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THE
CANADIAN PRACTITIONER

FORMERLY "THE CANADIAN JOURNAL OF MEDICAL SCIENCE."

EDITOR:

A. H. WRIGHT, B.A., M.D. Tor., M.R.C.S. England.

Business Management, - - THE J. E. BRYANT COMPANY (Limited), 58 Bay Street.

TORONTO, FEBRUARY 16, 1891.

Original Communications.

THE GERMS OF TYPHOID FEVER.

BY VICTOR C. VAUGHAN, M.D., PH.D.

In 1880, Eberth,¹ of Zurich, in eighteen out of forty cases of typhoid fever, found in sections of the spleen and mesenteric glands a short bacillus which he was unable to detect in similar examinations of twenty-four persons who had died of other diseases. From these facts this distinguished Swiss investigator thought himself entitled to the claim of the discovery of the true germ of typhoid fever.

Before Eberth's report, Koch had observed this germ, and had micro-photographs of it. Indeed before Koch,² Browicz,³ Sokoloff,⁴ and Fischel,⁵ had individually found in the tissues of typhoid subjects oval bacilli which were probably identical with those of Eberth and Koch. This organism is designated by some as the Koch-Eberth germ, and by others as the Eberth germ, or bacillus. For the sake of brevity, I shall in the following discussion employ the shorter name.

This organism is now regarded by the majority of bacteriologists as the true and only cause of typhoid fever, and I shall give my attention first to the grounds upon which this belief is founded. Has it been conclusively demonstrated that the Eberth germ is the sole and sufficient cause of typhoid fever? To what extent have the four rules of Koch been complied with in the study of this germ? Are there any reasons outside of

these rules for founding a belief either for or against the specific nature of this bacillus?

In the first place, is this organism found invariably in typhoid fever? As has been stated, in his first report Eberth found it in eighteen out of forty cases; but since that time methods have been perfected and many other investigators have studied this point. In the examination of twenty-eight bodies, dead from typhoid fever, Gaffky⁶ found the Eberth germ in twenty-six; Frankel and Simmonds⁷ reported it in twenty-five out of twenty-nine cases; Seitz in twenty-two out of twenty-four; Rietsch⁸ in thirty-five out of thirty-six; and Kowalski⁹ found it present in each of twenty-nine cases.

Without citing further authorities, I may say that the number of cases of genuine typhoid fever in which this germ has not been reported, when the examinations have been made by competent men, is so small that we are justified in claiming that the first of Koch's rules has been complied with. Indeed, in reading over the great number of cases referred to in the volumes of Baumgarten's "Jahresbericht," in which the Eberth germ has been reported to be present, one becomes somewhat suspicious of the universality and readiness with which this organism has been detected. Gaffky and others found it always present in the mesenteric glands and spleen, often in the liver, and not so frequently in the kidney. In the intestine it has been found, in the early stages, in the swollen follicles and plaques; and in the deeper layers, before there is any necrosis; but with the advent

of ulceration there is found secondary invasion. But in addition to these organs, the Eberth germ has been reported by Chantemesse and Vidal¹⁰ in the lungs of typhoid patients with bronchitis, broncho-pneumonia and pneumonia. The same observers report the germ in the brain; Curschmann,¹¹ in the spinal cord; Zenker and Hoffmann,¹² in the voluntary muscles and in the marrow of the bones; and Reber,¹³ Neuhaus¹⁴ and Chantemesse and Vidal, in the placenta of typhoid patients. In twenty samples of blood taken from the finger, during life, of typhoid patients, Meisels¹⁵ reports the finding of this germ in nineteen. Neuhaus examined the blood taken from the eruptive spots in typhoid fever with success. Maragliano¹⁶ and others examined the blood taken from the spleen during life as a means of diagnosis. The Eberth germ has been found in drinking-water by Dreyfuss-Brisac and Vidal,¹⁷ Michael,¹⁸ Moers,¹⁹ Chantemesse, Loir,²⁰ Thoinot, De Blasi,²¹ Galbucci,²² Beumer,²³ and others. Granting that these observers have not been mistaken in the nature of the germs which they have found, we must admit that the Eberth germ is widely distributed. Banti²⁴ reports Eberth's germ in a case which he calls atypical typhoid fever, and in which the intestine was perfectly sound. This case is pronounced typhoid fever, notwithstanding the absence of any indication of the characteristic lesion of the disease, because the germ was found to be present. Whether or not this is justifiable is, I think, very questionable.

That the second of Koch's rules has been complied with, there is no doubt. Pure cultures of this germ are to be found in every bacteriological laboratory. It grows readily in our artificial media, and its (accredited) characteristics of growth are well known. The attempts which have been made to induce the disease in the lower animals by inoculation with this germ are numerous and interesting. I shall not attempt to review all of these, but will call your attention to some of the more important ones. The first to experiment with pure cultures was Gaffky. To five Java apes, he fed daily cultures without any results. Later these animals died of tuberculosis, and section showed no lesions resembling those of typhoid fever. Equally without result were two experiments upon apes; in one, the inoculation being made

directly into the blood, and in the other, in the tissues over the sternum. Gaffky also inoculated rabbits, guinea-pigs, rats, mice, pigeons, a chicken, and a calf, all without result. In these, the inoculations were made directly into the blood vessels or into the peritoneal cavity.

The experiments of E. Frankel and Simmonds²⁵ seemed at first to prove that the Eberth bacillus is pathogenic to some of the lower animals. These experimenters suspended the germ taken from potato cultures in sterilized water and injected this either directly into the blood or into the peritoneal cavity. Nineteen, out of thirty-two rabbits, and twenty-seven, out of thirty-one mice, succumbed under this treatment. These animals survived the injections from a few hours to four days, the majority dying on the first day. The symptoms observed consisted of lessened response to surroundings, lessened inclination to move, loss of appetite, and, in many, diarrhoeal discharges. Section showed enlargement of the spleen and mesenteric glands, and swelling of the intestinal follicles. In some there were observed enlargement of the axillary and inguinal glands, ecchymoses of serous membranes, inflammatory redness and hemorrhage in parts of the intestines, and parenchymatous swelling of the liver and kidneys. In no case was there suppuration at the point of inoculation. In the spleen, mesenteric glands, liver, kidneys, and Peyer's patches, the bacillus could be detected. However, the abundance of the bacilli did not correspond with the extent of the macroscopic changes. From these experiments, Frankel and Simmonds concluded that they had successfully transmitted typhoid fever to these animals.

Independently of the above, Di Vesta inoculated, with fatal results, rabbits and guinea-pigs. The lesions were not so marked as those found by the Hamburg investigators. Later, A. Frankel²⁶ inoculated guinea-pigs with the Eberth germ after the manner employed by Nicati and Rietsch in their experiments with the cholera bacillus, and which consists in introducing the germ directly into the duodenum, either with or without ligature of the bile duct. Of the fourteen animals thus treated, seven died. The anatomical changes observed were similar to those found by E. Frankel and Simmonds.

Seitz,²⁷ employing Koch's method for pro-

ducing cholera in guinea-pigs, found that seven out of sixteen animals died within forty-eight hours, and one other after four days. The germ could be detected in the intestines, but not in the other organs. Intravenous and intraperitoneal injections of the germ produced results similar to those obtained by E. Frankel and Simmonds.

