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The KINGSTON MEDICAL QUARTERLY is presented to the Medical Profession with the compliments of the Editorial Staff. Contributions will be gladly received from members of the Profession and willingly published. JOHN HERALD, Editor.

DOMINION MEDICAL REGISTRATION.

FOR years it has been felt that the present system of Provincial Licenses to practise medicine was anomalous. The Canadian Medical Association took the matter up and appointed a Committee with Dr. Roddick as chairman to report upon the practicability of Inter-Provincial Registration. The Committee considered the question and reported favourably, and their report was adopted by the Canadian Medical Association. In the QUARTERLY of April, 1897, we criticized the Committee's report. We felt then, and still feel that Inter-Provincial Registration was so beset with difficulties both present and prospective that we were constrained to come to the conclusion that Inter-Provincial Registration was an impracticable scheme, and we concluded our criticism of the Committee's report with these words, "Would it not be well for the Canadian Medical Association to drop this question of Inter-Provincial Registration which is so beset with difficulties both present and future, and consider the wider and, to our mind, the more feasible question of a Dominion Council with a Dominion License." We refer to this criticism of the Committee's report to show that we have always been in favour of a Dominion Council and a Dominion License, and that anything which we may now urge against the scheme as at present proposed is not in antagonism to the general principles of the scheme, but against details. The Committee already referred to was continued and under the chairmanship of Dr. Roddick has laboured industriously and has crystallized its proposals into a Bill which it is proposed to submit to Parliament at its next session. A copy of this Bill we now have before us:

We find that Inter-Provincial Registration is dropped and that it is now proposed to form a Dominion Council which shall have power to issue on certain conditions a Dominion License. As this proposal is in accordance with the views which we expressed nearly three years ago, we naturally accord it our hearty endorsement.

With all of the details of the proposed Bill, however, we cannot agree. By Section 6 it is proposed that the Council shall consist of 24 members—three representing each of the seven Provinces, and three representing the Territories. Now it seems to us that this is unfair. Ontario and Quebec, with an approximate population of four millions, would have only six representatives, and the rest of the Dominion, with an approximate population of one and a half millions, would have eighteen representatives. We are strongly of opinion that any scheme likely to prove satisfactory and acceptable must be based on fairness and justice, and that any proposal to establish a Dominion Council which unduly favours any section of the country is inevitably doomed to failure. Desirous as we are of seeing a Dominion Council established, we trust that its permanency and efficiency will not be jeopardized by such a manifest injustice. We clearly recognize that the smaller Provinces, fearing that they would be controlled entirely by the two larger Provinces of Quebec and Ontario, if representation in the Council were based upon population solely, are justified in asking for a larger representation than they would be entitled to on the basis of population. We are prepared to grant them this larger representation, but not to the extent proposed in the Bill. This would throw the control of the Council into the hands of the smaller Provinces. In the one case we would have the greater controlling the smaller. According to the proposed Bill we will have the smaller controlling the larger. Neither state of affairs is desirable. On a basis of representation by population a council of 24 members would have its representatives allotted as follows:—Prince Edward Island, Nova Scotia, New Brunswick, Manitoba, British Columbia and the North-West Territories, each one; Quebec, seven; and Ontario, eleven. No one, we believe, would desire to insist on this composition of the Council, and yet such an arrangement would only give to Quebec and Ontario, the numeri-

cally strong, what the proposed Bill would give the numerically weak, viz., 18 representatives. If it is unfair to give to Quebec and Ontario what their population would entitle them to, is it not much more unfair to give to the rest of the Dominion the same representation, or just exactly three times the number of representatives their population calls for? Now we recognize that it is much easier to find defects in a scheme than it is to remedy those defects, and we feel that he who points out a defect should be prepared to suggest an improvement. We accept the responsibility. We do not ask for representation by population in its entirety, but we claim that population should be to a certain extent recognized in the composition of the Council. We would suggest that the 24 representatives be allotted as follows: Prince Edward Island, Nova Scotia, New Brunswick, Manitoba, British Columbia and the North-West Territories, each two; and Ontario and Quebec, each six. This would give to the four millions of Ontario and Quebec twelve representatives, and to the one and a half millions of the remainder of the Dominion twelve representatives. Thus the representation of the numerically weak Provinces would be doubled, and that of the numerically strong Provinces reduced by one-third. This, it seems to us, is a fair concession for the majority to make to the minority. The adoption of this suggestion would necessitate some change in the method of appointing the representatives. As the Presidents of the various Provincial Councils hold office for only one year, and the other representatives are to be appointed for four years, we would suggest for the sake of uniformity that the clause making the Presidents of the Provincial Councils *ex officio* members be struck out. In those Provinces having two representatives one would be appointed by the Governor-General in Council and one by the Provincial Council, and in Ontario and Quebec one would be appointed by the Governor-General in Council, and five by the Provincial Council. All representatives would be appointed for four years.

In the proposed Council, the Universities and the Medical Schools are entirely ignored. Why? The Council shall prescribe the course of studies and conduct the examinations of those desirous of obtaining the Dominion License. Now, it does seem to us that in matters of education the men whose lives

are devoted to that work should be the best fitted to give advice on the question of a curriculum, and upon the best methods of conducting examinations. We would, therefore, suggest that the Council be increased by the addition of one representative from each University in Canada that has a Medical Faculty actually engaged in giving instruction to Medical students.

One point more as to the composition of the Council. By the proposed Bill every representative must be a medical man. One naturally asks Why? Medical men are not the only ones who are interested in Medical education. The general public have as much interest in seeing that medical practitioners are properly equipped for their work as have medical men. By leaving those who have the appointment of representatives to the Council free to appoint whom they choose an opportunity would be given them to appoint any one whose qualifications would fit him for the discharge of his duties, as a member of Council, whether he was a medical practitioner or not. Thus, too, would be removed from the public mind a suspicion which has been more or less generally entertained regarding Provincial Medical Councils that they are nothing more nor less than close corporation formed for the benefit of the medical profession at the expense of the general public. We do not mean to affirm that this suspicion has in any case been well founded, but we re-assert that such a suspicion has existed and has been the cause of more than one attack upon these Provincial Councils. By the plan here suggested the grounds for this suspicion would be removed, the profession would in no way be injured, and the Dominion Council would meet with a fuller endorsement at the hands of the general public.

We would now draw attention to Section 9. By this section it is proposed that the first meeting of the Council shall be held in Ottawa, and thereafter an annual meeting shall be held at such time and place as may from time to time be appointed by the Council. We are of opinion that both the time and place of the annual meeting should be fixed. As to the time, we would suggest a date just after the annual meetings of the Provincial Councils. As to the place of meeting it seems to us that there can be no question. This is a Dominion organization and ought naturally to meet in the capital of the Dominion.

Subsection 1 places a quorum at seven. We think this is too small—not one third of the whole Council. This should be increased and especially so if University representatives are added.

By Subsection 2 a deliberative and a casting vote are given to the President, or the Vice-President when in the chair, and also to the chairman of any meeting of the Council or of any committee of the Council. This we feel is unwise and places too much power in one member. If this power is given at all it should be limited to the President, and to the Vice-President when acting as President.

By Section 15, Subsection 2, we find it is proposed that until otherwise decided, all practical and clinical examinations shall be held in Montreal and Toronto alternately. This we regard as highly objectionable. These examinations should not be held at the seat of any medical school unless they are held at the seats of all such schools. We must, however, strenuously oppose any proposal to unduly favour any medical school at the expense of or to the injury of any other. By the proposal all going up for examination would have to go to either Montreal or Toronto. Those medical students attending the schools at those places could go up for their examinations at their own door without extra expense, while those who were attending the schools at Halifax, Kingston, London and Winnipeg would have to travel to Montreal or Toronto. Now one can readily understand what the effect of this would be. A student who intended to take the examinations of the Dominion Council would naturally say, if I must go to Montreal or Toronto for examination for my license I will save expense by going to one of the medical colleges in either of those cities. Thus the Dominion Medical Act becomes a means of building up the Montreal and Toronto schools at the expense of all others. Now, we do not presume that those who framed the Bill had any design of this kind. We feel confident that when this effect of the Bill is pointed out the gentlemen who have the matter in hand will see that this objectionable, unjust and partizan feature is removed. What remedy do we propose? Simply that all written examinations shall be held simultaneously and all practical and written examinations in succession at those

centres at which there are medical schools. The papers could be sent to those centres under seal, and a presiding examiner could be appointed at each place. For the practical and clinical examinations examiners would have to go to the various centres. To this plan it may be urged that it would be expensive. True. So would the other plan. The difference is that by the plan now proposed the expense would fall upon the Council, whereas by the plan suggested in the Bill the expense would have to be borne by the students who, as a rule, can ill afford extra expense. By this plan no school would be unduly favoured and no school would be prejudicially legislated against. Unless this most unfair provision is removed from the Bill, we will be compelled unwillingly to most strenuously oppose the Bill both in and out of Parliament.

We have now frankly given our opinions upon the provisions of this Bill. As heretofore, we are in favour of a Dominion Medical Council, but we must insist that in the details of the Bill justice must be done to all and injustice to none.

*NOTE ON THE OPERATIVE RELIEF OF
ECTOPIA VESICÆ.

MY attention has been lately directed to this condition, on account of a young patient æt. 8 mos. suffering from the unfortunate results of this form of defective development, and, on reviewing the literature of its surgical treatment, one is led to the conclusion that an ideal operation has not yet been devised for Ectopia Vesicæ. There have been two lines of operative treatment pursued:—one plastic, recovering the exposed posterior wall of the bladder by means of skin flaps—the other placing the urinary receptacle deeper in the pelvis, either by diverting the urinary passages, or by approximating the innominate bones.

In the case of the Tendelenberg operation,—approximation of the bones, or that of König-osteotomy of the pubic bones, the

*Read before the Kingston Medical and Surgical Society, October, and published in *Annals of Surgery*, Dec. 1899.

urine being in contact with the line of junction of the symphysis, will necessarily interfere with union. Added to this there is the probability in the female of a resulting deformity of the pelvis. In the operation for diverting the urinary channels towards the rectum, the great objection is, the possible infection of the kidney from an ascending ureteritis, besides which the urine is, as a rule, an irritant to the rectal mucous membrane. To overcome this possible infection Maydl transplanted the bladder and ureters intact into the rectum, the expectation being, that the oblique entrance of the ureter would act as a valve as in the normal bladder, but the rectum would not contain sufficient urine to mechanically compress the ureter entrance, and hence, Fowler devised a special valve—a tongue-like projection—which may be a success, but there have not been a sufficient number of successful cases of this operation reported as yet, to allow one to judge. (*Annals of Surgery, April, 1899*).

Gersuny converted the rectal pouch into a receptacle for urine only, by making an artificial anus and shutting off the upper end of the rectum entirely. Besides the dangers mentioned as objections to these transplanting operations, there is the important fact that they all necessitate the total extirpation of the bladder. In the plastic operation, on the other hand, the bladder is retained, and an attempt is made to cover up the defect by a flap, generally of skin. Ayers, of New York (1858), and Pancoast, of Philadelphia (1859), first used the flap method in America, and since then many modifications of this method have been proposed, as Wood (1887), by whom an upper flap was reversed and placed with its skin surface towards the bladder, the raw surface being covered by means of two lateral flaps. (An improvement in the technique of this was devised by Forest Willard, Philadelphia Academy of Surgery, 1898).

Thiersch used granulating flaps, and Rutkowski (*Annals of Surgery, 1899*), instead of using the skin as a flap, brought down a portion of the intestine, which he had first excised, and leaving it still to receive its vascular supply through the attached mesentery, sutured it to the edges of the defective bladder. The rationale of this latter operation emphasizes the defects that have characterized the flap method all through, viz.: an attempt is made to remedy a defect in a muscular organ by means of tissue,

skin and fascia, which does not possess any contractile muscular action. Again, the presence of hairs in the inturned skin tends to the formation of calcareous deposits; the macerating and irritative effect of the urine induces inflammatory action in the flap, and the absence of any sphincter-like muscle necessitates the constant use of a pad to block the outlet of the built-up bladder. Hence Rutkowski substituted the muscular structure (bowel) for the skin flap, but his operation presupposes a procedure risky, to say the least, viz., intestinal resection.

In view of these varied objections to the present status of reparative surgery of the exstrophed bladder, it seems to me that the possibility of replacing the skin flap of Wood, or the bowel flap of Rutkowski, with a flap of normal bladder tissue, if it could be carried out, would be preferable, and with this object in view I undertook some experiments on animals, to ascertain whether it would be feasible to transplant the bladder of one animal into the body of another. These experiments were successful. The method was as under:

Opening the body on one animal—a dog—I removed a piece of the bladder wall and placed it in warm boracic solution, and made a straight incision in the abdominal fascia of another animal (both being on the operating table at the same time) and down to the deep layer of superficial fascia. I separated these two layers, namely, the deep and the superficial of the superficial fascia, and transferred the excised bladder wall to this space m.m. downwards, interposing a sheet of gold foil between the m.m. and the deep layer of superficial fascia, the object being to prevent union of this surface with the subjacent tissue, and the gold foil and bladder tissue were sutured to the integument and fascia by means of catgut. The results were perfectly satisfactory, and demonstrated that it was possible to transplant bladder tissue to the superficial fascia.

In view of the above results, it seems to me that one is justified in suggesting the following procedure in *Ectopia Vesicæ*:

Transplant a portion of bladder wall of, say a sheep, to the lower lateral abdominal fascia of the patient. After a period of seven or eight days, the union of the two tissues is sufficiently strong to allow a plastic operation, whereby a skin flap with the bladder attached may be swung over upon the extruded bladder,

and the edges of the attached piece of bladder sutured to the defective bladder. The skin flaps will afford sufficient nourishment to the excised bladder until union has occurred between the bladder edges, then the superimposed skin may be separated from the now perfect bladder. By employing a transplanted piece, which has been removed from the lower portion of the bladder of the animal, the aggregation of circular muscular fibres will approach very nearly to an ideal sphincter. I would suggest seven or eight days for the union of the transplanted bladder to the fascia, since we know that the fate of most transplanted tissues is to lose their normal structure, and become converted into connective tissue. In one case in which I left the transplanted piece in the tissues twenty-four days, the histological structure was almost lost, as seen in the accompanying report of Dr. W. T. Connell:—

“Serous coat has become completely vascularized, and from it a small number of vessels pass into the muscular coat. This layer everywhere shows the outlines of the muscular fibres, but these are in all stages of degeneration. The mucous coat is also vascularized, the mucous membrane as such having disappeared, and being replaced by vascular granulation tissue. The epithelium has completely degenerated. The vascularization has occurred over the edges of the gold foil.”

