ill, and who would be necessarily neglected if allowed to remain in camp.

With regard to the contents of the "Medical Spice Box," they consist of those articles which certainly have been in the Pharmacopœia from time immemorial, but are all that are requisite for sudden attacks of illness. Furthermore, the Medical Spice Box is more complete than the "Medical Field Companion" used by the British army in the field, and we think that those members of the profession who are desirous of having modern inventions added, such as corn plasters, toilet powder, and such like articles, forget that they would encumber their baggage with useless material, and run the risk of becoming the laughing stock of many hardy old veterans who are to be found attached to all our volunteer battalions. We certainly think that 'the Old Woman of Montreal' deserves the thanks of the members of the medical staff of the militia, as perhaps it is not generally known that that gentleman was instructed to use great economy, and get up a cheap but useful box.

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#### DEATHS OF PROFESSORS WAGNER AND NIEMEYER.

From the *British Medical Journal* we learn that among the victims whom the medical profession has furnished in connection with the recent Franco-German war, have been two men of more than common note,—Professor Albrecht Wagner of Königsberg, who died at Dole on February 15, and Professor Felix von Niemeyer of Tübingen, who has died lately at Nancy. The cause of death in both cases was typhoid fever, contracted in the discharge of their duty. Dr. Wagner was well and favourably known in Germany for his works on Resection and Regeneration of Bone (translated a few years ago by the New Sydenham Society), on Hydrophobia, Diabetes in connection with Carbuncle, Resection of Nerves, etc. The name of Felix von Niemeyer is well known among us through the translation of his excellent *Text-Book of Practical Medicine*, and his *Lectures on Phthisis*.

## A MOVE IN THE RIGHT DIRECTION.

Under this head the *Boston Medical and Surgical Journal* has the following excellent remarks :

"In our own practice we are constantly cognizant of cases of malpractice on the part of apothecaries who overstep the bounds of their legitimate business. That a pharmacist occupies a corner store in a crowded locality, and enjoys a local repute as a 'Doctor,' is no reason that he should treat venereal diseases, surgical injuries, and supposed constipation, or prescribe for 'the chiel who is a little ailing,' but may be on the threshold of serious disease. It is true, the patient, who may have but little money in his pocket, gets his advice for the price of the medicine administered, but the remedy is often dearly paid for by aggravation of disease, when a moderate fee to our younger brethren would secure sound advice and a satisfactory cure.

"We cannot help calling the attention of our friends the apothecaries to a sign we have just seen conspicuously posted in the shop of one of their own number. It is not for our sakes alone, but for their own good, that we advise them also to set up as a public notice,—'We are pharmacists, but not physicians; we dispense medicines, but do not prescribe for diseases;' and when they have done so, we trust they will keep to their own legitimate calling and allow physicians to *treat* diseases."

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*To the Editors of the Canada Medical Journal.*

GENTLEMEN,—In the brief report contained in the May number of the *Canada Medical Journal*, of the few observations upon a case of pleurisy and miliary tubercle made by me at a late meeting of the Medico-Chirurgical Society, I am unintentionally represented as attributing views to the late Professor Niemeyer which he would disclaim could he read the report, and as holding an opinion upon the great pathological question discussed that I did not advance. The tenor of my remarks was as follows: "The point of greatest interest to myself in this case is the combination of pleurisy with effusion and miliary tubercle of the lung and kidneys in an old man, and the relation of the lesions to each other." Niemeyer and his school would maintain that the pulmonary tubercle was secondary to and a consequence of the pleurisy; but I did not think that that doctrine had been proved as yet, and I was rather disposed to believe that the tubercle preceded the inflammation of the pleura, or at most that the constitutional deterioration and local disturbance of nutrition incident to the pleurisy had evoked a pre-existing tendency to tuberculosis. It was a question of great scientific interest, which I was not prepared then to discuss, but which I submitted for the consideration of the Society." The patient took *one* of Addison's pills, not "two" twice a-day—and thirty-five or forty ounces of fluid, not "about five ounces," were removed from the right pleural cavity after death.

Your obedient servant,

R. P. HOWARD, M.D.

Beaver Hall Hill, 18th July, 1871.