Beumer and Peiper²⁸ confirm the observations made by E. Frankel and Simmonds, and A. Frankel and Seitz, but interpret their results in a wholly different way, and reach a diametrically opposite conclusion. In the first place, they find that the effect upon the animal is in direct proportion to the amount of the culture injected; an amount which is small, but which still contains many thousand germs, fails to produce any symptoms. Secondly, they prove that the Eberth germ does not multiply in the body of the animal, but, on the contrary, soon dies. Thirdly, they show that the same symptoms and identical structural changes result from injecting like amounts of certain well-known non-pathogenic bacteria, such as bacillus subtilis, the greenish-yellow liquefying bacillus of water, the white bacillus of water, and others. Thus they show that the death of the animals is due to intoxication, and not to infection; also, that the intoxication is not specific, inasmuch as it is produced with equal facility with other bacteria.

Finally, Sirotinin²⁹ demonstrates that the same symptoms and pathological lesions are produced by sterilized and non-sterilized cultures of the Eberth germ. This completes the proof that the effects observed are not due to the growth of the germ within the body.

To sum up the evidence which we have on this point, we may say: (1) A germ giving the tests supposed to be characteristic of the Eberth germ is found invariably in the bodies of those dead from typhoid fever. (2) It has been isolated and grown in pure cultures. (3) All attempts to induce typhoid fever in the lower animals by inoculation with this germ have so far been without success. (4) Experiments show not only that the germ fails to multiply in the lower animals, but that, when introduced by inoculation, it soon dies.

I think that every bacteriologist will agree with me in the conclusions which I have just drawn; but some of them will say with Baum-

garten³⁰ that the lower animals do not have typhoid fever, while others agree with Welch³¹ that it is not necessary to reproduce the disease in animals in order to prove the causal relation between the germ and the disease. Welch states: "From our present knowledge, we are justified in believing that a micro-organism which is invariably associated with a disease, which is found in the lesions of the disease and in situations which explain the symptoms and lesions, and which is never found except in association with the disease, must be regarded as the cause of the disease." By one or the other of these assertions the majority of bacteriologists satisfy their scientific minds and accept Eberth's germ as the true, sufficient, and sole cause of typhoid fever. Neither of these explanations has been satisfactory to me, and we will briefly consider their weight.

In the first place, do any of the lower animals ever manifest the symptoms and demonstrate the lesions of typhoid fever? In discussing this point, we will leave out of consideration for the time being all questions about the identity of the germ which may cause typhoid fever in man and that which may cause the same disease in the lower animals. In fact we will not, for a few moments, say anything concerning germs, but will endeavor to answer the plain question, are the lower animals wholly free from typhoid fever? In the first place, it will be necessary for us to know just what symptoms and lesions are necessary in order to constitute typhoid fever. The only constant symptom is the continued fever. There may be either diarrhoea or constipation; the eruption either may or may not appear: active delirium is frequent in some epidemics, while in others the mind is perfectly clear throughout the course of the disease. In short, as we all know, there are the greatest variations in the symptoms among which the only constant and invariable one is the continued fever. The only constant lesions are the inflammations of the solitary and agminated glands of the ileum, often progressing to ulceration, and the enlargement of the spleen and mesenteric glands. If a man should manifest a constant elevation of temperature extending over from seven to forty-two days, then should die, and a careful *post mortem* examination should reveal ulceration, possibly perforation, of the ileum, and

enlargement of the spleen and mesenteric glands, with no tuberculosis, what would any intelligent jury of medical men give as the cause of death? Could it be other than typhoid fever? Now, if this symptom and these lesions are observed in a dog or cat, what shall we name the disease? These symptoms and these lesions may be induced and do occur in the lower animals.

Huguenin, as quoted by Murchison, says that enteric fever with characteristic intestinal lesions is by no means uncommon among cattle in Switzerland.

In 1882, Brautlecht³² inoculated rabbits with germs from water supposed to be the cause of a typhoid epidemic, with the following results: The animals had some elevation of temperature, lost flesh until they became mere skeletons, and died after from two to eight weeks. Very young animals died within two or three days after having profuse diarrhoeal discharges. *Post mortem* examination showed in the adult animals invariably a severe catarrh of the small intestines, and marked enlargement and coloration of the spleen and mesenteric glands. The walls of the intestines were thickened and friable, the mucous membrane, swollen, yellowish, and reticulated. Ulceration was present in some cases, but was generally confined to individual glands.

In 1885, Semmer³³ reported typhoid fever in three dogs and a horse. The lesions were found to be well marked. In 1888 he confirmed his statement regarding the occurrence of typhoid fever among the lower animals, and reported finding in some of these a germ similar in form and size to that of Eberth, and in others an organism which differed from this only in being larger.

Drs. Hendricks and Brewer made the following report on the *post mortem* examination of a dog which died on the twenty-eighth day after I had inoculated it with a germ obtained from water supposed to have caused typhoid fever: "The weight of the body was twenty pounds. Upon the abdominal cavity being opened, the mesenteric fat was observed to be pink, with hemorrhagic spots. The blood vessels of the intestines were found to be highly injected, with numerous points of hemorrhagic infiltration. On the mesentery near the cæcum were large hemorrhagic spots. The lower inch and a half of the ileum was distended and covered exter-

nally with clotted blood. The mesenteric glands were enlarged to four or five times their normal size, and each contained numerous hemorrhagic spots. The glands of the vermiform appendix were much enlarged, and were pitted in the centre. Under the mesentery of the cæcum was a plastic exudate, almost transparent. The descending portion of the duodenum showed numerous bright hemorrhagic spots along the line of its junction with the mesentery. Along the upper portion of the ileum the intestine presented a greenish macerated appearance, and a soft, pultaceous feel. Air blown into the intestine escaped through the walls at this point. The mucous membrane of the ileum was swollen and Peyer's patches enlarged, but there were no distinct ulcerations. The liver was indurated and of a deep cherry color; weight, twenty-three ounces. The kidneys were enlarged to twice the normal size, and were dark in color. The spleen was enlarged, and of a dark purple color. The stomach was contracted, and its mucous membrane showed a few hemorrhagic spots. The lungs were normal, and the heart in diastole and free from clots. The thymus and lymphatic glands were enlarged."

Flügge³⁴ says: "It is known that a group of widely distributed organisms, which, however, are wholly different from the typhoid bacillus, have the power, when injected subcutaneously or intravenously, of producing in animals death, with marked swelling and ulceration of Peyer's patches. To these organisms undoubtedly are due the apparently positive results which some authors have supposed to be due to inoculation with the typhoid bacillus."

It is unnecessary to bring forward further evidence on this point. It must be acknowledged that members of several species of our domestic animals occasionally manifest the essential symptoms and lesions of the disease which in man we designate as typhoid fever. This disposes of one of the two explanations given by some bacteriologists for the failure to induce typhoid fever in the lower animals by inoculation with the Eberth germ, and we will proceed to a discussion of the second explanation.