Even though some slight changes do occur in the transplanted bladder, in the eight days mentioned above, yet I believe the new vascular supply from the defective bladder, as well as the stimulating action of the urine in the exposed *m. m.*, would prevent any further connective tissue changes. Should, however, there be no union of nerve fibres sufficient to allow the muscular tissue of the transplanted piece to become of service, there would still be a decided gain in having a bladder formed entirely of bladder tissue covered with mucous membrane and lined with bladder epithelium.

D. E. MUNDELL.

A CASE OF COLLES FRACTURE ENDING IN LITIGATION.

MR. EDITOR,—After a delay of somewhat more than two years, I desire to report the following case, not because of any new ideas in the line of treatment, but rather on account of a complication which led to unsatisfactory results, and, further, to show how a surgeon in the faithful performance of his work may be called on to defend himself in a most unjust action.

On the 11th of September, 1896, I was summoned to the home of Mr. A—, whose boy, a strumous lad of about ten years, had fallen from a beech-nut tree. On examination I found a fracture of the lower end of the radius (right arm) and a bruised condition of the Thenar Eminence corresponding to a point marking the junction of the middle with the inner third of the outer head of the Flexor Brevis Pollicis muscle, also some scratches which had been bleeding on the dorsal surface of the hand.

After administering an anæsthetic I washed the hand and arm in a bichloride solution, 1 to 2000, and proceeded to reduce the fracture, the dressing used to retain the fragments in proper position being two lateral splints, well padded, measuring about two and one half inches wide and extending from near the elbow to the Meta-Carpophalangeal Articulation.

A pad consisting of a roll of bandage one inch in diameter wrapped in absorbent cotton was placed in the palm of the hand on which rested the distal end of the anterior splint and an antiseptic pledget was placed over the bruised area.

The splints were held in position (in the absence of adhesive plaster) by two "ties" of bandage placed one at the wrist and the other near to the elbow. A bandage was then applied over the splints and the arm placed in a sling. Directions were given to keep the boy at rest and the hand elevated.

I afterwards saw the boy on September 12th and 17th, and found everything satisfactory, the boy did not complain. He was playing about two days after the accident. I did not see the boy again until October 4th. When I called at his home on this

date I removed the splints and found the dressings and hand in a very dirty condition. So far as the bone was concerned, I found union good and with no deformity. After bathing the hand in tepid water I discovered an indurated patch of skin over the seat of bruise, which was showing signs of separation from the healthy tissue. I considered it the remains of the bruised tissue which nature had not been able to take care of. It was superficial. Before going I dressed the hand antiseptically.

Three days after, on October 7th, I again called, and on examining the hand found signs of separation more marked. Again dressed it as on previous day, giving instructions to have the boy brought to my office the next day to have his hand dressed.

He did not come as directed, and I afterwards concluded that his hand was all right. On Nov. 14 Mrs. A——, who was about to be confined, called to consult me regarding her condition. On leaving my office she mentioned that the sore on the boy's hand had not healed yet. I reminded her of my instructions to bring the boy to my office to have the hand dressed, and told her to have the boy brought to my office at once.

On Nov. 16 Mrs. A—— brought the boy, and on removing a dirty rag, which served as a bandage, I found a deep sloughing sore the size of a twenty-five cent piece. The indurated patch of skin referred to before was still hanging to the surface of the sore by some fibrous shreds.

The wound had become infected with pus-germs, which had burrowed deeply. The thumb was held in an adducted and semi-flexed position in order to relieve any tension on the ulcerating surface.

On inquiring why the boy had not been brought to me as I had directed, Mrs. A—— replied that "she thought that the hand would have healed all right." She said she had tried everything on it, to heal it. Spoke of some salve she had obtained from a neighbour woman, the healing properties of which were unsurpassed and concluded by saying that "she didn't know what kind of a young one he was for if he got the slightest scratch it seemed never to heal."

On this occasion I got the sore cleaned up as well as pos-

sible, applying a proper dressing and instructed the boy to come to my office every day until his hand was all right. He came irregularly and it was about middle of December before the sore had completely healed.

As there was loss of integument and subadjacent tissue there was contraction in the process of healing which was much favored by the position in which the boy held his thumb—hence he had a slight deformity consisting of an adduction of the first Metacarpal bone causing the thumb to be drawn toward the median line to such an extent that it interfered with the complete flexure of the index finger.

Owing to the youth of my patient, and being deterred from doing a plastic operation owing to the unsanitary and septic conditions existing in the boy's home, I determined to try "massage," promising good results if I had the co-operation of the boy and his parents. I encouraged the boy to come to my office every day for treatment, and I explained to his parents how to rub and manipulate the boy's thumb in order to restore its position.

The boy came five times during the month of January. After this I did not see anything of him until the first of June, when he appeared, accompanied by his father, who for six months previous had been threatening to bring an action against me for malpractice and boastfully claiming that he would get \$5,000 if he did so.

On this occasion I charged the father and son with neglect, and pointed out to them where my directions had not been followed. I informed the father that I had not seen the boy for more three months. The only excuse elicited from Mr. A—for his negligence in not executing my orders was that he thought the treatment I was giving the boy was not doing any good. He further stated that he had consulted other doctors, who said that "the hand would have to be split up."

I discouraged any operative procedure at this time, and told Mr. A—that I would not operate without giving "massage" a fair trial; that "massage," properly and persistently applied, would, in my opinion, restore the functions of the thumb.

After again showing him how I wanted the hand treated, I

recommended Mr. A—to so treat it for ten minutes every night, and to advise the boy's mother to do likewise every forenoon. The boy was also to come to my office every afternoon at five o'clock. He came only four or five times when I again lost all trace of him and have not had an opportunity to do anything for him since.

In answer to inquiries as to home treatment the boy told me that his mother had not time to treat his hand, and he was always in bed when his father came in at night, consequently there was no home treatment.

During this latter treatment I procured a plaster-cast of another boy's hand, and after padding it carefully would place my patient's hand in the cast with the thumb in an extended position. The hand was kept in the cast by a bandage. This, I afterwards learned, was taken off as soon as the boy would go home.

On the 15th day of January, 1898, I received a letter from Mr. A's Solicitor asking for damages for alleged mal-practice and threatening unless they heard from me, a writ would be issued within a week's time. The letter also stated that Mr. A. would consider any offer of settlement. There was no offer of settlement, consequently at the expiration of a week the promised writ was issued asking for \$6000 damages.

The case came down for trial at the Spring Assizes in Perth, but owing to the inability of the Plaintiffs to secure medical testimony they asked for a postponement. We thought the case would end here, but during the summer the counsel for the Plaintiff secured a medical witness who was willing to give evidence in his behalf.

Consequently at the fall assizes the case went to trial and after a fight, lasting two days, we succeeded in getting a non-suit with judgment for costs.

The Plaintiffs at once appealed to the Divisional Court asking for a new trial on the grounds that "on the evidence the case should not have been taken from the Jury."

After waiting almost a year the Divisional Court, I am sorry to say, gave judgment against us, ordering a new trial and saddling all the costs on me. Mr. B. B. Osler, my counsel, at once appealed from this Judgment of the Divisional Court to the

Court of Appeals where the case now stands for argument. We expect a decision from this court before the end of the year, as the case is on the list for argument during the present session, and let us hope the decision may be favorable.

NOTES—The contention of the Plaintiff is that the sore on the boy's arm was produced by a splint.

We know the cause as stated. The fall, causing devitalization of tissues, and subsequently infection, and we further declare that owing to the location of the sore, it is impossible to produce such by means of a splint.

The deformity, which is slight, is due to cicatricial contraction in the process of healing rather than to any peculiar art in bandaging, as alleged by the plaintiffs. There would never have been any contraction had there not been neglect on the part of the plaintiff (from Oct. 7 to Nov. 16) allowing the sore to become deeply infected by pus organisms with consequent loss of tissue.

The plaintiff is a worthless fellow, Judgment Summons proof who at the time he began the action was under an order of commitment to jail for debt. No matter, therefore, how successful we may be in defending, we must pay the costs of defence.

Just a word about settlement. During the first days of the proceedings we offered (rather than spend money in litigation) to take the boy and place him in a hospital, public or private, and operate on his hand or otherwise treat it, in order to restore its usefulness, provided we could have absolute control of him while under treatment. They would not accept this unless we gave them a guarantee. We, of course, could not guarantee anything.

I presume at the time \$200 would have settled the case, but I considered that such a course would not only be unjust to the profession, but particularly so to myself.

While it would have been much better for me financially to have settled, yet in doing so I would only be encouraging a class of unprincipled and irresponsible fellows who are after plunder rather than the benefits of our earnest efforts in their behalf.

It is true, and I have experienced the force of it, that when confronted by all the annoyances of protracted litigation, the enormous expenses, which many of us can so ill afford, besides the injury to our professional standing, etc., the first suggestion

which is apt to come is to get the matter settled as quickly and, consequently, with as little publicity as possible. And we ask the question, would it not be better to pay something at once and get out of the difficulty? My answer is No! A thousand times no, so long as we feel that we have done our duty and are therefore morally irresponsible.

It is the duty, and ought to be the desire, of each member of the profession to suppress by every possible means all such actions which are so fraught with injustice, not only to the individual, but also to the profession as a whole. Just so soon as a certain class of the public and those who advocate their claims in court understand that we do not listen to their "bluff,"—that they in order to possibly gain anything must fight every inch of the ground against a united profession—then, and not till then, will the members of our profession be relieved largely, if not entirely, of a most painful annoyance, as well as, to many of us, a severe financial loss.

Sincerely yours,

J. M. CONERTY.

Smith's Falls, December, 1899.

[We extend our sympathy to Dr. Conerty in this very annoying case. We are confident that we voice the sentiments of our fellow practitioners, when we say that too many members of our profession have been thus put to trouble and expense, and have had their reputations thus assailed by irresponsible parties who previously had received the best of care from a surgeon and had at the same time neglected to pay his small fee. It is hard to say how such cases should be dealt with. We agree with Dr. Conerty that in justice to the individual against whom the action is brought, and in the interests of the profession generally, the case must be defended to the utmost. For once let it be known to the public that all anyone has to do in order to force a settlement from a practitioner, is to enter an action for mal-practice and there would be no end of such actions by irresponsible parties whose aim is money, not justice. The defense of such a case then being in the interest of the profession at large, it seems to us that the practitioners of the Province should come to the financial assistance of any confrere who is

thus made the object of attack. How this can best be done is a difficult matter to decide. It is our opinion that a Medical Defense Association should be formed, the members of which should be required to pay annually a sum agreed upon, and that the fund so raised should be used to defend such actions as the one now pending against Dr. Conerty. We do not mean to imply that all actions for malpractice should be thus defended by the Association. We would suggest that when an action is entered against any member of the Association, the case should be referred to a committee for investigation, and that only when the committee reports that in its opinion the action is unjust and that the practitioner has exercised ordinary care and given the patient the benefit of recognized treatment should the Association undertake the cost of the defence. Of course, in Dr. Conerty's case, there is no time for such united action on the part of the profession. We learn, however, that the practitioners of Nos. 15 and 16 Medical Districts are making an effort to assist financially in this case. We commend such action, and express the hope that not one member of the profession will neglect to avail himself of this opportunity of assisting a fellow practitioner in his efforts to defend his reputation, and to save others from similar action. No one can tell whose turn it will be next. We trust Dr. Conerty will meet with a liberal support from his confreres in this district and will come out of this trying ordeal triumphantly.

THE EDITOR.]

A METHOD FOR THE DETECTION AND ESTIMATION OF SUGAR IN THE URINE.

THE method of sugar testing, herein described, was first introduced by the writer to the attention of the profession over four years ago.* Continued use of the test, since that time, has yielded ample verification of the claims then made for its superiority in accuracy and convenience for clinical purposes. It belongs to the class of tests known as the "copper tests," and

*New York Medical Journal. July 27th, 1894.

shares the same principle of reaction as governs Fehling's, Trommer's, Haines and other less well-known modifications, which are included in that class. They all depend for their reaction upon the power which grape-sugar possesses of reducing cupric oxide to a lower form of oxidation, with the formation of a yellowish-red precipitate of suboxide of copper or cuprous oxide. The power of reducing cupric oxide in alkaline solution is possessed by certain urinary products of organic nature, principal among which are uric acid, creatin, creatinin, and glycuronic acid, bodies which are present in both normal and abnormal urine. Although ordinarily not present in sufficiently large amount to give rise to confusion in testing for sugar, they may be obscuring factors when the urine becomes concentrated to any considerable extent through the operation of one or more of the many circumstances which may raise the specific gravity and increase the urinary acidity. If in examining such a urine a sufficient quantity be added to the test solution, these non-saccharine reducing substances become operative, a partial reduction of the test ensues, resulting in a reaction closely resembling in appearance that due to small traces of sugar. It is a frequent occurrence to encounter a urine of greatly increased density and heightened acidity, but containing no sugar. Such urines are always scrutinized with double carefulness with reference to the presence of sugar. It is in such cases where discrimination is essential that Fehling's test, the most popular of the copper tests is most misleading. This is owing to the manner of its application, so large a quantity of urine being used as to introduce such an abundance of organic reducing bodies, when dealing with concentrated urine as to produce a distinct reaction.

Haine's test, although constituting a considerable improvement over Fehling's in manner of application, leaves much to be desired in point of delicacy, and also in stability.

Delicacy with reliability of reaction and stability of reagent are the essential attributes of a perfect test for sugar. These qualities, it is claimed, my test possesses in larger measure than any of the copper tests heretofore devised.

The formulæ for the preparation of the reagents for my test are as follows :

Reagent No. I.—

Sulphate of Copper C.P.....27 grains.

Glycerine C.P.3 drachms.
 Distilled water2½ drachms.
 Liquor Potassa.....to 4 ounces.