When we say that Eberth's germ is invariably found in the spleen and mesenteric glands of those dead from typhoid fever, we understand

that this germ possesses certain characteristics either in form, size, reaction to staining agents, in manner of development or growth, or in some other particular by which it can be distinguished from all other germs. If it can be shown that the organism has no such characteristics, or that its supposed characteristics are possessed and reproduced by other germs, then all the evidence of its specific nature which has been drawn from its constancy in typhoid cases becomes worthless, because no investigator can then say that the organism which he finds belongs to the same species which Eberth observed; for the means of recognizing this species to the exclusion of all others do not exist, or at least have not been applied.

What are the tests upon which we rely in the identification of Eberth's germ? No bacteriologist will, I suppose, claim that there is anything in the form or size of Eberth's germ by which it can be distinguished from the ordinary saprophytic bacilli; but on this point I will fortify myself with a quotation from Baumgarten, a most earnest believer in, and a most able exponent of, the specific character of the organism. He says: "The typhoid bacillus in those development forms in which we most frequently see it (namely, in the infected body of man) shows a very great resemblance to the common, widely-distributed bacilli of ordinary putrefaction which were formerly designated by the now antiquated name of bacterium termo. In the fresh typhoid infiltrations of the intestinal walls, in the typhoid, infiltrated mesenteric glands, in the hyperplastic spleen, in the liver, in the kidney, etc., our bacilli (Eberth's germ) appear just like (gleichwie) the bacterium termo, as short (scarcely three times as long as broad), relatively thick rods with rounded ends, which are often joined in twos, at times in threes, so as to appear on a superficial examination like long single bacilli." We therefore see that it is admitted by those who believe in the specific nature of Eberth's germ that there is nothing in its morphology by which it can be distinguished from certain putrefactive bacilli. We turn, therefore, from this point and now proceed to the study of certain tinctorial and growth properties, by means of which Baumgarten and others state that Eberth's germ can with absolute certainty be distinguished from the bacilli already men-

tioned. Great stress has been laid upon the statement that Eberth's germ takes the aniline colors with difficulty and imperfectly, while the putrefactive bacilli take such stains promptly. I once carried to one of the most renowned teachers of bacteriology in Germany cultures of a germ which I had isolated from drinking-water, and after inoculation with which three dogs had manifested continued fever, one of the animals dying on the twenty-eighth day, a second on the thirty-fifth day, both showing on *post mortem* examination in a marked degree the essential lesions of typhoid fever, and the third finally recovering, and asked him if it might not be a modified form of Eberth's germ. He found that my germ did take the ordinary stains, and from this fact alone claimed that it could have no relation to Eberth's germ, that it was nothing more nor less than a saprophytic bacillus, and that either it had no causal relation to the diseased condition in the dogs or the disease in the dogs was not typhoid fever, "Because," said he, "there is no doubt that Eberth's germ is the specific cause of typhoid fever." I think that I am now in a position to demonstrate that this test is wholly unreliable. In the first place, the behavior of Eberth's germ to staining reagents is wholly dependent upon circumstances, and will vary greatly with different samples. This is partially recognized by Baumgarten, who states that the difference in the readiness of taking stains between Eberth's germ and the putrefactive bacilli is less marked in cover-glass preparations than in sections. Eberth's germ, which has been grown artificially through many successive generations, takes the stain quickly and deeply. I have here preparations made from a pure culture obtained in the Hygienic Institute of Berlin in August, 1888, and that these bacilli have taken the stains to which they were exposed for less than a minute can be readily seen. In the tissues, or in cultures recently taken from the tissues, this germ is stained with difficulty; but when long grown outside the body, it takes these same stains readily. Now, what condition leads to this difference? I have here two germs taken from drinking-water, which as first obtained are stained quickly and deeply, but after they have been kept at a fever temperature for a time, they manifest the same difficulty in staining as is shown by Eberth's germ. This difficulty in

staining increases directly with the temperature and time of exposure. Moreover, readiness in being stained is not immediately regained by succeeding generations of the heated germ, even when these successive generations are grown at ordinary temperature. I will present to you preparations of these germs along with those of Eberth submitted to the action of the same stains for the same length of time, and I am sure that no one will be able to distinguish one from the other by any difference in depth of stain.

Great stress has been laid upon the fact that Eberth's germ is not stained by Gram's method, while the putrefactive bacilli are stained by this method. This apparent difference is also due to differences in temperature at which the germ has been grown. I have here preparations of a bacillus obtained from drinking-water, which, after having been kept at from 39° to 40°C. for from three to twenty-one days, are wholly unaffected by Gram's method of staining. I must therefore conclude that the method of distinguishing Eberth's germ from certain putrefactive bacilli by differences in behavior towards staining reagents is wholly unreliable.

I will submit to you numerous preparations of two germs obtained from water along with similar preparations of Eberth's germ, the length of exposure to heat and the degree of heat being indicated on each slide, and I am sure these preparations will bear me out in the above conclusion.

We have been taught that the invisible mould which Eberth's germ forms on potato is an absolutely certain means of its identification and distinction from all other germs. Frankel says that this "is a sure means of correct recognition, for the growth is perfectly characteristic and does not occur with any other known germ." This positive statement is reinforced in the same paragraph as follows: "This growth of the typhoid bacillus is so characteristic that with its help one can at any time distinguish this from other bacteria, and one should not report the presence of this germ until every doubt has been removed by the potato culture."

Baumgarten also states that the growth on potato is wholly characteristic, and gives this method of distinguishing Eberth's bacillus from that of Emmerich (a *fæces* bacillus), which, as he

states, agrees with the Eberth germ in its growths on gelatine, agar, and blood-serum. Now, the two germs which I have here, and which I obtained from drinking-water supposed to have caused typhoid fever, grow on potato, when they have not been kept at a fever temperature, much like the *fæces* bacillus, forming a raised whitish mould; but after having been kept for seven days or longer at from 39° to 40°, they form a wholly invisible growth on the potato which cannot be distinguished from Eberth's germ. Proofs of this statement are here before us, and I am sure that no one could select out the potato which bears Eberth's germ.

It will be well to speak somewhat in detail concerning these germs which I have obtained from water, and also of the exact conditions to which they have been subjected in order to so modify them that they are affected by staining agents in the same manner as Eberth's germ, and also produce a like growth on the potato.

In October, 1888, I adopted the following method of making a sanitary analysis of drinking-water: In all cases where the samples are collected by the laboratory assistants, a sterilized flask closed with cotton is used. In other instances, when I am informed by a health officer that he desires the analysis of a water, I send him a sterilized bottle, or instruct him how to sterilize a bottle. In other cases, the water comes usually in a new bottle or jug which has been well rinsed with the same water. As soon as the water is received, plate cultures are made and a test tube of beef-tea is inoculated with one drop of the water. This tube is placed in an incubator and kept at 37° C. for twenty-four hours. Then twenty drops of the beef-tea cultures are injected with a sterilized syringe into the abdominal cavity of a white rat or guinea-pig. In some instances rabbits have been used, and with these the amount injected has varied from twenty to sixty drops, according to the size of the animal. If the water contains a toxicogenic germ the animal dies, usually within twelve hours. A *post mortem* is made, the gross appearances noted, and plates are prepared from the spleen, liver, kidney, and sometimes from the blood. After twenty-four hours longer these plates have generally developed (in some instances a longer time is required), and they are compared with the plates made directly from the water. On

the plates made directly from the water the germs are counted and their general appearance noted after twenty-four, forty-eight, and seventy-two hours. In the meantime the chemical analysis is completed, and under favorable circumstances the report can be made three days after the water has been received; a week furnishes all the time required in any case. The germs taken from the plates can then be studied at leisure.