Prepare by dissolving the copper sulphate in the glycerine and distilled water. Gentle heat will facilitate solution. To this solution add the liquor potassa. Stir thoroughly with a glass rod and then filter the solution.

Reagent No. II.—

A saturated solution of chemically pure tartaric acid in distilled water.

These reagents, if properly prepared, are quite stable, and with a little care will keep indefinitely.

Mode of Application.—Pour one drachm of the copper test solution (Reagent No. I.) into an ordinary test tube and bring it to the boiling point over a spirit lamp. Then add three drops—not more—of the tartaric acid solution (Reagent No. II) and boil again. A slight deepening in color is the only change observed to follow the addition of the tartaric acid. Now add the suspected urine drop by drop, boiling and shaking the test solution after each addition until reduction takes place, or until eight drops of urine have been added. If no change follows the addition of that amount of urine sugar is not present. If sugar be present a reddish or yellowish precipitate of cuprous oxide forms. The richness of color and density of this deposit varies according to the amount of sugar present in the urine. The reaction produced by slight traces of sugar will be found to deepen and become more positive if the test be allowed to stand for a few minutes and be then re-examined. Applied in this manner the test will detect one part of sugar in one thousand of urine, or one-tenth of one per cent. More than eight drops of urine should not be used, since that amount never fails to give a distinct reaction with half a grain of sugar to the ounce, and smaller traces than that are of no interest to the clinician. Greater delicacy may be attained by the addition of a larger quantity of urine, but by so doing accuracy is sacrificed, for greater sensitiveness and one of the special virtues of the method is destroyed. The danger of false reduction by non-saccharine reducing substances, to which Fehling's test is so liable, is almost entirely obviated by the use of this small amount of urine,

the delicacy of the test rendering this possible without impairing the reaction. Eight drops of urine, however concentrated that fluid may be, can hardly contain enough of these reducing bodies to effect any change in the test. Another feature of this method is the perfect clearness of the reagent during the progress of its application. The employment of tartaric acid endows the test with its peculiar sensitiveness and serves as well to keep in solution the earthy phosphates of the urine as that fluid is added so that the phosphatic cloud, which is so annoying a feature of other copper tests, does not ever appear.

I have adapted this test so that with some variation in procedure it may be used for the quantitative estimation of sugar. The method pursued is in imitation of the ammoniated cupric tests (Pavy-Purdy). The process is as follows—measure off one drachm of the cupric oxide solution (Reagent No. 1), and add to it three drops of tartaric acid solution (Reagent No. 2), and one drachm of liquor ammonia U.S.P. These are thoroughly mixed together and poured into a small glass flask or beaker, and enough distilled water is added to raise the total volume to one ounce. The presence of the ammonia entirely changes the reaction which characterized the employment of the test for qualitative purposes as already described. The cuprous oxide formed by the reducing power of the sugar is held in solution by the ammonia, and does not at any time appear as a reddish precipitate. The blue protoxide of copper which gives the color to the test solution is, however, still reduced by the sugar of the urine, the resulting suboxide being dissolved by the ammonia as quickly as it is formed, the solution remaining clear, the complete disappearance of the blue color marking the completion of the test. The test carefully measured and mixed as indicated possesses the fixed sugar value of one tenth of a grain of grape sugar—that is, it is reduced and decolorized by exactly one-tenth grain of sugar. The flask containing the reagents is placed over a spirit or bunsen flame, and when it is smartly boiling the urine is added slowly, a single drop at a time from a burette or graduated minim pipette. Care must be exercised not to add the urine too fast, an interval of four or five seconds being allowed to elapse between the first few drops, and when the blue color begins perceptibly to fade, the addition should proceed still more slowly until an interval of

eight or ten seconds separates the last drops. The exact point of complete reduction is reached when the blue color has entirely disappeared, leaving the solution clear and transparent, with a slight opalescent tint. The number of minims of urine necessary to accomplish this result is then carefully noted on the graduated stem of the burette or pipette. Since the test as prepared, has a fixed equivalent of one-tenth grain of sugar, it necessarily follows, providing the conduct of the test has been correct, that the number of minims of urine required to completely discharge the blue color must contain exactly that amount of sugar. It is now only necessary in order to complete the determination to divide four hundred and eighty, (the number of minims in an ounce) by the number of minims found necessary to complete the reaction, and again divide the quotient by ten.

Example:—Eight minims of urine are found necessary to fully discharge the blue color from the test. The equation then becomes—

$$\begin{array}{r} 8 \mid 480 \\ \hline 10 \mid 60 \\ \hline \end{array}$$

6 grains of sugar per ounce
of urine.

It is advised that all urines having a specific gravity exceeding 1.028, which by their conduct with the qualitative test give evidence of containing a large amount of sugar, should be diluted with an equal volume of distilled water before submitting them to estimation and the result be multiplied by two. This precaution renders the progress of the test more capable of delicate and exact supervision.

In place of a flask or beaker an ordinary test tube of large lumen may be used as the containing vessel in which to boil the reagent, which under such circumstances should not be diluted by the addition of water. Some slight loss of accuracy necessarily attends the crudity of this method, although the results are even sufficiently accurate for clinical purposes. No preparation of the urine is needed previous to submitting it to estimation, and no precautions are necessary in applying the test except care in the addition of the urine lest it be added too rapidly. On the other hand, the procedure must not be conducted too slowly, for

fear that volatilization of the ammonia will proceed more rapidly than reduction and thus allow precipitation of some suboxide. Such an accident is rare and will necessitate reapplication of the test.

The sensitiveness of the reagents results in a singularly clear and transparent end reaction. The fact that the same test solutions are used for estimation as are employed for the detection of sugar constitutes a great convenience and avoids multiplicity of reagents.

A. R. ELLIOTT.

Chicago.

CANCER.

[Read before the Kingston Medical and Surgical Society.]

MR. PRESIDENT AND GENTLEMEN—: It would be impossible in the time at my disposal to read a paper dealing with the whole subject of Cancer. I shall therefore confine my remarks to those features connected with the disease that have been receiving most attention from the profession during the last few years. I can scarcely call these features *new* for like many more subjects of medical interest, they have been discussed in the long past, and in many instances it is simply an example of history repeating itself.

There are so many disputed points in the causation, history, pathology and even treatment of cancer that it would be useless for me in a paper of this length to attempt to pass them all in review. And then as Malcolm Morris says "there is a gruesome fascination about cancer just as there is about a ghost story that is apt to make us all dwell upon its ravages even though we may dread its onset in ourselves." My paper must necessarily be fragmentary and incomplete, and from the very nature of the questions involved, it must be marked by indecision. Let me hope, Mr. President, that these features which appear to be inseparable from it may be the means of eliciting a full and free discussion. Is cancer increasing? is a question that is now receiving a great deal of attention from both the Medical Profession

and the laity. If we depend upon statistics or upon that equally unreliable source, public opinion, we cannot help arriving at the conclusion that cancer is increasing, and that very rapidly. Many authorities do not even question its increase but accept it as proven and base their observations and conclusions upon this ground. Thus, Dr. Roswell Park, of Buffalo, Director of the State Laboratory for the study of Cancer, tells us that cancer is the only disease that is increasing, and makes what he himself calls a startling prophecy: "If for the next ten years the relative death rates are maintained we shall find that ten years from now, viz. in 1909 there will be more deaths from cancer in New York State than from consumption, smallpox, and typhoid fever combined."

If we could place absolute reliance upon statistics, Dr. Park might be justified in making this prophecy. I will not take up your time showing the many fallacies that exist in statistics dealing with a subject like cancer, but will make these examples suffice. For the last few years the Registrars of Great Britain have adopted the plan of sending letters of inquiry to those Doctors whose returns are incomplete or unsatisfactory. In one year we find that these inquiries resulted in the transference of 597 deaths from vague headings such as "Tumors" or "Disease of Ovaries," "Liver," "Brain," "Uterus," etc., to "Cancer." Out of 111 cases returned as "Stricture of the Oesophagus," 74 were found, on inquiry, to be due to cancer. Such results as these make it clear that in former years when the death rate under "indefinite causes" was three times greater than at the present time, accurate certification would have greatly swelled the mortality returns from cancer. We must remember that the "Registrar General's figures represent certified causes of death and not always actual facts" and that while American and foreign writers are quoting British statistics as being reliable on account of the greater attention paid to Registration in the British Empire, and are basing their conclusions upon these statistics. British writers are at the same time showing us the unreliability of these same statistics. That the number of deaths reported as due to cancer has increased within the last few years is certain, but I think there is not sufficient evidence to show that the increase is due to any cause but improved certification. Some have gone even farther than saying that cancer is increas-

ing and have pointed out to us that certain cancers are increasing, others remaining stationary, and others decreasing, although the aggregate has increased. Various causes are given for the increase of deaths reported as due to cancer and also for the alleged increase of the disease itself. I have mentioned improved certification as the one upon which I place most reliance. I may mention improvement in Diagnosis so that many cases are now returned as cancer that would not have been diagnosticated as such a few years ago. This is a two-edged weapon and there may have been many cases diagnosticated as cancer in the past, that would now be classed among the non-malignant diseases. There is some evidence to show that the length of human life has increased and as cancer especially Scirrhus and Epithelioma is a disease of middle and late middle life, this would naturally account for some increase. If there are more people alive at 60 than were one hundred years ago, there are more people with the cancer liability or as we might say in the cancer age. Closely related to the causes of the increase in the number of reported deaths from cancer are the causes that are said to have led to the increase of the disease itself. Among the laity in this and other countries the increased consumption of the tomato as an article of diet is looked upon as a cause and it is really surprising how widespread this belief is. In France the increase is said to be due to the increase in the consumption of meat as an article of diet, and in Mahomedan and Hindu countries the prevalence of cancer among the British is attributed to their beef-eating propensities. The increased consumption of alcohol is held by many as a very potent cause. Increased prosperity by which the working classes are enabled to live better is also blamed for a great deal of the increase, for although experiments in inoculation were much more successful in the weak, debilitated animals, we are asked to believe that the prosperous, well-fed human animal is much more prone to cancer than his less fortunate neighbour. The increased mental worry of the 19th century has also been made to do duty as a cause of the increase of cancer, but that 70 Brewers die of cancer to 43 Doctors is scarcely compatible with this theory. The contrast between Lawyers and Clergymen is also very marked. I will only briefly refer to these reputed causes; noticing that the very

great discrepancy existing is pretty strong evidence for believing that none of them can be considered trustworthy. A great change has taken place within the last few years in the views concerning the "Causation of Cancer." Formerly the battle was between Local and Constitutional origin, now both have been set aside and the Microbe has taken their place. Heredity was also supposed to exercise a very great influence upon the development of cancer but now since the belief in Bacteriological Pathology has become almost universal, hereditary influence is accorded a very secondary place indeed. Formerly when the hereditary nature of cancer was considered well established, among the first questions asked a patient suffering from a suspicious growth was one bearing on the occurrence of cancer in near relatives or ancestors. Now there is very little value attached to this question, and since the growth of the parasitic theory of cancer, hereditary influence has been given a very insignificant place in the causation, and all that is now claimed for heredity by those who look upon cancer as a parasitic disease is that the soil favorable for the growth of the parasite has been inherited. The parasitic theory does not prohibit belief in cancer being a local disease and by attaching less importance to heredity and constitutional causes, has had a beneficial effect upon the means used for the removal of the disease. Cancer in this respect has followed Tuberculosis. There are two causes that have played an important part in the old and new theory of causation; I mean irritation and injury. Formerly it was held by many that cancer was a purely local disease and resulted from chronic irritation or injury previously received. On the other hand, some maintained that cancer was always constitutional and that the only part irritation or injury played in the production was one by which its locality was determined. Now that Bacteria are thought to play the principal part in its production, irritation and injury are supposed to determine its locality as with Tubercle. It is difficult to obtain evidence to show that either one plays even the very secondary part that is claimed for it by believers in the Bacterial origin of cancer. Cancer is very often found in the Mammary gland of the human female, both irritation and injury being looked upon as determining causes; the udder of the cow is almost immune, yet we all know that the

latter suffers from both injury and irritation much more often than the former. Mastitis is exceedingly prevalent in the cow, and imperfect recovery the rule. The Mammary gland of the sheep is subjected to very violent usage and yet cancer is almost, if not, entirely unknown. I think there are no cases of cancer reported as having occurred in the pig, and yet perhaps the Mammary glands of the sow suffer more from both irritation and injury than those of any other domestic animal. Frequent conception (I think sometimes three times in twelve months) will account for the irritation, while the pendulous belly of the brood sow will expose the Mammary glands to many sources of injury. The parts of the body of the horse, mule or ox, pressed upon by the harness or yoke or saddle are not prone to cancer, in fact, are practically free from it. Certainly the lips of the horse, cow, sheep or any of the lower animals are subjected to much more irritation and injury than those of man, yet cancer is more prevalent in the latter than in the former. Cancer is very rare in the male Urethra. I once heard Mr. Reginald Harrison say that he had seen only three or four cases (his opportunities are almost unlimited at St. Peter's Hospital), yet we all know that the majority of the Urethræ a man sees at St. Peter's have been subjected to diseases causing irritation, and treatment often causing injury. Irritation is said to cause cancer in the Scrotum of the chimney sweep, but it is difficult to understand why sweeps who work in coal dust above ground should have a cancer mortality of 156 per 1000, while coal-miners who work in coal dust under ground have a cancer mortality of only 36 per 1000.

I have dwelt at length upon these two reputed causes of cancer, irritation and injury, for no matter whether we accept the old or local theory of causation or the new bacterial, they have been made to do duty as important factors, both in causation and determination of location of cancer. I have not attempted to prove the negative, so much as to show that the evidence upon which we accept many things relating to cancer is very unsatisfactory indeed. I think there are many of the commonest questions, those that confront us in almost every case of cancer that cannot be satisfactorily answered by either the local, the constitutional, the parasitic or Cohnheim's theory of causa-

tion. Simple questions I may call them such as the frequent occurrence of cancer in the female breast while it is almost unknown in the male breast, its occurrence in the stomach of the male much more frequently than in that of the female, its frequent occurrence in the lower lip and its rarity in the upper, while it is perhaps more common in the upper than in the lower jaw.