In this manner 160 samples of drinking water have been examined. Of this number, twenty-four were supposed to be causing at the time typhoid fever. In all of these, with the exception of three, toxicogenic germs have been found. I will now give a short description of each of these micro-organisms:

Bacillus A.: This is a short, motile bacillus, which does not liquefy gelatin, and which forms a white, raised, moist growth on potatoes. It forms no gas, takes the ordinary stains fairly, and gives a positive result with Gram's method. Of six full-grown rabbits, each inoculated by the injection of sixty drops of a twenty-four hour culture in beef-tea into the abdominal cavity, all died. Two rabbits which were given subcutaneously twenty drops each of this culture were not affected. Of fifty rats which were given twenty drops of this culture by injection into the abdominal cavity, all died. Two rats which were inoculated subcutaneously died after ten days. Of eight guinea-pigs inoculated in the abdominal cavity, all died; and a like result followed the subcutaneous inoculation of the animals.

Cultures of this germ were tested along with similar cultures of bacterium coli commune, and the former appeared to be slightly more pathogenic with rats and guinea-pigs, and less so with rabbits than the latter.

In every case of death after inoculation with this germ, plate cultures were made from the spleen, liver, and kidney, and in many instances from the blood of the heart, and without exception the plates showed abundant growths; therefore, this germ not only produces death, but is capable of living in the rat for ten days.

The *post mortem* appearances vary with the time which elapses between the inoculation and death. In cases dying within twenty-four hours the intestines are, as a rule, slightly reddened; the liver, spleen, and kidneys engorged; and the

heart has invariably been found in diastole. In cases which survive for several days there may be ulceration of the intestine; the spleen and kidney are enlarged, and the liver dark. In a few instances, all of which, strangely enough, have followed subcutaneous inoculation on the back, a marked peritonitis has been found after death. The same fact has been observed with bacillus B. In no instance has suppuration resulted from an inoculation.

In the majority of samples of drinking-water in which I have found bacillus A, no other organism has developed on the plates. In other words, the water is a pure culture of this germ. It may be of interest to say something about the cases of typhoid fever which were supposed to be due to the waters containing this germ.

Of the cases of typhoid fever supposed to be due to one of these waters I had personal knowledge. First, a girl of sixteen was attacked, and I was called in attendance. I inquired concerning the drinking water, and was told that the hydrant water only was used. I could not believe that this water was the cause of the fever, but ordered that all water used in the house should be boiled. I was asked if the disease was contagious, and replied in the negative; but having in consideration the comfort of my patient, advised that two young brothers and a girl visiting at the house should be kept away from the sick room. Within eight or ten days one of the brothers came down with the fever, and we learned that the visitor, who had gone to Cincinnati, was suffering from the same disease. Of course, the mother now claimed that the disease was contagious. Believing that there must be some local cause, I examined the house throughout, but without satisfaction. A little later, a young man rooming in the house was attacked. By this time the first patient, who had been continuously delirious for three weeks, had become rational. When asked about having used any other than the hydrant water, she stated that she always used the cistern water to clean her teeth, and had also used it in a nasal douche, and in spraying her throat. The brother and the student also remembered that they had occasionally drunk the cistern water.

The examination of the water was immediately undertaken, and it was found to be exceedingly bad chemically, and in number and kind of

germs. The stools of two of the patients were then examined, and the same germ found so abundantly in the water was recognized in the stools. Of course, I cannot say that this germ was the cause of the disease. Fortunately, all of these cases finally recovered, and there was no opportunity to examine their spleens. I cannot refrain from mentioning a few facts concerning the symptoms. Delirium was a marked and persistent feature; it was not the low muttering delirium, but wild, causing the patients to attempt to get out of bed. The fever ran forty-two days. There was no eruption over the abdomen.

A sample of water from Wyandotte, Mich., illustrates how a source of infection may continue to spread the disease through months. Dr. E. P. Christian reports eleven cases occurring among those who used the milk furnished by one man. The first case occurred July 13th and the last Nov. 15th. Dr. Christian sent me some of the water from the well of this milkman, also some of the milk. The water was a pure culture of bacillus A, which with three other germs appeared on the plates made from the milk; while from the organs of the animals inoculated with the milk, bacillus A in pure culture was obtained.

Bacillus A, as obtained from water, differs from Eberth's germ in the following particulars:

(1) It is somewhat shorter and thicker than the Eberth germ which I obtained in the hygienic institute of Berlin. In the majority of the samples the length is about twice the breadth.

(2) The readiness with which the ordinary stains (as Bismarck brown) are taken is very variable; the germs from some of the waters taking the stains readily, while others are not so deeply stained as the Eberth germ.

(3) It is stained by Gram's method.

(4) It forms a plainly visible, white, raised, moist growth on potatoes.

(5) The colonies on the gelatine plates are very uniform, quite generally spherical, and never spreading over the surface.

(6) It grows on gelatine plates more rapidly than Eberth's germ.

(7) Its toxicogenic properties are greater than those of the Eberth germ.

I have studied the effect of heat on this germ in the following manner: A man, while drunk, fell down stairs and fractured his skull, from

which injury he died in about three hours. The spleen was removed with aseptic precautions. With sterilized knives it was cut into pieces, which were placed in sterilized Esmarch dishes. Three pieces were inoculated with bacillus A from as many different sources, two with bacillus B, and one piece was left without inoculation. The dishes with their contents were placed in an incubator and kept at from 39° to 40°. Plate cultures were made from these pieces from time to time, and the germs thus obtained were compared with the bacilli as obtained from the water, and with Eberth's germ. The plates from the uninoculated piece of spleen have invariably remained sterile, showing that the organ contained no germs. As these cultures were made from time to time, the above mentioned peculiarities of growth, by which bacillus A had been so easily distinguished from Eberth's germ, were found to become less and less prominent, and finally they have so far disappeared that they can no longer serve for purposes of distinction between the organisms. After only seven days bacillus A has been so altered that its growth on potato can no longer be distinguished from that of the Eberth germ, with which the comparison was made. Bacillus A becomes less and less receptive to the ordinary stains, and I will show you slides made from the spleen after fourteen and sixteen days which have not taken any of the aniline stains, not even the carbolic fuchsin, as deeply as the Eberth germ. This is true even after the altered bacillus A has been exposed to the stain for seventy-two hours, while the Eberth germ was exposed less than five minutes to the same stain. Gram's method ceases to produce positive results. The length of the germ increases in proportion to its breadth, and in some cases long filaments have appeared. I have been much impressed in carrying on this work with the longer time necessary for colonies to develop on plates after heating the germ. It is generally stated, and I had always, before making these experiments, believed, that pathogenic germs develop more tardily than the ordinary saprophytic ones; but the longer this germ is kept at a fever temperature, and the higher that temperature is, the more tardily do the colonies appear on the gelatine plates. In short, I have been forced to the conclusion that the so-called

characteristics of Eberth's germ are not inherent in the organism, but result from the effects of the environment under which the organism has grown in the body.

Bacillus B. : This is also a short motile rod, which in cultures from four to six weeks old shows some liquefaction of the gelatin. It is certainly a slowly liquefying germ. It sometimes forms gas along the line of the stick. This occurs, if at all, during the first few days of growth, and long before liquefaction begins. It forms a white, raised growth on potato. The ordinary stains are taken readily, and Gram's method yields positive results. This germ agrees very closely with bacillus A in its pathogenic properties and in the *post mortem* appearances induced, so closely, indeed, that it will not be necessary to repeat details. This germ has, in the few instances in which it has been tried, failed to kill rats when inoculated subcutaneously, though it is fatal to guinea-pigs when employed in this manner. The plates prepared from the organs of animals killed with this germ have invariably yielded numerous colonies. The germ is also found in the blood.

As has been stated, bacillus B was subjected to the same line of experimentation as A, with practically the same results, and a detailed repetition is not necessary. It should be remarked that the chief points of difference between A and B, as well as those between these and Eberth's germ, have disappeared in B, as obtained from the pieces of spleen. Stick cultures in gelatine tubes have not been found to generate gas or to liquefy gelatine.

From these experiments I must conclude that the evidence of the existence of a specific and individual germ which is the sole and only cause of typhoid fever is not at present conclusive. On the contrary, I am strong in the belief that there exists a family or class of germs, any one of which may cause the essential symptoms and lesions of that disease or group of diseases which we now designate by the name of typhoid fever. It may be that in the future we shall be able to differentiate from the symptoms the diseased condition caused by one of these organisms from that caused by another; but at present we are unable to do so.

It seems to me that the view expressed above is supported by the clinical history of the disease.

As has been already remarked, the symptoms of this disease do not show the constancy and invariability characteristic of a truly specific disease, such as anthrax or small-pox; but on the other hand, the symptoms show marked variations in their appearance and in their gravity.

I may state that I do not stand alone in questioning the specific nature of Eberth's germ. Some months ago Rodet and Roux³⁵ found that the bacterium *coli commune*, when cultivated at a temperature of 44° to 46°, undergoes changes which cause it to bear a remarkable resemblance to Eberth's germ, and they conclude from their work that the latter is only a degenerative form of the former. If this be true, the stools of healthy persons, when they contaminate our drinking-water, may cause typhoid fever, and the existence of a preceding case is not necessary in order to cause the disease.

Chantemesse and Vidal³⁶ state that Eberth's germ will grow on gelatine containing 0.25 per cent. of carbolic acid, while all other water germs fail to grow on this medium. They therefore propose this as a method of isolating the typhoid bacillus. Such gelatine was prepared, and tubes inoculated with A, B, and Eberth's germ were poured on plates. A and B grew more vigorously on these plates than did the Eberth bacillus. The growth of the Eberth germ was markedly retarded, while that of the others was not.

Thonoit³⁷ modifies the above test by adding the carbolic acid to the water to be examined, and after a time inoculates ordinary gelatine with the water. He claims that after this treatment only the Eberth germ will develop. This test was applied to natural waters containing A and B, and their development was not affected. The water was allowed to stand three hours after the addition of the carbolic acid.

Holz³⁸ prepares a gelatine from the expressed juice of raw potatoes, and claims that the Eberth germ will grow on this medium while all similar organisms fail to do so. Such gelatine was prepared according to the directions given by Holz, and on this A and B grew beautifully.

I may remark, in conclusion, that it seems very strange to me, after going over the literature of the subject, that so much stress should have been placed, by the compilers of books, on the means of recognition of the so-called Eberth germ by

its behavior towards staining agents. This so-called characteristic is not warranted by the statements of the original investigators. It is true that Eberth makes this an important means of recognition, and states that the bacillus is not colored by Bismark-brown and hæmotoxyl n and only slightly by the other ordinary stains ; but what is the verdict of others on this point ? In the paper in which he calls attention to the fact that he had photographed the short bacillus before Eberth had published his discovery, Koch says : "Eberth states that these short bacilli have but slight tendency to take stains-The photograph here given shows to the contrary, that in taking stains these bacilli are but slightly inferior to other bacteria." Fischel stained easily with hæmotoxyl n. Coats³⁹ had no difficulty in staining with Bismark-brown. Gaffky states that the best color is methylen-blue, but that the bacilli are stained very well (*sehr gut*) with methyl-violet, gentian-violet, Bismark-brown, and fuchsin, and less well by hæmotoxyl n. Meyer⁴⁰ had the same difficulty in staining reported by Eberth ; but Friedlander⁴¹ found that the bacilli in the sections stained intensely (*intensiv*).

The conclusion which necessarily follows is that either the action of the organism towards stains is variable and dependent upon conditions, or the majority of the above-mentioned investigators were working with germs which were not identical with those of Eberth.

Since the above lecture was given, a very valuable paper on the "Variability and Varieties of the Typhoid Bacillus," by the distinguished bacteriologist, Babes,⁴² has appeared. This paper in part confirms and in part complements the work reported in the lecture. The chief points made in the paper may be stated as follows :

(1) The bacillus varies in size and appearance according to the culture medium on which, or the temperature at which, it is grown.

(2) Many of the saprophytic bacilli stain as feebly in Gram's method as the typhoid germ.

(3) The growth on potato is not always invisible, and many other organisms do form an invisible mould ; therefore this test is not characteristic.

(4) The flagella discovered by Löffler are not characteristic.

(5) There is nothing characteristic in the effects upon the lower animals. The "typhoid-like" bacilli are more highly pathogenic than the typhoid germ.

(6) He has found in a case of dysentery, in the body of a mouse dead from an unknown cause, and in water, germs which give all the so-called characteristic reactions.

(7) He has found in twelve *post mortems* of typhoid subjects germs, all of which respond to the characteristic tests, but which differ from one another.

(8) In all of these cases, with one exception, he finds what he calls the typical typhoid bacillus along with atypical ones. This "typical" germ is not identical with that obtained from the Berlin Hygienic Institute, and among its properties are two which other bacteriologists would deem sufficient to exclude it from any possible connection with the typhoid germ. These are : (1) it evolves a putrefactive odor*, and (2) old cultures on potato are brown.

(9) Although he has been unable to convert any of the varieties into the typical germ, he has seen the differences grow less marked. (We are not told what means he has used to influence the properties of the germs.)

(10) A very important statement is made by Babes, but upon this he lays no stress. It is that he has found some of the varieties outside the body (in drinking-water), but the typical germ has been found only in the bodies of those dead of typhoid fever. The facts given in the lecture will probably explain this statement.

(11) Animals inoculated with the atypical germs die more certainly (*i.e.*, a larger proportion of them die) and more rapidly than those inoculated with the typical germ. Exactly the same changes are found after death.

How Babes can conclude, after all this valuable work, that his typical germ is the sole cause of the disease, I cannot understand. It is true that he indirectly implies that other germs might cause the disease, because he constantly speaks of his typical germ as "the typical germ of our cases."

In again concluding, I wish to say that it seems very evident to me that the belief that typhoid fever is due at all times to one and the

*Neither A nor B give off any putrefactive odor, even after the pieces of spleen, described in the lecture, were kept in the incubator for two months.

same germ, which can be recognized to the exclusion of all others by certain peculiarities in staining or in manner of growth must be abandoned.