Personally, I have had great difficulty trying to relinquish my belief in the hereditary nature of cancer, and I sometimes think that the present almost universal belief in the microbe to the almost entire exclusion of heredity is only the return swing of the pendulum; exaggerated perhaps because it had been forced too far the other way. Just at the time when Scientists and the old maids had perfected the hereditary theory, and had demonstrated that it was responsible for everything from Phimosi's to Ingersoll's infidelity, the ubiquitous microbe appeared on the scene and destroyed the whole structure. We are naturally led to ask—Is cancer a parasitic disease? Perhaps the most important question connected with the disease. If it is parasitic, we can easily account for its at least apparent contagiousness, for its occurrence in many members of the same family, for so called cancer houses, for its apparent geographical distribution and may be able to devise some means for its destruction. A great deal of investigation has been carried on to ascertain the nature of cancer, and, if possible, discover the organism if such existed. Mr. Plimmer of St. Mary's Hospital, London, has examined nearly 1,300 cases of cancer during the last six years, and claims to have found what he called a parasite in nearly 1,200. A great deal of discussion has taken place over this structure which was present in such a large proportion of Plimmer's cases, some maintained that it was an actual parasite, the nature of which was not known, others maintained that the bodies discovered by Plimmer were simply due to degeneration of the cells. Plimmer is certain that the bodies found by him are neither parts of the cell structure nor any known change, and are only found in cancer and only at the periphery of the growing parts of a cancer, and not in the degenerated parts, and that they have distinctive micro-chemical reactions. He also says that they can be cultivated outside the body; that these cultures when introduced into suitable animals will cause death with production of

tumors, and that cultures can be made from these tumors which if inoculated into suitable animals will produce again similar growths. Many others are working in the same field, (especially some Italian Physicians) devoting much labor to the task, but as yet the weight of scientific evidence seems to be against the bacterial origin of cancer. One reason for the difficulty of experimentation is that the lower animals are not nearly so prone to cancer infection as they are to some others such as tuberculosis. The fact that the microbe has not yet been discovered, (unless we admit that Plummer's organisms are the true cancer microbe) is not evidence that it is not present. There has been none discovered yet in syphilis or small-pox, two of our most contagious diseases, and some even doubt that the true one has been discovered in gonorrhoea. Roswell Park says that "probably the question can not be settled until we get better lenses, more media, other stains and improved chemical methods." The question of contagion and infectivity of cancer is naturally closely connected with its pathology and etiology. Is cancer contagious? is an old question that has been receiving renewed attention since the germ nature of the disease has been suspected. Nearly 200 years ago cancer was declared to be infectious and as proof a case was cited where a woman with cancer of the breast slept with her three sons, two of whom died of cancer within a short time, and the other became cancerous but was saved by operation. It is, I think, over 300 years since Tulpius announced that "an ulcerated cancer was as contagious as an inflamed eye." In France cancer was looked upon as contagious, and 150 years ago patients were refused admission to the Hospitals; were treated in pest houses outside the town, and it is only about 50 years since they were received into a General Hospital, and then they were placed in a separate block. The records of Cancer Hospitals do not uphold the theory of the infectivity of cancer. There are many instances of the accidental inoculation in the same individual, and the majority of surgeons are careful to guard against the inoculation of the wound made in removing a cancer. Few surgeons would use the same knife for cleaning the Axilla and making primary incisions. It is said that a surgeon after removal of a cancerous breast deliberately placed a piece of it under the skin of the breast on the opposite side; the

wound healed, but a cancerous nodule formed in two months. A watery emulsion of cancer has been made, filtered and mixed with the defibrinated blood of a dog and then injected into the veins of another dog; within two months several cancerous tumors developed. Cancer has followed in the track of a Trocar used in tapping a case of Ascites due to cancer. There is no doubt cancer can be transmitted from one animal to another of the same species, but it is not so certain that it can be passed from man to the lower animals or vice versa. Cases are reported, one where it was carried from a man's lip to a dog's, another from a dog to a man's hand.

The comparative infrequency of cancer in the lower animals and the difficulty of its inoculation, (some of them seeming almost immune) interfere with experimentation. The number of cases of cancer in the lower animals is greater than was formerly supposed. Cancer has been transmitted through a series of dogs, and it was observed that in some cases the tumors entirely disappeared and in these, further experimentation showed the dogs to be immune. This might open the question of inoculation for the purpose of securing immunity. During the last few years the question of cancer houses has received a great deal of attention. My attention was drawn to it some years ago by the laity. At present it is believed by many, both lay and professional, that cancer lurks in certain houses and in certain localities. There is a good deal of evidence in favor of this view and also to show that cancer is more prevalent along the banks of rivers, especially rivers with clay banks, and liable to overflow surrounding marshes. Here the disease appears to resemble Malaria, and some have gone so far as to say that there is a host for the organism as has been now demonstrated in Malaria. If these speculations can be proven to be true, the theory of a cancer germ, of the infectivity of cancer, and the hope that some means can be taken for the destruction of the organism must in time become fully established. When we consider the results that have been secured since steps were first taken for the control and destruction of Tuberculosis, we are prepared to believe that some thing may also be done to stay the ravages of cancer. Already the germ theory of cancer, although not proven has had a beneficial effect on the results that follow operative procedures.

The changes and improvements that have taken place in the treatment of Cancer during the last few years are legion. There are a few general lines along which these advances have taken place. The days of dallying with a cancer or suspicious growth have passed, and early operation is now insisted upon. If we fully realize that "a cancer that is thoroughly removed will never recur," we will have the keynote to a great deal of the improvement in the operative treatment of cancer. This will imply not only early but free removal not only of the growth but of the structures that we know are in the line of infection. The treatment of internal cancers can scarcely be compared with that in existence a few years ago; it is practically a new growth. The operations for cancer of Pylorus, Gall Bladder, Uterus, Intestines, &c., unknown a few years ago, are now being done by all surgeons and with good results. The demand for early operation is even more imperative here than in external cancers. It is not my intention to discuss the details of any of these operations. Many changes have taken place since these operations were first performed, and in many instances we might almost say they were still in the experimental stage. The statistics have steadily improved. I will refer to cancer of the body of the uterus as an instance of the changes in surgical practice. Cancer of the body of the uterus, except as an extension of the disease from cancer of the cervix, had always been looked upon as a very rare disease. When it became known that surgeons could remove the uterus by the vaginal route without injuring the ureters too often, the cases of cancer of the body of the uterus became very common, and Vaginal Hysterectomy very popular, and the immediate results simply marvellous. At a recent meeting of the British Medical Association Dr. J. More Madden, of Dublin, read a paper in which he stated that out of all the cases of cancer of the body of the uterus in which he had done the vaginal operation not one recovered, and recurrence had taken place in every case. He spoke highly of the abdominal operation for this class of cases. There were always two puzzling features connected with cancer of the body of the uterus and its removal by the vaginal route. One was the very great increase in the number of cases, and the other was the remarkably good results that followed the removal of the organ while the surgeon worked com-

paratively in the dark. That many of the cases operated on were chronic metritis is certain, and Dr. Madden's statistics have shown us that the results in the actual cancerous cases were the opposite of favourable. Abdominal Hysterectomy done early, with careful search for and removal of all suspicious tissue, is said to be giving good results. Speaking of operations for external cancers I may say the field has been enlarged in every sense of the word. Many cases are now operated upon which would have been classed as inoperable a few years ago. Cancer occurring in both breasts was looked upon as inoperable, yet one of the best results I ever saw was in a case where both breasts were removed. I would classify the case as a *cure* had not the poor woman died of cancer of the throat 14 years after removal of the breasts. Extensive ulceration occurring in a cancerous growth as of the mamma was looked upon as a contra-indication to operation, but many of these cases are now subjected to operation with good results. After earnest solicitation on the part of a patient and her friends I removed an extensively ulcerated cancerous breast in 1889. After the operation there was little left on the front and side of thorax except the ribs. There has been no return, and the woman is now in good health, over ten years after the operation.

I suppose we are all prone to dwell upon our successes—the ease with which a man can recall his successful results after operations for cancer is the saddest feature about them. Cancer of the breast is a very common disease, and its operative treatment has received a great deal of attention. New operations with new names (of course) have been devised and new statistics published, but the same principles must guide us here as in dealing with cancer elsewhere. We must remember that there are several routes by which the infection travels; the most common being along the Lymphatics lying along the Pectoral muscles, next the Lymphatics leading to the supra-clavicular glands, next through the Lymphatics of the opposite breast forming deposits in that gland or even in the glands of the axilla without involving the mammary gland, and, lastly, through the deep Lymphatics into the Post-sternal glands. Bearing these routes in mind will enable us to seek for invaded glands, and acting on the principle that if a “cancer is thoroughly re-

moved it will never return," we ought to be able to get better results than heretofore. The principal change that has taken place in the operation for removal of a cancerous breast has been in the direction of thoroughness and increased tissue sacrifice. A few years ago the operation was a very simple one indeed. An elliptical incision was made, the gland removed, sutures introduced, and if proper precautions had been taken, the wound would be healed in a few days. The operation could scarcely be called "critical," even by the Kingston daily newspapers. The immediate results were generally good, but I think there is no doubt recurrences took place much more frequently than at present when the more extensive operation is done. I suppose there is no doubt there is a time in the history of every case of cancer when there has been no invasion of the tissues, when a man would be justified in removing the growth and leaving the immediately surrounding tissues and Lymphatics, but it is impossible to tell when infection takes place. Some place the time at six months, but it is impossible to tell when the six months have expired, as the growth may have been in existence before the patient's attention was called to it. Had I read this paper a month ago I would have said that no man was justified in removing a cancerous breast without removing the axillary glands, fat and Pectoral fascia at least, but within the last month I have read that at a meeting of one of the London societies many of the older men were still in favour of the old operation. The statistics of the new operation are said to be much better than those of the old, but it is a singular fact that a Scottish surgeon back in the 18th century published apparently reliable statistics as good as any of the present day. In marked contrast is the experience of the late Dr. D. H. Agnew, of Philadelphia, whose opinion, based on years of surgical practice, was that operations for removal of cancerous mammary glands were perfectly useless. Agnew said that he did the operation to satisfy his patient, never even hoping for benefit.

There is a difference of opinion amongst the advocates of the new operation as to how much tissue should be removed. Some remove the surrounding fat, axillary glands, Pectoral fascia and muscles and supra clavicular glands in all cases. Many are not so thorough, and although they may remove the axillary

contents in all cases do not remove the supra-clavicular glands unless there is some evidence of their being involved, and do not remove the Pectoral fascia and muscles unless the cancer is adherent to or has infiltrated them.

It is hard to lay down rules, but seeing that infection through the axilla is so frequent and often occurs very early in the disease, I think it would be better if the axilla was always cleaned of all fat and glands. That we ought at the same time remove the Pectoral fascia and Lymphatics is evident; it would be useless to remove the tissues in the axilla and allow the route of infection to remain. That we should always remove the supra-clavicular glands, the pectoral muscles, etc., is a question not yet settled. It is a comparatively serious operation, and if the disease has not been long in existence and has not grown rapidly, most of us will be content to allow the supra-clavicular glands and pectoral muscles to remain, hoping that the result may be satisfactory and knowing that we can remove the structures in the future should they show signs of becoming involved. Unfortunately there is in all cases a possibility that infection has taken place through the most infrequent route, the Post-sternal glands, and recurrence may take place no matter how extensive our operation has been. There is another class of cancers—the saddest of them all for both surgeon and patient, the inoperable cases, where surgery is unable to grapple with or remove the disease. During the last few years investigation and experiment have been busy trying to find some means by which the unfortunate victims of this class of cancers could be benefitted. I will mention only a few of the many methods that have been employed. Removal of the ovaries has been done by many surgeons, and we would naturally expect that it would have an inhibiting effect on any growth, malignant or otherwise, in the breast. Unfortunately the effect is not lasting. Thyroid extract has been given a good trial, both alone and along with removal of the ovaries; given alone the effect was *nil*, but it seemed to do some good when given after the ovaries had been removed. Beatson, who introduced this plan of treatment, concludes that “we must look in the female to the ovaries as the seat of the exciting cause of cancer, certainly of the mamma, in all probability of the female genital organs in general, and pos-

sibly of the rest of the body." The NUMBER of his cases or the results does not warrant such a conclusion. Cases of inoperable cancer occurring in other organs have been known to cease growing and some (Sarcoma) have disappeared after the ligation of the supplying arteries. On the other hand, Sir Spencer Wells and Dr. Alexander Simpson, and many others, believed that removal of the ovaries tends to increase the liability to cancer. Electricity is said to have given good results in some cases. Injection of alcohol has some very enthusiastic supporters. The diffusion of the oxychloride of mercury has some advocates. Carbide of Calcium has been used in Chicago (Ethridge). Lymph gland extract by Dr. Herbert Snow, of the London Cancer Hospital, and good results have been reported by both. Perhaps the most important treatment of inoperable cases is that by the mixed Toxins of Erysipelas and Bacillus Prodigiosus, introduced and carried on for some years by Dr. Coley, of New York. There was good ground for trying these agents in cancer, for cancerous tumors had apparently been checked by the occurrence of Erysipelas. Dr. Coley concludes that the mixed Toxins have an inhibiting action upon the growth of malignant tumors of whatever variety, but that it is more marked in Sarcoma and differs even in its effects in the different kinds of Sarcoma. He claims that their use rests on a perfectly logical and scientific basis. I cannot go into this matter as fully as it deserves. There is one feature about cancer that we must not forget when reading the praises of some new remedy and the glowing reports of the so-called *cures* attributed to its use. Cancers are often quiescent for years, are some times atrophied, and there is no doubt some (especially Sarcomata) have entirely disappeared. I know a case at present (Atrophic Scirrhous of breast) which has been "cured" at least four times during the last ten years, each "cure" the result of a new remedy in the hands of a fresh quack, the last being "the laying on of a dead man's hand." Twenty years ago Cundurango and Chian turpentine had many cures to their credit, now they are never heard of. Let us hope that all the new remedies may not share a like fate.

E. KIDD.

THE PRESENT POSITION OF ANTISTREPTOCOCCUS SERUM IN THE TREATMENT OF PUERPERAL INFECTION.

[Read before the Kingston Medical and Surgical Society.]

BY puerperal infection we understand all of the diseased conditions occurring in puerperal women caused by such organisms as gain access to the system by wound infection. This infection may be slight, as a local affection of the external genital organs, or severe, as when the internal genital organs are affected; or it may extend to the whole system, producing what has been, and is yet, by many, called "puerperal fever." Infection may be local and "putrid," and known as sapræmia, or general, "septic" and known as septicæmia. The former is produced by the organisms, known as saprophytes, which develop in dead organic material, and by their growth and development produce a toxin which may give rise to fever. Septicæmia is due to the introduction of pathogenic organisms which develop in the blood and other fluids of the living body as well as in dead tissue.

¹Recent investigations have proved conclusively that puerperal infection is not dependent upon one specific micro-organism, but may be produced by various types of bacteria. The bacterium most commonly met with, and responsible for the most serious cases, is the *streptococcus pyogenes*. Streptococci may inhabit the vagina without giving rise to any symptoms, but under certain conditions cease to be harmless and cause the most alarming symptoms. Streptococci from septic wounds are the most virulent, and have the greatest capacity for infection. The entrance gates of infection are abrasions of the mucous surfaces, and the cervix and uterine cavity, especially the placental area, are most apt to spread the infection.

Fatal infection may follow the invasion of *staphylococci*. These bacteria are often found in the normal vaginal secretion as harmless parasites, but they also may change their habits and take on a most dangerous character. In the medical literature of the day will be found reports of groups of cases of pyæmia and sepsis in which the blood and metastatic abscesses were subjected to careful bacteriological examination.

In one group of four cases (Hahn) of pronounced pyæmia streptococci were found in three, and in the fourth case staphylococci. In another group of seven cases (Brieger) of typical sepsis almost pure cultures of staphylococci and streptococci were found. In another group of ten cases (Mironow) streptococci were found seven times, in two both staphylococci and streptococci, and in the tenth streptococcus and the bacillus fetidus.

The *bacterium coli*, too, has been found in well marked cases of puerperal infection, alone and with other bacteria. In a group of six cases (Erbhart) the colon Bacillus was found alone four times, and twice together with streptococci.

Gonococci are frequent inhabitants of the genital tract. They may remain latent for a long time, but under favorable circumstances may be aroused to new activity. Gonorrhœal infection is usually circumscribed and limited to the immediate neighborhood of the uterus, but when associated with other microorganisms, as they not unfrequently are, are apt to produce a general sepsis. That puerperal infection from gonococci is not rare is also shown from numerous reports (Kronig), and doubtless explains many cases occurring in spite of every precaution.

Pneumococci and *diphtheriae bacilli* have in a few cases (Canon, Bumm) been found as the cause of puerperal sepsis.

The *bacterium* of *putrefaction* is a frequent inhabitant of the vagina, but as a rule causes but slight disturbances, producing the so-called one-day fever. Authors (Kronig, Bumm), however, report fatal cases in which only this bacillus was present. In a group of 296 reported cases of puerperal infection the bacillus fetidus was discovered 32 times, and in a group of 166 cases, 58 times.

A few reports (Vinay) record *typhoid* and *tetanus bacilli* producing puerperal infection.

Mode of Infection.—In the older works on obstetrics “puerperal fever” is described as a specific fever, and the cause is stated as often existing within the patient herself; or that it might be contracted after the manner of typhoid or typhus, or other specific fevers; and further that the lying-in women of whole neighborhoods might become affected, that some neighborhoods were more likely to be infected than others, and that

during certain seasons of the year it might be especially virulent. Recent investigations have made the pendulum swing to the very opposite point, and now it is a belief that cannot be denied that the cause comes from without, in the form of septic organisms introduced, in the vast majority of cases, by the accoucheur, his assistants, or from the surroundings, or by all three. Other investigations have shown that nearly all types of bacteria capable of producing puerperal infection inhabit the vulvar opening, and may inhabit the vagina, especially the lower part, but it has also been shown that the healthy human organism, whether acting through the vagina bacillus (Doderlein), a bacillus claimed by some to exist and to have germicidal action by means of its acid secretion, or whether acting through its own inherent powers, is well capable of combating these bacteria, provided the normal functions are not disturbed, or the vitality of the tissues lowered by useless manipulations and interference, or by the introduction of new crops of virulent bacteria at the time of labor (Williams, *Bacteria of the Vagina*).

Various forms of infection.—For a proper understanding of the complicated and often perplexing literature on serum therapy a clear idea of the various forms of infection is necessary, and, accordingly, it will serve to better advantage if the opinions of two or more writers on the subject be given, rather than to try to summarize their views.

(¹Rosenberg.) The symptoms of puerperal infection and the pathological conditions vary largely according to the causes of infection and its extent. Accordingly, text-books usually divide puerperal infection into different classes, but it must be said that clean cut types of the disease are rarely met with, as they overlap each other, so that the symptoms of infection must vary, or differ in almost every case. If a piece of placenta remains in the uterus, there undergoing putrid changes, the symptoms found will be due entirely to the absorption of toxins, and will disappear very rapidly on removal of the cause. Again, puerperal infection may remain localized in the lower part of the genital tract—cervix, vagina or vulva—in the form of ulcerations or abscesses; and although strictly a puerperal infection, and may become general, the symptoms are of a local character. The first variety may, after appropriate treatment, steadily and rapidly grow

worse, as indicated by high pulse and other symptoms, in which case it may be taken for certain that, in addition to putrid intoxication, pathogenic bacteria have entered the circulation, and the picture of the disease will reflect disturbances produced by the irritation of the bacteria in the organism, and the absorption of toxins. Diffuse septic peritonitis is usually a consequence of septic endometritis, and rarely the result of infection originating in the cervix or vagina. Here, to one who has seen a case, the symptoms are well known—the marked cardiac depression from absorbed toxins, the pulse ratio high and in disproportion to the existing temperature, while the patient feeling comfortable and contented, and perfectly conscious, takes leave of her friends. The rarest form of puerperal infection is described under the form of pyæmia, or metrophlebitis, in which the bacteria enter the venous circulation from the endometrium, or from ulcers of the cervix or vagina. This form appears late in the puerperium. The thrombi which normally form in the uterine veins immediately after parturition become infected and a phlebitis occurs. The patient may be quite well for a few days, or what is more common, during the first week presents symptoms of a slight endometritis, with perhaps a slight rise of temperature, but with a disproportionately rapid pulse, while the patient's general condition is not good. All at once she is seized with severe chills, followed by decided rise of temperature, and soon there are evidences of metastatic abscesses elsewhere. These chills repeat themselves in shorter or longer intervals, and it is not long before most of the vital organs have become the seat of more or less intense infection.

(³Mundé.) Three forms of puerperal sepsis are recognized. *First*, sapræmia, or the variety in which the septic forces remain localized, and the microbe or germ infection does not enter the circulation. This form produces its systemic results, not by the passage of its germs into the general system, but through local irritation, causes a general elevation of temperature and pulse. *Second*, septicæmia, in which the septic germs find their way into the general system and, by invading the blood, produce general systemic infection. In sapræmia the products of decomposition are usually putrid and their odor exceedingly characteristic and offensive; in septicæmia there ordinarily is no distinctive odor,

and not necessarily any pathognomonic discharge from the genital organs. *Third*, pyæmia, or the variety of septicæmia in which deposits of micro-organisms take place in distant portions of the body, and there produce decomposition and abscesses.

(Spencer.) Severe puerperal sepsis is met with under three forms, *acute septicæmia*, which terminates fatally within a few days, owing to the virulence of the infection; *lymphatic septicæmia*, the usual form, characterized by a rigör at the outset, and by septic peritonitis; and *venous septicæmia*, characterized by a succession of rigors, and by septic phlebitis, and frequently by secondary deposits. A sloughy condition of the placental site is generally, and abscesses in the uterine wall are sometimes met with in both the latter forms. Milder cases of puerperal sepsis are met with, as *sapræmia* or poisoning by the chemical products of micro-organisms, and is often in practice hard to distinguish from true septicæmia.

(Marx.) In determining the course of treatment and prognosis in puerperal infection a proper classification of such disturbances is absolutely essential. Two varieties may be recognized; (1) local, (2) general. Under the first head comes *sapræmia* and *septico-sapræmia*. Under the second head are (a) the *lymphatic*, (b) *pyæmic* or *phlebitic*, (c) *septico-pyæmia*, (d) *acute virulent septicæmia*. These different varieties are seldom distinct entities, being often combined, and merging the one into the other. The diagnosis of a local septic condition, the so-called *sapræmia*, is easily made. There should be made a direct visual inspection of the perineum and the genital tract, the former for sloughing masses, the latter for patches of torn or unhealthy grayish and malodorous tissues with painful swelling of the parts. A speculum examination will always confirm or exclude pathological states of the cervix, and a careful digital examination will reveal the condition of the uterine cavity. In true *sapræmia*, or decomposition sepsis, there is a stinking discharge, resultant and dependant upon retained and decomposed secundines. *Septico-sapræmia* can be diagnosed when there has been evidence of a *sapræmia*, in which, though relieved by direct physical examination and local treatment, a low fever persists. It will, however, first be necessary to exclude by digital examination bilateral disease of the appendages, a cellulitis, and a true pelvic peritonitis. It is in the cases

of general sepsis, however, that the greatest difficulty is experienced in making a diagnosis. In some it can only be done by careful exclusion; our main guidance must ever be the rapid pulse and the comparatively low temperature, both of which are out of proportion to each other, together with the general physical condition. Often there is absolutely no evidence of a local nidus for the source of the sepsis. The uterus will be found small and fairly well contracted, and the examining finger will find its interior smooth. The lochial discharge, often much diminished in quantity, is usually normal as to odor. The diagnosis of true acute puerperal septicæmia may be further certified by the gradual diminution in the function of the kidneys, the profuse sweats, and the increasing rapidity of the pulse. A positive diagnosis can only be made by finding the streptococci in the blood.

In the phlebotic or pyæmic form one notices the enlarged and painful uterus, the desperate condition of the patient, the irregular fluctuations of temperature, and the very great rapidity of the pulse, together with evidence of metastatic deposits elsewhere.

Serum therapy. Ever since Pasteur discovered his vibrio in 1877, much interest has been shown in the bacteriology of puerperal infection. Dr. Watt Black (*Transactions of Obstetrical Society*, 1891), in a paper on puerperal fever, was the first to suggest its treatment by antitoxin, but the history of antistreptococcus serum practically began in 1895 when Marmorek presented a communication to the Society of Biology, Paris, on the preparation of the serum. In July of the same year he published an article in which he reported the results obtained by its use in the treatment of 413 cases of erysipelas with a mortality of 3.4 per cent. He also stated that he had treated 16 cases of puerperal fever by the same method. In 7 cases he had to deal with a pure streptococcic infection, and all of them recovered. In three other cases the streptococcus was combined with the colon bacillus, and all of them died. In the other cases the staphylococcus was associated with the streptococcus, and of these two died. From his experiences he concluded that his serum was a most efficacious means of treatment in cases of pure streptococcus puerperal infection, but that it was useless if other organisms were combined

with it, as in such cases the serum could only neutralize the effects of the streptococcus, but could not be expected to have any effect on the organisms associated with it. In April, 1896, Charpentier reported to the Obstetrical Society of Paris 40 cases of puerperal infection in which antistreptococcus serum had been employed, of which 24 died. After eliminating from the fatal cases 5 which were moribund when seen, and 11 which were not examined bacteriologically, there were left 24 cases, and on analyzing these he found that streptococci were demonstrated in pure culture in 16 cases, of which 7 died; that the streptococcus was associated with either the staphylococcus or colon bacillus, or both, in 8 cases, of which 4 died, thus making a mortality of 11 out of 24, or nearly 48 per cent. In his conclusions Charpentier stated that the results of its employment were not as satisfactory as he had been led to expect; and, while it might be used in appropriate cases he believed that the advice given by Marmorek to discard all local treatment during its employment, could not be conscientiously followed. In the discussion which followed, most of the speakers endorsed Charpentier's conclusions, and Budin went so far as to state that he considered the serum absolutely useless. The unfavorable opinions elicited in this discussion have not prevented many others in France, Great Britain, Germany and America, from continuing the use of the serum. As a result of their work and experience an enormous amount of literature on the subject has been piled up. After a careful perusal of much of it, it must be admitted that the conclusions are greatly at variance, and that the subject has not by any means reached a satisfactory termination, so diverse are the opinions expressed. Accordingly it would seem that the object of this paper would be better reached, and a better grasp of the subject obtained, if the conclusions of some of those writers who have been investigators and accurate scientific observers along the line of modern medicine were summarized, rather than to arbitrarily hand down a decision on the subject.

Dr. Herbert R. Spencer in a paper read before the British Medical Association, 1899⁷ thus sums up his address on serum therapy: (1) As usually applied it has no scientific basis. (2) It has not lowered the mortality of puerperal sepsis. (3) It usually lowers the temperature and sometimes improves the general con-

dition. (4) Its use is not free from danger. Whether it will prove of value in the treatment of pure streptococci infection, detected by the examination of the uterus or the blood, remains for the future to show. I think the remark of Oldshausen is justified "we obstetricians have derived no benefit from serum therapy."

In the discussion which followed Dr. R. H. A. Whitelocke (Oxford) cited two cases. In the first the serum was employed from the 4th to the 10th day, 20 c.cm. being used daily in two injections. He believed the serum did good, causing sleep and profuse sweating. Notwithstanding its use however the temperature on two occasions reached 107° F. The patient also developed an abscess over the left sacro-iliac joint about the 14th day, but ultimately recovered. Murdoch Cameron (Glasgow) never had any satisfactory results from serum therapy, but had observed frequently serious complications due to its use. C. E. Purslow had on the whole formed a favorable opinion of the effects of the serum, and could say certainly that he had never found unfavorable symptoms, either local or general, following the injections. J. W. Byers (Bristol) stated that unless a specimen of the uterine contents could be examined bacteriologically no scientific value could be placed upon it. If there were streptococci present then antistreptococci serum was indicated. The preponderance of opinion seemed to lean towards the remarks of T. Moore Madden (Dublin) and G. W. Taylor (Birmingham). The former believed that puerperal fever was not one distinct disease, but a name applied to many forms of pyrexial infectious disease consequent on parturition, sometimes originating in local inflammatory conditions, sometimes in decomposition of retained placental or other debris, sometimes in ante-partum as well as post-partum exposure to some form of infectious disease, and lastly to direct infection from specific micro-organisms. These different forms of puerperal disease obviously required very different treatment. G. W. Taylor stated that there were strong reasons to believe that acute puerperal septicæmia might be due to various organisms, and until serum was adapted to infection by the staphylococcus and the colon bacillus, as well as to that due to the streptococcus, they could not even begin to use the term treatment in the disease scientifically. At present if any organ-

ism other than the streptococcus were found no suitable serum was available for treatment.