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KOCH'S TREATMENT OF TUBERCULOSIS.

BY PROF. R. RAMSAY WRIGHT.

Communicated from Berlin to the University of Toronto.

To-day's number of the *Deutsche Medicinische Wochenschrift* is memorable by containing the continuation of Koch's communication on a cure for tuberculosis. The substance of this article has no doubt been telegraphed already to the American papers, with the same detail as was that of the first article two months ago.

After premising that the experience of those who have tried the lymph confirms the statements made by him in regard to it, he describes

the observations which led to its discovery. He found that the local ulcer which characterises the point of inoculation in experimental tuberculosis followed a very different course in a healthy guinea-pig, inoculated for the first time, from that which it does in one already tuberculous. In the former, it does not heal; in the latter, it does; the tubercle bacilli not finding favorable conditions for propagating themselves. These unfavorable conditions—or, in other words, immunity—can also be produced in a healthy animal by the injection of cultures of dead tubercle bacilli. Such cultures, innocuous to a healthy animal, are, in the same quantities, *fatal* to a tuberculous one; while in very much reduced doses they lead to the destruction of tuberculous tissue, and to an eventual cure.

Thus was indicated the direction in which to work. The substance which exercises such a remarkable effect on tuberculous tissue was evidently one which could easily be separated from the dead bacilli. Experiments were made with extracts of pure cultures of the bacilli, treated with 40 to 50 per cent. glycerine, and the result was the now celebrated lymph.

From such extracts the active ingredient can be separated as a colorless dry substance, which is insoluble in absolute alcohol, is not destroyed at temperatures which destroy the toxalbumins, and diffuses easily through an organic membrane; but the use in practice of this concentrated substance has no advantage over that of the glycerin extract. The hypothesis formed by Koch as to the mode of action of the substance in question is essentially that indicated by Buchner, and cited in a previous letter.

It has the power of killing living protoplasm when sufficiently concentrated; the symptoms following injection in a healthy person result from this action—perhaps on the leucocytes; but the necrotising power has much more effect on tubercular tissue which is already half dead, and therefore leads to its complete death and to interference with the proper nutritive conditions of the bacilli if they remain shut up within the dead tissue. Thus indirectly the substance has also the power of affecting the bacilli, causing them to fall into heaps of coccus-like granules.

It is unnecessary to say that (with the exception of the use of glycerine to extract the cultures) all that Koch has disclosed was already

taken for granted. Without further details as to the culture medium employed and the methods adopted to secure a constant strength, it is regarded as quite certain that would-be imitators have a long and arduous research in front of them before they can venture to experiment on man. It is surmised that these details will be, in the meantime, withheld. Great preparations are being made for the manufacture of the lymph *en gros*. Special large incubating chambers, adapted to the peculiarities of the tubercle bacillus, are being made, and thousands of small Ertenmayer flasks have been ordered.

It was expected that Koch might make his announcement last night at the Medical Society, and that some one would take up the cudgels for the Koch cure in opposition to Virchow. The large hall was crowded in consequence, but comparatively little of interest took place. Guttman spoke of the favorable aspect of affairs at the Moabit, and undoubtedly made the greatest impression of the various speakers. Bernhard Frankel also defended the treatment and exhibited some cures, but Albert Frankel narrated a depressing case of a patient with apical affection on both sides, who in the course of treatment improved, as far as the lungs were concerned, but acquired a very serious tuberculosis of the tongue, which Frankel did not hesitate to describe as metastatic and subsequent to the treatment. Psaginsky brought forward a somewhat similar case of a young woman who had acquired lupus of the nose through using her consumptive sister's handkerchief. For nine years the affection has been distinctly local, but in the course of treatment tubercular patches appeared on the left tonsil.

It is obvious that such complications are not necessarily ascribable to the treatment, but the attitude of these speakers seemed to be unfavorable to Koch. Virchow had some more pathological material of the character which he exhibited last Wednesday, but he also showed the lungs of a case of catarrhal pneumonia of the same character as he had previously been inclined to attribute to the injection, but which had not been subjected to the Koch treatment, and so he was obliged to retire partly from his position in this respect.

I trust you have safely received the very large consignment of lymph which I sent you on

Saturday, and that as favorable results may be obtained with it in Toronto as here in the Moabit.

Selections.

TETANOID CONVULSIONS IN AN INFANT ; OPERATION : RECOVERY.

BY T. R. RONALDSON, M.B., F.R.C.P. ED.

The child whose case forms the subject of this short paper was born on the 8th of Dec., 1886. He was a strong, well-nourished boy, above the average size and weight, and the third child. The labor was easy and normal in every way. The umbilical cord was unusually thick, from an excessive development of Wharton's jelly. The mother nursed the child from the beginning, and continued to do so until the usual time for weaning, no other food than her milk being required.

After my visit on the 17th December, when the child was nine days old, it was first noticed that there was something wrong. At times the child cried and was restless, the left eye winked, the muscles of the left side of the face twitched, and the left side of the tongue swelled up in marked contradistinction to the right side. These symptoms began so slightly and progressed so quietly that the mother and a most intelligent nurse did not think it necessary to send for me until the 19th, two days after the onset of the illness. Having been informed of the nature of the attack, and knowing how healthy the child was, and that the umbilical cord had not separated although it was the eleventh day, I began to connect the case, as I proceeded to pay my visit, with those cases of tetanus we read of as occurring so frequently among babies in certain countries, and whose starting-point seems to be the neglected or badly taken care of umbilicus. On my visit I saw no reason to doubt the accuracy of the opinion I had formed. The child was, apart from the convulsions, the picture of health, well-nourished, with digestion perfect, and with evacuations natural in quantity, quality, and frequency. The anterior fontanelle was somewhat fuller than usual, but that was entirely from the exceptionally well-nourished condition of the child. The temperature was normal,

and continued so throughout the attack. The only unusual condition, apart from the convulsions, was that the cord had not separated. There was a distinct putrefactive odor about the black dried-up cord, but to the eye there was nothing to be seen but the usual amount of ulceration at the line of demarcation between the dead cord and the umbilicus. There was no inflammatory blush, no swelling, no unusual tenderness. At that time the convulsions were confined to the left side of the body; restlessness led on to tonic muscular contraction, and that was succeeded by well-marked clonic convulsions. Between the attacks the child was apparently perfectly well. I gave it as my opinion that probably the convulsions arose from some irritative condition at the umbilicus. I completed the separation of the cord with scissors, gave directions that the stump should be carefully washed at intervals with corrosive sublimate solution, and dressed with zinc ointment; ordered one drop of chlorodyne to be given, mixed with water in six doses, and expressed the hope that, as the supposed irritation at the umbilicus subsided, the fits would disappear. The fits got worse; bromide of potassium and chloral, given up to 6 grains of the bromide and 4 grains of the chloral in twenty-four hours, had no effect whatever in subduing their number or severity; nor had chlorodyne, two minims every four hours; nor had the local application of a 20 per cent. solution of cocaine, nor of the solid nitrate of silver. Warm baths increased them; they extended soon to the right side, and ultimately the whole muscular system was involved. They increased to the number of 204 in the twenty-four hours, while during their occurrence the tongue became blue-black, and at times well-marked opisthotonos supervened. On one occasion they were never absent for one whole hour, and for a period of nine hours the child was unable to suck in consequence of the frequency of the fits. When they did not come too rapidly, it took its nourishment greedily. They varied in frequency—from about 100 to 204 in the twenty-four hours—but not in such a way as to warrant us in believing that treatment, local or general, had any beneficial effect.