⁸Paul F. Mundè in his article on *Puerperal Sepsis* said his experience with this remedy was limited to three cases, all of which seemed desperate, and had failed to respond to any of the other remedies which had been employed. Three to six hypodermics of 10 c.cm. of serum were injected at intervals of from four to twelve hours and all three cases recovered. In cases where the septic focus had evidently passed from the pelvic organs, going on from bad to worse, he would unhesitatingly employ the serum injections.

⁹Barton Cooke Hirst, in an article on the subject, describes the use of antistreptococcus serum in three cases, one of which died, and in the other two cases he declares that, although they recovered, the injection of the serum did not seem to be of the slightest use. He urged the following objections against its use. *First*, the well tried older plans of treatment for puerperal sepsis will result in a cure of about four-fifths of the cases. *Second*, it is difficult to procure a reliable preparation. *Third*, its use must always be more or less empirical. *Fourth*, the treatment is not entirely free from risk. *Fifth*, there is some danger that too great reliance for a time may be placed in it, to the neglect of older and better tried, and perhaps more successful plans. *Sixth*, no one knows how the remedy acts. If it simply produces a hyperleucocytosis, there are other and simpler agents which will have the same effect.

¹⁰E. W. Saunders, in a paper read before the St. Louis Obstetrical Society, stated that the exact properties of the serum are only partially determined. It has no antitoxic potency, and its favorable effects have been ascribed to certain antibodies or alexins—that is certain definite germicidal compounds. But Bordet has shown that this serum has no more bactericidal properties than normal serum, and that streptococci lose none of their virulence when cultivated in this antidotal serum. He maintains that its principal value lies in its power to stimulate phagocytosis. For the present reliance can safely be placed only in its specific activity against the streptococcus pyogenes and erysipelatis. The study of statistics certainly does not lead one to expect brilliant results. The effects in some cases have been

reported as poor, in some as totally useless, in some downright dangerous, while in others death even has been directly attributed to it. These results may be accounted for in a great measure by the use of worthless or inferior material, or to the possibility of a decomposed serum, or by delay in inaugurating the treatment. A great majority of cases reported were those in which all other measures had proved useless, and in which the disease had existed for many days, and a variety of secondary affections had occurred. Then again too little care is bestowed on the test cases in regard to bacteriological diagnosis. It is necessary in all cases to establish the identity of the microorganisms present, and if a good serum has little effect, it may be safely assumed that other bacteria besides the streptococcus are present. The good effect may be noted in less than twelve hours. The temperature drops, the great mental depression disappears, the appetite returns, the mouth becomes moistened with an increased flow of saliva, the secretion of urine is increased, and suppuration is checked. In conclusion Dr. Saunders reports minutely five cases. In cases 1, 4 and 5, with typical and uncomplicated streptococcus infection, the cure was rapid, and, compared with the usual results in such cases, certainly remarkable. Case 2 had a large number of complications, nevertheless the streptococcus invasion was entirely stamped out, as shown by temperature and lochial examinations. In case 3 the streptococcus infection was eradicated probably after the sixth injection, but the staphylococcus finally caused the death of the patient.

¹ Rosenberg states that the serum has been advocated and recommended by some, while others report utter failures, in proof of which he quotes from English, American, German and French writers to show that the testimony for and against is most contradictory and perplexing. The future, he says, will show whether in serum therapy we possess a valuable factor in combating pronounced puerperal sepsis, or whether it will form another addition to the many remedies which for a time aroused great hopes, but were soon relegated to the vast grave-yard of useless remedies. From what we know about the action of the toxin it is essential that the bacterium responsible for the infection in a case should be isolated, and the corresponding antitoxin administered.

¹¹ Haultain reports three cases of puerperal infection treated with serum. One was owing to the Löffler bacillus, and was cured by diphtheria antitoxin. The second and third were owing to the streptococcus and colon bacillus, and in these the antistreptococcus serum was ineffectual. In discussing the effects of the serum, he says its value is diminished in mixed infection, and in no case should treatment be confined to the serum injection.

¹² M. V. Wallich (at 12th International Medical Congress, Moscow) stated that in his experience the methodical use of serum had materially modified the mortality of puerperal infection. Pinaud when discussing Wallich's report stated that in his experience the mortality had not been reduced through its agency.

¹³ During a discussion which followed a paper read before the Obstetrical Society, London, England, by Dr. Walters, Dr. Amand Routh stated that out of five or six cases of septicæmia treated by himself one had recovered as a result of antistreptococcus serum alone. It was not wise to inject so potent an agent unless it had been previously ascertained that the infection was due to streptococci, and the ordinary treatment, especially the removal of all debris, should not be omitted. John Phillips had administered the serum in several cases but in only one was he certain that the patient's recovery could be attributed to its use. The patient had been ill for many weeks with acute septicæmia. Twenty injections had been given in the course of twelve weeks. The temperature, which was very high, was always lowered, the delirium ceased, and the skin acted, the effects lasting several hours. The President of the Society, in summing up the discussion, said he did not quite agree with the opinions that had been expressed, that the serum ought not to be administered until it had been definitely ascertained that the offending microbe in the case under observation was the streptococcus. As the streptococcus was usually present we ought to give the serum treatment while waiting for bacteriological examination. The safe rule of practice was to explore the uterus digitally under anæsthetic, and then administer the serum without delay.

¹⁴In 1898 a Committee of the American Gynæcological Society was appointed "to consider the value of antistreptococcus serum in the treatment of puerperal infection," and at their meet-

ting in 1899 an elaborate report was handed in.¹⁴ This report is unusually interesting, and worthy of perusal by all interested in modern therapeutics. The committee collected as far as possible all the cases of puerperal infection in which antistreptococcus serum had been used, and found that in France it had been used by 27 observers in 214 cases; in Germany by one observer in 15 cases, and in Great Britain and America by many observers in 123 cases, making in all a total of 352 cases. In these cases it was found that 73 died, a mortality of 20.74 per cent, "which we (the committee) must observe is not sufficiently low to encourage us to proceed further with its use." On further analyzing these cases it is found that 18 German and French observers treated 70 cases in which a bacteriological examination had been made and streptococci found, with 24 deaths, or a mortality of 34.28 per cent. and 31 English and American observers treated 31 similar cases, with 9 deaths, or a mortality of 29 per cent. making a total of 101 cases, with a mortality of 32.69 per cent. Another method adopted was to ascertain the opinions of the various authors who had employed it. Of the 28 German and French observers, 14 expressed a favorable opinion, and the rest an unfavorable opinion, and further when taking the sum total of cases reported on each side, those who reported unfavorably far outstripped the others.

The committee sum up the result of their studies in the form of nine conclusions, the last of which—the one which for the most part directly bears upon the subject under notice—thus reads:—"The experience of one of the members of the committee with antistreptococcus serum has shown that it has no deleterious effect upon the patient, and therefore may be tried if desired, but we find nothing in the clinical or experimental literature, or in our own experience, to indicate that its employment will materially improve the general results in the treatment of streptococcus puerperal infection."

¹⁴Henry D. Fry has published a very interesting and detailed account of eight cases. In selecting the cases cultures were promptly taken from the uterine cavity. Infection due to the staphylococcus, colon bacillus, saprophytic germs, etc., was eliminated, and the serum treatment employed only in the streptococcus infection. The local treatment consisted of a preliminary

and thorough cleansing of the utero-vaginal canal with an antiseptic douche. Two of the cases occurred after criminal abortion, and 6 after labor at full term. Of the 8 cases, 3 died and 5 recovered. After a careful analysis of the effects of treatment and the results of autopsies on those who died, he sums up thus:—
 “If any benefit is to be derived from the use of antistreptococcus serum in a given case of infection, it will respond to the injection of 20 to 30 c.cm. of serum, and from 30 to 50 c.cm. will control responsive cases if treatment be commenced early.”

¹⁶ Richard C. Norris, in a paper read before the Section of Gynæcology, Philadelphia, records very carefully the effects of serum upon a case which had been treated unsuccessfully in the general way for six days. Six days after the first dose the patient developed erysipelas, but ultimately made a good recovery. The article concludes by formulating guiding rules in the treatment of puerperal infection:—“While a careful and early bacteriological study of the secretions from the infected area in the vagina, the cervix, or the endometrium, is in progress, the patient should be subjected to the usual antiseptic and stimulating treatment. The results claimed for hyperleucocytosis induced by nuclein, and for subcutaneous injections of normal salt solution, warrant the employment of these remedies as an adjunct to the general treatment. Should the bacteriological study of the case demonstrate the presence of streptococci, Marmorek's, or other equally reliable serum, should be administered in doses of at least 10 c.cm. daily for three days. The local antiseptic treatment meanwhile should not be neglected. When the streptococcus is found unassociated with other micro-organisms a reliable serum employed early and before widespread and systemic infection has occurred, often will be efficacious. When infection is mixed in character it will avail little, and when streptococci are not present it will be useless.”

Dozens of other quotations might be produced from the mass of literature collected in order to gather together the facts and opinions expressed in this paper, but it seems that sufficient have been given to fulfil the objects for which this paper was written.

R. W. GARRETT.

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THE CURE OF STAMMERING AND STUTTERING.

AN unusual number of cases of impediments of speech have been referred to me during the past few months. With one of these came a request from a medical friend for some details of the proper method of treating these cases. The attempt to comply with his wish has taken the form of this paper.

Adequate treatment of impediments of speech means first of all an accurate knowledge of the conditions absolutely essential for the articulate formation of syllables. These may be briefly stated before discussing the nature and cause of impediments. The first requisite for articulation is a sufficient supply of air in the air tubes and in the accessory tubes as far as the point where the articulation is accomplished. In the second place, there must be subordination of the consonantal to the vocalic action; and finally there must be the preservation of a certain rhythm, that is to say, a proper sequence of the different acts so that each is allowed the proper space of time.

The two most common defects of speech are stammering and stuttering—two terms which by many are thought to be interchangeable and to mean one and the same thing. It is well, however, to define two distinct conditions. Stammering is a fault of articulation of vocalized sound, while stuttering is a fault of co-ordination between articulation and vocalization. A stammerer can vocalize a sound in the larynx, but cannot form that sound into a distinct vowel with the tongue, palate and lips, and the hearer cannot tell which of the five vowel sounds is wanted. A stutterer, on the other hand, places the articulating mechanism in the right position for some consonant, but cannot combine it with the vowel sound, so that the hearer recognizes the consonant at once, but cannot tell which vowel sound is to follow.

In both classes of cases the apparatus of speech in the mouth and larynx is normal, but the development of the thorax and respiratory system is defective. These individuals do not inspire enough air and are not economical in using it. Sometimes they are compelled to stop in the middle of a word to draw breath. Stuttering or stammering may, of course, occur in one

with organic defects of the articulating organs, but this would be a mere coincidence. To differentiate further between stammering and stuttering it may be pointed out that stammering has relation to vowel sound, involves individual letters and may be detected in an attempt to repeat the alphabet; it is not associated with other faulty muscular movements, and is seldom due to lack of nerve control; it is improved in the presence of a teacher and by an effort of the will, and it is manifest in singing or measured talking. On the other hand, stuttering has relation to the pronunciation of consonants, involves only syllables or words while each separate letter can be delivered; it is associated with spasmodic movements of other muscles and is rendered worse by observation or by anything that makes the patient thing of his defect; and, finally, it is not manifest in rhythmical speaking or in singing. In solitude and in darkness the speech is as a rule not impeded. Stuttering is unknown among the Chinese, the exemption being due probably to the rhythmical construction of the language.

An analysis of the phenomena exhibited in the various cases brings out the following conditions: In the vocal mechanism, which includes the larynx, lungs and muscles of respiration, there is a decided want of promptitude in the supply of voice for the initial syllable. Sometimes this is the only fault, the lagging voice being strong in tone when produced, and the energy misdirected into the oral mechanism. In other cases the voice is not only lagging, but also feeble in quality, because the lungs are not filled with air and speaking is done from a half empty chest. When this is the case there is a well marked sense of fatigue after the effort of speaking. In rare cases speech is produced during inspiratory effort. This is called by Wyllie "drawback phonation." The only cases I have seen were two sisters of middle age who had acquired this habit in early life. Stammering at once returned on attempts to speak during expiration. In the oral mechanism the faults are due to surcharge of energy, and from the nerve centres an overflow often occurs; so that spasmodic and involuntary movements are excited in the face or body. Among such movements are spasmodic twitching of the lips and cheeks, working of the jaw, forcible winking of the eyes, contraction of the sterno-mastoid, spasmodic working of the arms, and stamping of the feet.

As to the causation of these troubles it is notorious that they develop more frequently in the male sex than in the female. The proportion is between four and five to one. Some children acquire the defect from imitation of playmates or seniors who stammer. It is more common among children of neuropathic tendencies, the exciting cause being some sudden and violent emotion or a severe fright. There is no doubt the defect may be transmitted by inheritance, though in such cases it is difficult to estimate how large a part is due to imitation of a parent's speech. Finally it is associated with real organic disease or defective development. In general paralysis of the insane and in true aphasia it is well marked, and it is one of the elements in the deranged speech of imbeciles and idiots. The prognosis depends on the age, temperament, and constitution of the patient, and on the causes, form, degree and duration of the affection. The outlook is most favorable when the patient is between twelve and sixteen; old enough to realize the importance of overcoming the defect and yet not so old as to have the habit deeply rooted in his organs of speech. The possession of a good musical ear is also a valuable assistance as it gives the patient a comprehension of tone. The prognosis is unfavorable, (1) when there is a hereditary predisposition, (2) when the symptoms indicate congenital irritability or weakness of the co-ordinating apparatus, (3) when there is some incurable irritative process in the central nervous system, (4) when spasms of the glottis are predominant, (5) when spasmodic movements extend to distant groups of muscles, and (6) when the patient is advanced in years.

Treatment :—There is no more melancholy chapter in the history of surgery than that which has to do with the treatment of stammering and stuttering. It was in 1841 that Dieffenbach proposed to cure stuttering by operations analogous to those done for strabismus and club foot. He made open and subcutaneous transverse cuts through the root of the tongue, and also excised a wedge shaped piece of the tongue. He had many followers who emulated one another in inventing all sorts of stutter operations, but the benefit lasted only so long as there was restraint on the articulation. When the pain subsided, the wounds healed, and the tongue became freely movable the stutter returned. Foriep divided one of the geniö-hyoglossus muscles, and Bennet

cut both. Amusat severed the muscular and ligamentous attachments of the tongue to the lower jaw. Phillip divided the hypoglossi. James Yearsley attributed it to the narrow fauces and cut tonsils and uvula. This history makes interesting reading but it is not pleasant. Mechanical methods of treatment have been equally disreputable. The example of Demosthenes who is said to have spoken with stones under his tongue and so overcome defective articulation, has been most pernicious. There is no record of any other cure from mechanical methods but charlatans have profited from them. In Paris in 1829 Schirman made a fortune from the sale of a small piece of wood as a secret remedy against stuttering. Tongue forks and tongue bridles have been heard of frequently since that time, even down to the present day. The only mechanical means of value are those employed by a dentist to correct imperfect narrowing of the jaw, or defective growth of the teeth; or by insertion of a plate where there is a cleft or a very arched palate.

The proper line of treatment is gymnastic and didactic; gymnastic, to invigorate the entire system and the respiratory organs in particular, didactic, to re-establish by pedagogic methods the correct co-ordination of the functions of respiration, phonation and articulation. Treatment should always begin with exercises in breathing, then follows vocal gymnastics, then exercises in the combination of consonants and vowels. In difficult cases it is well to place the patient under the care of a specialist in voice troubles. It is unfortunate, however, that most of those who conduct schools are persons without scientific training or knowledge, and many have secret methods which they bind their pupils not to divulge. Mystery and secrecy work upon the imagination of the pupil, and if he has any confidence in the remedy, he is likely to improve his speech. The improvement however, is only temporary, if it depends on some trick such as rhythmic movement of the hand or head. Many patients who are intelligent and persevering do not need the aid of a tutor. If they are taught the nature of the defect and the principles involved in its mastery, they will acquire the new habit of speaking in the normal way by dint of intelligent and persevering practice.

J. C. CONNELL.

LITERATURE.

- 1.—Wyllie's *Disorders of Speech*.
- 2.—Kussmaul's "Disturbances of Speech" in Ziemssen's *Cyclopædia of Medicine*.
- 3.—Melville Bell's *Faults of Speech*.
- 4.—Lennox Brown and Emil Behuke, *Voice, Song and Speech*.
- 5.—Gutzman, Stuttering; Its Causes and Treatment in *International Clinics*, Vol. II. 9th Series.

THE ROENTGEN RAY. EARLY DIAGNOSIS OF TUBERCULAR AND OTHER LESIONS.

(Read before the Kingston Medical Society November, 1899.)

FOUR years have elapsed since Roentgen announced his now famous discovery to the Wurzburg Medical Society.

The glow that ushered in this announcement has about disappeared from the horizon and it is now in order for us to survey the field and approximately estimate our gains.

The question may well be asked, of what value to the arts, to science, to humanity has the X ray been, or is likely to be?

In considering a reply, we must not forget that this branch of our knowledge has little more than emerged from its embryonic state, that the ripe fruit is yet far distant.

We are forced to omit as irrelevant any statement as to the value of the X ray in the arts, and to refer but briefly and in a general way to the many benefits claimed for it in medicine, surgery and obstetrics.

The Roentgen ray as an aid to surgical diagnosis is no longer questioned; fractures and dislocations are easily diagnosed; foreign bodies and necrotic areas of bone readily located.

Much—too much we fear—has been claimed for the X ray as a means of diagnosis of calculi—biliary, renal and vesical.

Our results with biliary calculi thus far have been absolutely negative, our efforts many. We have exposed these calculi, within an hour after their removal from the gall-bladder to the X ray, for varying periods, with results that amply justify us in

stating that it is impossible at present to diagnose, with any degree of certainty, gall-stones in the living subject by radiography. Our results with vesical calculi, especially the phosphatic and the oxalate, are somewhat better, but still far from satisfactory.

Macintyre of Glasgow is said to have obtained a radiograph of a renal calculus *in situ* as early as '96. Mackenzie Davidson about the same time demonstrated the presence of a uric acid calculus in the bladder of a living male. The stone measured 3.2 cm. in diameter. The scant literature on the subject, coupled with our own limited successes, forces the conclusion upon us however that failures are not infrequent and that our most modest expectations in this particular branch have not yet been realized.

A moot question too, is the therapeutic effect of the ray. Finsen of Copenhagen is one of the few who attach therapeutic value to it. He, it was, who in the early part of '93 revived the "red ray" treatment of variola. The patient was placed in a room into which only the red rays were admitted. Under this treatment "pitting" is said to be much lessened. It is claimed that the chemical action of rays of light is proportionate to the amount of hydrogen peroxide generated; this being the case, diminution in the suppurative areas can be readily understood. His reported successes in the treatment of *lupus vulgaris* not only with chemical rays but with the Roentgen ray are no doubt authentic.

Kaposi confirms the statement of Finsen and adds *lupus erythematosus* to the list of diseases amenable to this form of treatment.

Holland (Br. Med. Journal, April '99) reports successful treatment of chronic eczema, while Schiff and Freund, in the same journal vouch for the infallibility of the X ray treatment of hypertrichiosis.

To the therapeutic value of the X ray we have given comparatively little attention believing as we did, that it was to the field of diagnosis its operations *largely* belonged and that any therapeutic effects were merely incidental. We are not oblivious to the fact however that the Roentgen ray does exercise a somewhat remarkable influence in promoting germination as the following selected from the elaborate experiments of Maldiney and Thouvenin (Comptes Rendus 126 pp. 548-9, 1898) testify.

Convolvulus arvensis seeds exposed to the rays for about an hour daily germinated on the 3rd day. These sheltered from the rays but otherwise under similar conditions germinated on the 6th day. Seeds of the *lepidum sativum* similarly treated germinated on the second and sixth day respectively. There was no heating-effect due to the rays.

A pertinent question just here is the effect of the ray on the lower forms of vegetable life, viz.—bacteria. It is well known that the action of direct sunlight is fatal to the bacillus tuberculosis.

The results of many varied experiments of Tolomei and others seem to indicate that the action of the Roentgen rays on bacterial growth is the same as that of light but decidedly weaker. The question is by no means settled. The experiments encourage further investigations. Sufficient is in evidence however from laboratory experiments to convince us that the X ray can scarcely claim the distinction of a bactericide.

We have seen indolent ulcers freshen up after four or five exposures, but have rather inclined to the theory advanced by Tesla that the effects were not due to the X ray *per se*, but to the incidental formation of ozone and possibly nitrous acid. The "bleaching" observed by some operators seems to support this theory.

In the field of obstetrics the results are interesting, but so far of very little practical value. We have diagnosed the position of the viable foetus in utero by fluoroscopic examination. This method, however, possesses no distinct advantage over the older and tried methods, and until better results are obtainable its use cannot be seriously encouraged.

When, however, we focus the X ray on the field of tuberculosis the achievements already won cannot but broaden our views and strengthen our hopes. The disease as it exists in the joints is a morbid condition with which every surgeon is more or less familiar. Here, the aid given by the Roentgen ray is invaluable. The limits of the disease can be accurately defined. On the extent of osteal invasion may depend the question of excision or of amputation. To the upper extremity at least this is a question of vital importance. How often has the surgeon made his flaps so that amputation could be performed if excision were found impracticable? By calling the X ray to his assistance the surgeon

is able not only to satisfy himself regarding the extent of the disease, but to assure his patient, before operative measures are adopted, the exact nature of the operation to be performed. The benefits accruing to the patient in this connection cannot be over-estimated.

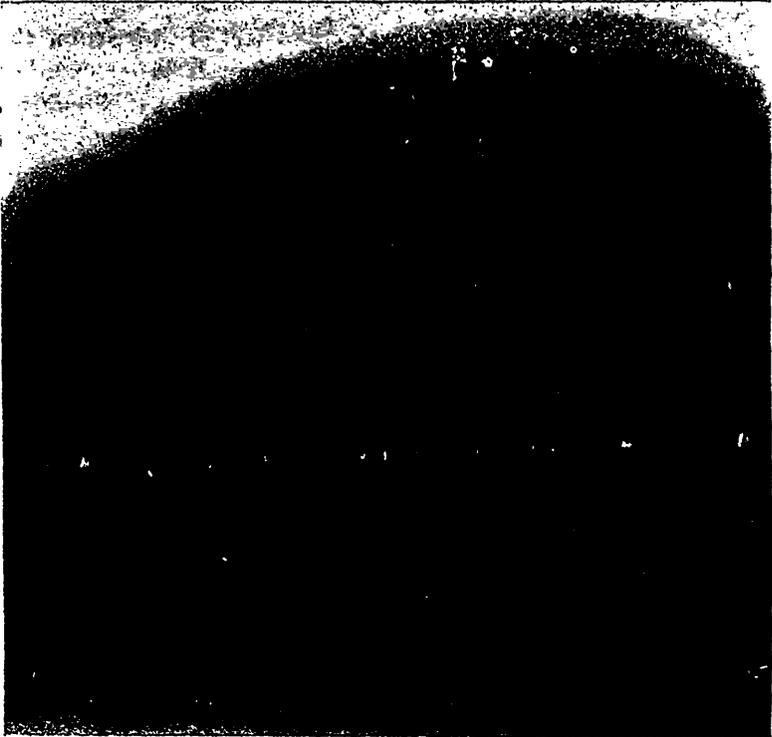


FIG. 1. Normal Elbow Joint. Joint line not very well shown.

Figure (1) shows the relation of the bones in a healthy elbow joint.

Figure (2) shows synovial tuberculosis with consequent ankylosis. The joint line is gone, there is general fusion of bone.

Figure (3) shows not only synovial but osteal invasion. The rarefying process has extended through the central portion of the articular surface of the humerus to the olecranon fossa. From the cut it would be quite impossible to locate the exact area in which the bacilli began operations. A radiograph of this

elbow was taken some few months previously, and it was ascertained that the disease at the time was confined almost entirely to the synovial area.

These affected joints (Figs. 2 and 3) were excised and the X ray diagnoses confirmed even to the minutest detail. An exceedingly useful limb was the result in each case.

Nor is the diagnostic ability of the ray confined to tuberculous disease of the osteal and synovial structures alone. The chest, too, gives up some of its uncertainties. Here, however, with the exception of perhaps thoracic aneurism, radiography yields to fluoroscopy. On account of the prolonged exposure

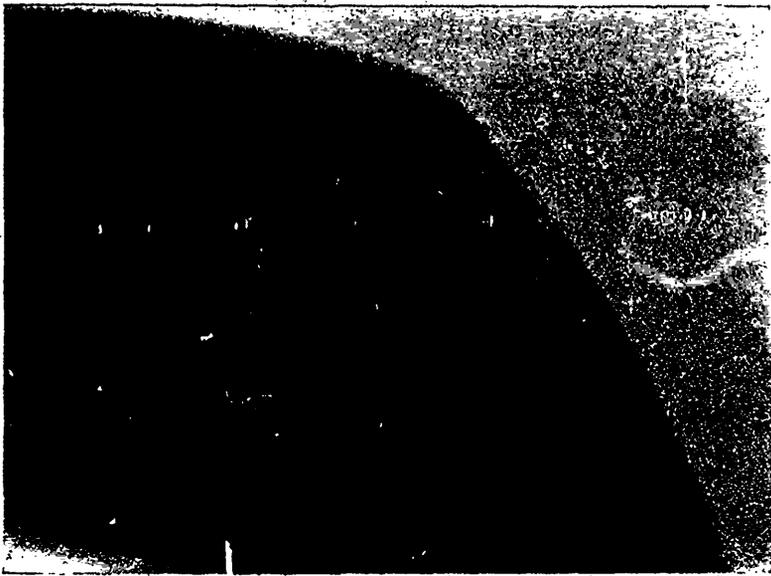


FIG. 2. Synovial Tuberculosis.

necessary it will be apparent to all, that organs endowed with motion the result of respiratory activity cannot be satisfactorily radiographed. By a rather ingenious arrangement, however, Séguy (*Science Abstracts*, Feb., 1898,) has obtained a radiograph of the thorax in thirty seconds. His method is briefly as follows:—Coat a thin glass with bromide emulsion on both sides. Make two flexible screens of linen with Becquerel's violet sulphide of calcium suspended in celluloid. Put one of these

screens on each side of the coated glass plate and back both with black card. Put in a frame under pressure.

This method, interesting though it be as a scientific advance, is not likely to supersede the fluoroscope in thoracic work.