Dr. Brakenridge, who saw the child with me on 24th December and subsequently, confirmed

my diagnosis, giving it as his opinion that the convulsions were not due to any disease originating in the brain, but that they were peripheral in their origin, and probably had the umbilicus as their starting-point. Meanwhile, in spite of treatment, and although the navel stump was quickly and nicely healing, the fits went on, and we asked Mr. Chiene to see the child with us on 27th Dec., to help us in deciding whether the umbilicus should be excised. Mr. Chiene's opinion was that the condition was allied to tetanus, and he advised that before excision was had recourse to, a more thorough antiseptic treatment of the stump should be tried. All medicine was stopped, and iodoform was carefully and diligently applied to the navel stump.

As no improvement resulted, chloride of zinc solution (40 grs. to ʒj.) was thoroughly applied on the 29th. That failing to benefit, Mr. Chiene consented to excision, and on the 31st December, I freely and deeply excised the umbilicus. At once there was improvement, and the fits gradually decreased.

From 8 a.m., 4th January, 1887, until the evening of the 5th, they entirely ceased, but on that evening they again began. Next day I removed three silkworm-gut stitches, which were apparently a source of irritation, as evidenced by an inflammatory blush around them, and the fits again began to decrease in number and severity, while at the same time the child slept more and took its nourishment better. As they did not entirely disappear, and as the nurse had previously noticed that the child was distinctly better at the time when, after the employment of carbolic lotion during the operation, the urine was dark colored, Dr. Brakenridge suggested the use of sulpho-carbolate of soda internally. On the 13th January it was prescribed in 4-grain doses every two hours. That quantity, however, upset the digestion, and the fits increased. The dose was accordingly diminished. The fits again disappeared, but they began to return mildly on the 25th January, and reached the number of twenty-three in the twenty-four hours on the 27th January, and sulpho-carbolate, which had been given up, was again administered. On this occasion the fits were not so severe, and after the first day or two they only affected the eyes and mouth, gradually becoming slighter. On the 30th January one slight fit ended the

attack, and the child has been in good health ever since. Dr. Edington, who kindly examined the excised umbilicus, did not detect any special organism in it.

In remarking on this case I do not feel that I am called upon to try to decide the question whether the convulsive motor phenomena were due simply to peripheral irritation, or to the presence of some specific organism which had gained entrance at the umbilicus. I must leave the decision of that question to pathological experts.

As to the effect of treatment, my friend Dr. Brakenridge, if I remember rightly, was of opinion that, in the first place, absorption of carbolic acid by the wound; and, in the second place, the administration of sulpho-carbolate of soda internally, had a distinct influence for good. My own opinion was that the excision of the umbilicus determined the recovery.

And, lastly, the occurrence of such a case raises the question whether the umbilical cord of newly-born children should not be treated more carefully than it generally is. I am in the habit now of tying the cord with thread soaked in corrosive sublimate solution, of directing the nurse to wash the cord with the same, then to surround it with sublimated wool, and to bathe it with the solution from time to time should any putrefactive odor occur.—*Edin. Med. Jour.*

THE
Canadian Practitioner

A SEMI-MONTHLY REVIEW OF THE PROGRESS
OF THE MEDICAL SCIENCES.

Contributions of various descriptions are invited. We shall be glad to receive from our friends everywhere current medical news of general interest.

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TORONTO, FEBRUARY 16, 1891.

MEDICAL SOCIETY OF THE STATE
OF NEW YORK.

The annual meeting of this society for this year was held at Albany, on Feb. 2, 3, and 4. It is one of the oldest medical associations on this continent, this being the eighty-fifth annual

meeting. Its organization is complete in all respects. Representatives from the various county societies are regularly elected, and generally make it a point to attend the meetings. Certain powers are given to the society by the State Legislature, corresponding to a certain extent with those given to the Ontario Medical Council by our Government.

The society is showing a commendable desire to raise the standard of medical education in New York, and through its influence an act has been passed by the State Legislature authorizing the establishment of a central examining board. This meets with a certain amount of opposition, which causes considerable trouble in properly carrying out the details. It will probably be some time before their methods of conducting examinations for a license to practise are equal to our own.

The meeting this year, under the Presidency of Dr. W. Warren Potter, was highly successful. In comparing it with the meetings of the Ontario Medical Association, we noticed that the proceedings were pushed along at almost railroad speed. For instance, the first morning session commenced at 9.15 o'clock, and its programme included the President's inaugural address, appointments of committees, executive business, and eleven papers. It will probably be a surprise to members of Canadian associations, who see practically nothing done on the first mornings, to learn that this programme was completed. The list of papers was a long one, and the committee decided to limit the time to ten minutes for ordinary members, and fifteen minutes for visiting members. The papers were good, but the discussions were limited in number and rather poor in quality, with two or three exceptions. It seemed somewhat remarkable that they should attempt so much work without dividing into sections.

All questions referring to matters not connected with the reading of papers were referred to committees, and much valuable time was saved thereby. They have an annual banquet which is managed in a very sensible way. It costs one dollar per head, and each member pays for his own ticket. It commences at the end of the evening session, at 9.30. The bill of fare includes a light meal and a glass of claret. Apart from this, the members may order as they

please. The after-dinner speeches on this occasion were remarkably good, being, as a rule, bright, witty, and brief. Among those who shone in this particular line were the genial and popular President; two of the most gifted sons of the sunny south, Dr. McMurtry, of Louisville, Kentucky, and Dr. Davis, of Birmingham, Alabama; Dr. St. John Roosa, of New York; Dr. Reed, of Cincinnati; and representatives of the local clergy.

The members extended to their guests a generous hospitality. The visitors from Canada, Drs. J. F. W. Ross and A. H. Wright, of Toronto, were entertained in royal style by the President and the good people of Albany.

FORCIBLE DILATATION OF THE CERVIX UTERI.

We had occasion to refer to the dangers connected with this operation a short time ago, in commenting on a paper written by Dr. Joseph Price, of Philadelphia, on the subject of minor gynecological operations. We have recently received from Dr. Howard Kelly, of Johns Hopkins University, an article reprinted from the *American Journal of Obstetrics*, which gives a report of a death caused by a uterine dilator. Dr. Kelly was called in consultation to see a case in which a uterine dilator had been introduced four days before. The patient had all the symptoms of septic peritonitis. A laparotomy was proposed, but the attending physician objected, because he thought the patient's condition could not be very serious, as she was suffering so little pain.

Five days afterwards the abdomen was opened, when the prospects of recovery were almost *nil*. Fluid gushed out when the opening was made—about one quart of foetid bloody serum mixed with flakes of lymph. There was extensive peritonitis, involving all of the smaller intestines. Death ensued in a few hours. The uterus was found slightly enlarged from a pregnancy in the third month; it was acutely anteflexed, and the dilator had been pushed through the posterior wall of the cervix into the peritoneal cavity.