It is perhaps unnecessary to point out that it requires time, patience and perseverance to acquire anything like a moderate proficiency in the use of the fluoroscope in the diagnosis of tubercular disease of the pulmonary organ. Besides this experience what is necessary? A current of even potential giving steady fluorescence and a fluoroscope large enough to cover both

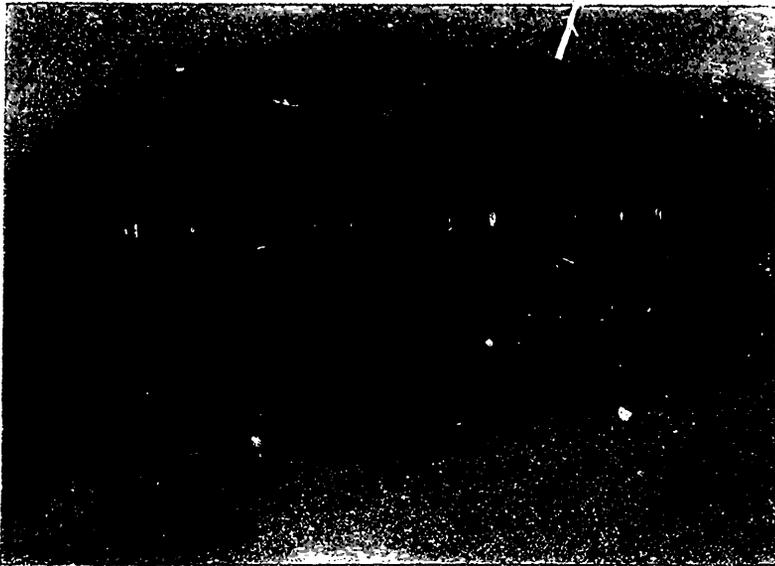


FIG. 3. Advanced Tuberculosis.

sides of the chest at one and the same time. The latter is necessary in order to establish comparisons so essential to an accurate diagnosis.

Having these requisites and applying the fluoroscope to the normal chest what are we able to discern? The ribs, the clavicles, the spine, the pulsating heart, and going a little lower the diaphragm rising and falling with each respiration and carrying with it the liver. The latter organ can be seen very distinctly—more so than any other in either cavity.

It will be readily understood, then, how easily the lung in

the stages of hepatization can be examined and the affected area accurately defined. Nor does the difficulty materially increase with the diminution of the area of the hepatization. Lobar and lobular alike are easily diagnosed.

This being the case it requires no stretch of imagination to understand the applicability of this method to the diagnosis of tubercular lesions even in their incipiency.

No attempt will be made in this brief paper to point out the appearance of the tubercular lesions of the lung as viewed on the fluorescent screen. A knowledge of the relation of the transmissibility of the ray to different densities can only be acquired by actual experience.

To those unacquainted with Roentgen ray work the foregoing will serve to indicate the lines on which differentiation of the physiological from pathological is based, while to those already familiar with it such a dissertation would be superfluous.

We append an analysis of a few cases recently under observation. Of fifty-two suspected cases of pulmonary disease examined during the past three months, largely at the out door-department of the Hospital, thirteen were diagnosed tuberculosis—seven in the incipient and six in the advanced stage. Of the seven in the early stage? the diagnosis was confirmed by bacteriological examination in all but one. The sputum of the seventh up to the present contains no tubercle bacilli. It may be that this is a "cured," case that the cloudy areas in the lung are the result of cicatrization and possibly of calcification. Against this, however, is the fact that the patient's temperature is never quite normal, although it rarely exceeds 100 F. It is significant to note in this connection that in at least two of these cases bacilli were only detected after several careful examinations. Of the advanced cases bacilli and pyogenic organisms were found in all.

Of the remaining thirty-nine there were found four only whose appearance suggested tuberculosis. The sputa of these were examined with negative results.

On more than one occasion during the past forty-three months has an X ray examination of the chest sounded the warning note when auscultation and percussion have indicated no departure from the normal. The progress, great as it is, must only be considered a reconnaissance.

When we realize how many cases of incipient tuberculosis are greatly benefited and perhaps cured by proper attention to diet, climatic conditions &c., we must recognize the duty imposed upon us and grasp every means at our disposal for enabling us to make an early diagnosis and thereby more effectually, than in any other way, combat the ravages of this dire disease.

JAS. THIRD.

KINGSTON MEDICAL AND SURGICAL SOCIETY.

THE annual open meeting was held on Nov. 2nd, twelve practitioners outside the city physicians attending the various sessions.

• The morning session was taken up with operative work in the various departments at the General Hospital.

Operation of Enucleation for Intra-ocular Tumor.—At 9 a.m. Dr. J. C. Connell presented a patient who had been sent to him two months previously by Surgeon-Major Duff. The man, a gunner of A Battery, had first complained of loss of vision in the left eye. The external appearance was normal. With the ophthalmoscope, a small neoplasm was discovered in the vitreous chamber, springing from the retina or choroid, and situated in the lower and outer portion of the chamber, well forward near the ciliary region. During the two months he had been under observation this tumor had gradually increased till about one-third of the chamber was encroached upon. There were no inflammatory symptoms, but a slight increase of tension had developed, and there was some appearance of a hæmorrhage having taken place in the growth. For these reasons the immediate removal of the eye was advised. The opinion was expressed that the neoplasm was probably a melanotic sarcoma, and the later examination by the pathologist confirmed this view.

The eye was removed in the usual way.

At ten o'clock demonstrations were given in the operating room of the Doran Wing by Dr. Garrett and Dr. Wood.

Trachelorrhaphy was performed for a hypertrophied and lacerated cervix, and perineorrhaphy by Emmett's method.

Subsequently the methods of illumination of the interior of the bladder by means of Kelly's cystoscope and head mirror were demonstrated. Three cases were shown, a normal healthy bladder, ulceration of the trigone, and tubercular cystitis descending from the left kidney.

Dr. Anglin next operated in the Fenwick amphitheatre, assisted by Dr. Mundell. A description of his case is appended.

J. C., æt. 34, a strong vigorous man was kindly referred to me by Dr. Duff for operation. The patient had an acquired left inguinal hernia, the result of an accident occurring about a year previously while he was employed on military duty in the Yukon.

The hernia was diagnosed as an enterocele and the sac had descended well into the scrotum. A well fitting truss had been worn for some months, but the patient was very anxious for the relief to be afforded by an operation for the radical cure.

The rules of procedure according to Bassini's method were followed throughout, and as the details of this operation have been given in former issues, they will be omitted here. Suffice to say that a well defined sac was found without difficulty, ligated well up to the internal ring and returned into the abdomen. As usual, sutures of kangaroo tendon were employed for the closure of the canal and also for uniting the divided aponeurosis of the external oblique muscle. Formerly catgut suture had been relied upon for the skin suturing, but owing in some instances to the catgut becoming absorbed too rapidly and thus causing delay in the healing of the superficial wound, it was considered best to supplement the continuous catgut suture by several points of silk worm gut.

The result was all that could be desired in this case. The patient was left in bed for three weeks, and then allowed to get up and sit about. He was supplied with an abdominal pad to be worn for three or four months.

After this operation we find that patients are desirous of getting out of bed at the expiration of two weeks. It is well to keep them recumbent for at least three weeks, and a New York surgeon assured me that he would never consent to operate for the radical cure of hernia, unless the patient agreed to remain six weeks in bed.

In the afternoon, at 2 p.m., Dr. Clarke, Superintendent of Rockwood Asylum, gave a very interesting clinic at the Asylum on Paranoia and Dementia Paralytica. Dr. J. C. Connell demonstrated the eye affections found in these conditions, particularly in latter.

Dr. Herald then gave a clinic upon Heart Disease, presenting a number of patients for examination. The Doctor dealt with the physical examination, laying special stress upon the position and character of the apex beat; the area of cardiac dullness and the heart sounds. As to murmurs the Doctor would always ask the following questions: Is there a murmur? If there is, is it constant or does it sometimes disappear? Where is it best heard? In what direction is it transmitted? In disease of the heart it was pointed out that it was of the utmost importance to obtain all the evidence before making a diagnosis, as upon that depended not only the treatment but the gravity of the prognosis.

At the Medical building, in the evening, Dr. W. T. Connell gave a lantern demonstration on Bacteriology, and spoke of methods of Bacteriological analysis and their value practically. This demonstration was followed at the General Hospital by a demonstration on the X ray and Fluoroscope, particularly in lung affections. Dr. Third has elsewhere in this issue embodied his remarks in a paper.

At 10 p.m. the Society and their guests sat down to a well prepared dinner at the British American Hotel, and after staying the demands of the inner man, with speech and story spent the time till the hours again grew long.

Nov. 14th.—A special meeting, largely attended, was held this evening to consider (a) action, if any, to be taken in the malpractice suit against Dr. Connerty, of Smith's Falls; (b) action to be taken regarding the forms issued by the local Board of Health for certification in cases of contagious diseases; (c) questions arising out of the scheme for Dominion Registration.

The Secretary was instructed to communicate with Dr. Conerty and with Dr. Dickson, of Pembroke, our territorial representative, and to take steps with them to assist in forming a fund for Dr. Conerty's defence.

The Society reiterated its desire for its members to govern

themselves according to the provisions of the Public Health Act. Practically this meant the non-acceptance of the certificate forms issued by the Board of Health. Action was postponed on the question of Dominion Registration.

Dec. 4th.—The regular monthly meeting was held this evening, Dr. Herald, President, in the chair, and a fair attendance of members.

Dr. Sands was elected a member, and Dr. Shurie, of Trenton, an honorary member of the Society.

Dr. Dickson, of Pembroke, was given entire charge of matter relating to Dr. Conerty's Defence Fund, the Society agreeing to back him up at this end of the district.

Dr. W. T. Connell showed the following specimens :

(a) A Larynx, Pharynx and Æsophagus from an insane patient, in whom a partially cooked potato, $1\frac{1}{4}$ by 1 inch, had lodged opposite the cricoid in the æsophagus, causing a necrotic ring of mucous membrane and death by œdema of the glottis in 36 hours.

(b) Epithelioma of Æsophagus, opposite and below Tracheal bifurcation, giving rise to almost complete stenosis of æsophagus. The growth had ulcerated through into left Bronchus by an opening 1 inch long, $\frac{3}{8}$ inch wide. Death was due to suppurative Broncho-Pneumonia and starvation.

(c) Colloid Cancer, primary in stomach, with very extensive secondary deposits in Omentum, Transverse Colon, Liver and Spleen. Received from Dr. Amys, Lakefield. History of over three years' duration, with symptoms of dyspepsia and occasional attacks of acute gastric neuralgia and diarrhœa.

Dr. Kidd then read his paper on Cancer, which appears in this issue of the QUARTERLY.

Dec. 12th.—A special meeting of the Society was held to-day to consider further action regarding School certificates in Contagious diseases.

The President, Dr. Herald stated that he had been invited to a conference with the Chairman of the Board of Health and the Chairman of the School Board. At such meeting it was agreed to accept a properly worded certificate from a medical practitioner without reference of such to a third person.

On motion, the following certificate was adopted :—

I ——— a legally qualified medical practitioner, do under and in accordance with the Public Health Act and the Public Schools Act, hereby certify with respect to ——— lately exposed to, or suffering from ——— and resident at ——— street, Kingston, that he or she is now free from infection, and the house, clothing and other effects of said ——— have been disinfected to my satisfaction, and that all danger from exposure to contact with said person has passed away.

Drs. Wood and W. T. Connell were appointed a Committee, to embody in a report and distribute among the practitioners, the means of disinfection best adapted for houses and clothing infected with Diphtheria, Scarlet Fever or Measles.

Dec. 20th.—A special meeting was called to-day to hear Dr. Roddick, M.P. of Montreal, on his proposed scheme and Bill to effect Dominion registration of Medical practitioners. Dr. Roddick clearly pointed out the objects of the Bill and distributed copies of his proposed Bill and asked criticism on any of its features.

Definite action was postponed till the members had time to more fully consider the details of the Bill.

Jan. 8th.—The regular monthly meeting of the Society was held this evening, Dr. Herald in the chair and 16 members present.

Dr. Morrison was elected a member and Dr. Cowen of Galt an honorary member of the Society.

Dr. Garrett read a paper on "Serum therapy in Puerperal infections," which appears elsewhere in this issue.

The question of Dominion Registration again came up for discussion by Drs. Ryan, Herald, Garrett, Forster, Knight and also by Rev. Principal Grant, who was present. No definite action was taken at this meeting.

A CASE OF HETEROTAXIA.

MRS. G., pregnant with her third child, called me on the 13th inst. I found everything normal except that the pulsations in the fontanelle were slow. The first stage of labor was rather slow, but the second was completed in a few minutes. On birth the child did not cry, so I gave it a slap, whereupon it emitted a rather gasping noise. Every effort was made recommended for the treatment of threatened asphyxia, but without avail. The intervals between gasps became longer and finally ceased, but the heart kept on beating strong, at the rate of about 60. Judge of my surprise when I first noticed that the apex beat was manifested on the right side, and not at all on the left. As a last resort I passed a small gum elastic catheter into the trachea, and tried to forcibly inflate the chest, without result. In doing so I noticed that the trachea was very small; it was barely large enough to hold the catheter, which was a very small one. Finally the heart stopped beating and I quit working, but I must have been at it at least half an hour, and very probably longer. I asked permission to perform a post-mortem, which was granted. Here is what I found: Left lung lying right under first piece of sternum, in size about $1\frac{1}{2} \times \frac{3}{4} \times \frac{1}{4}$ inches; right lung a little bigger, lying on the back of the right side of the chest; both solid; thyroid gland larger than either lung, and extending down into the chest over the left lung; heart—a good one—lying in right side of chest, with its axis in the normal direction; stomach, where the heart ought to have been, and the rest of the left side of the chest filled with small intestine right up to the apex; left side of diaphragm represented only by a narrow band across the front, leaving an opening through which the intestines could be moved freely up or down; rest of organs and abdominal cavity apparently normal. Outwardly, the child was normal in every respect, except that I would have thought it not quite mature, but the mother says it was.

P. M. CAMPBELL.

QUEEN'S MEDICAL GRADUATES AND THE WAR IN
SOUTH AFRICA.

WHEN we find that the Empire is roused over the war in South Africa, it is not surprising that medical men from every quarter of the Empire are offering their services. We are pleased to note that the medical graduates of Queen's are not behind others in this respect. The following are now in service :

H. R. Duff, 1884. Surgeon-Major to Mounted Rifles.

A. E. Ross, 1897. Hospital Sergeant in same corps.

R. R. Robinson, 1892. A private in the same corps.

H. E. M. Douglass. Surgeon in the Highland Brigade.

J. Alton L. Harriss, 1899. First with the Hospital Ship "Maine;" now attached to the 19th Field Hospital and Bearer Corps.

Knowing all these men as we do, we are confident they will discharge the duties assigned them faithfully and in such a manner as to reflect credit upon themselves, their Alma Mater and Canada. We hope to hear from some of them before long, and we shall be pleased to publish any notes they may send us from the front.