Dr. Kelly says, in commenting on this case: "He who runs may read. The lesson taught by this awful tragedy is a plain one. The first

objection is manifestly not against dilators in general, but against those dilators which end in a sharp point, and are strongly curved in the dilating blades. The liability to perforate an anteflexed uterus with such an instrument is so manifest, that I cannot help thinking that this accident has occurred more than once. A further objection is against the manner of inserting the dilator—*force should never be employed.*"

The doctor then goes on to describe the proper methods of using this modern abomination. His directions are good in a way, but we think that suffering humanity would lose but little if the dilator were calmly consigned to oblivion. In skilled hands the instrument may do good in a few cases, but the dangers connected with its use are so serious at all times, that the novice should consider that he is assuming an awful responsibility in employing it in any case.

KOCH'S LYMPH IN TORONTO.

Experiments with Koch's lymph, in the treatment of tuberculosis, are being conducted with much care in the General Hospital, and in the Hospital for Sick Children, in Toronto. The results will be published shortly, but are not very pronounced up to the present time, excepting in a limited number of cases. In a general way we may say that lupus, when not too far advanced, is much benefited by the injections. The prospects are fairly good in the treatment of tubercular glands and joints. Pulmonary phthisis appears to be the least amenable to the treatment, but it is hoped that a few cases, where the disease is in the first, or early part of the second stage, are showing gradual but certain signs of improvement.

We are pleased to know that great care is being exercised in the proper selection of cases, and extreme caution is being observed in the dosage, especially where the lungs are involved. It will require months to fully investigate the results, but we feel confident that they will prove favorable in certain varieties of tuberculosis. The authorities of the University of Toronto are still sending supplies to various parts of the province, but the use of the lymph is still confined exclusively to hospital practice.

Meeting of Medical Societies.

THE TORONTO MEDICAL SOCIETY.

Jan. 8, 1891.

The President, Dr. Spencer, in the chair.

Dr. Grasset exhibited a series of specimens of

VESICAL CALCULI.

Seven large faceted calculi had been removed from a male patient, *æt.* 51, by the operation of lateral lithotomy. The man had suffered from symptoms of vesical calculus for fourteen years, but not until latterly, when the pain and discomfort became almost unbearable, would he consent to an operation. The aggregate weight of the seven calculi was just short of half a pound.

KOCH'S TREATMENT FOR TUBERCULOSIS.

Dr. Bingham made a communication to the society, giving an account of the impressions he had formed whilst studying the cases under Koch's treatment in the New York hospitals. Seventy-five per cent. of these were lupus cases. He described the symptoms which indicate the reaction after injection, and then proceeded to refer to the effect of treatment in the various manifestations of tubercular disease. In phthisis, all the cases seen were incipient; the swelling which occurs as a local effect of the injection causes great dyspnoea in laryngeal phthisis and almost makes tracheotomy necessary, but it soon subsides, and diminishes at each subsequent injection. The reaction is more marked in lupus than in other cases of tubercular disease. A case of tubercular prostate, which was complicated with a supra-pubic fistula, was much improved under treatment, and after eight injections the urine passed almost entirely by the urethra. The remedy was tried in a case of leprosy; after the fourth injection, the face assumed a shade two degrees lighter than formerly; the trunk and extremities became marbled, in consequence of patches of healthy tissue occurring among the leprosy. In a case of doubt in the diagnosis between lupus and tertiary syphilis, a typical reaction was obtained.

Dr. Sheard referred to the indications for and against the employment of the remedy, and condemned the practice of submitting patients far advanced in phthisis to the treatment. He then referred to two cases under treatment in

the General Hospital. In the case of incipient phthisis, the sputum was not increased in amount; it was not examined for bacilli. Before injection there were rales to be heard at the right apex, and there only; twenty hours after injection, rales were developed all over the left lung; at the right apex were coarse crepitations and tubular breath sounds. In the second case, that of lupus of twenty years standing, there is a marked reaction, consisting of swelling and injection of the cicatricial tissue; since the injection, the highest temperature has been 99.6 F. In speculating as to the nature of Koch's fluid, Dr. Sheard stated that from a study of the symptoms presented during reaction, one would be inclined to look upon the effect as an induced form of septicæmia.

Dr. Price Brown called the attention of the society to the efficacy of chloride of gold in the treatment of phthisis. This method has recently been suggested by Messrs. Gibbs and Shurly of Detroit.

Jan. 15.

The President, Dr. Spencer, in the chair.

Dr. Oldright presented a specimen of

(a) PERFORATION OF THE INTESTINE OF TRAUMATIC ORIGIN.

The man was said to have been kicked on the belly; he walked home after the injury, and subsequently developed symptoms of peritonitis; an operation was thought advisable, but he suddenly became collapsed and died thirty-six hours after receiving the injury. A *post mortem* examination revealed injection of the peritoneum, intestines glued together, deposits of lymph and a large quantity of pus in the peritoneal cavity. On the ileum, six or seven inches from the cæcum, was found a rupture seven-eighths of an inch in length on the anterior aspect of the gut. A second specimen was presented by Dr. Oldright,

(b) PORTION OF A NEEDLE REMOVED FROM THE KNEE-JOINT.

A child, four years old, drove a darning-needle into her knee by falling upon it. After cutting down upon it, the needle was found to have been driven into the upper portion of the tibia, and in the efforts to extract it a portion was left behind embedded in the bone.

(c) MILKY FLUID FROM A CYST OVER THE OLECRANON.

Dr. Oldright removed this from a patient suffering from rheumatism. The nature of the fluid had not been determined. A precipitate is thrown down on heating, which is dissolved by adding nitric acid. Under the microscope, there were no pus cells found.

Dr. Atherton referred to the case of a man who received a kick on the abdomen, causing a rupture of the duodenum. He walked half a mile after the accident, and lived thirty-six hours.

Dr. Peters asked how it was that rupture could occur in the ileum as the result of a blow on the abdomen. There is no portion of the bowel more freely movable than the ileum, and its contents are usually of a fluid character. If the injury had been severe and great pressure had caused compression of the bowel against some resisting surface of bone, then the cause of rupture would be evident.

Dr. McPhedran narrated the history of a man who received a kick on the abdomen; there were no external marks of violence; he lived from thirty-six to forty-eight hours. Blood was found extravasated about the head of the pancreas, and a rent in the peritoneum at that locality permitted blood to be effused into the general peritoneal cavity. The intestines and stomach were bruised.

Dr. Oldright, in reply, suggested that the rupture of the intestine may have been due to a knuckle of bowel becoming constricted, on receiving the injury, the contents of the gut imprisoned, and the force continuing to act, would cause the wall of the intestine to give way.

Dr. Powell exhibited a patient affected with lupus who is at present under treatment in the Toronto General Hospital. Four injections have been given; there is evident softening of the cicatricial tissue.

Dr. A. A. Macdonald presented

A SPECIMEN OF FETUS

dead at the third month carried until the fifth month. Some years ago the woman came to him with a history of several miscarriages; she had a uterus large and flabby, and she suffered from menorrhagia. She improved under treatment, became pregnant, and carried a child to full term. About five months ago the menses

ceased and she thought herself pregnant, but she did not increase in size. Three months after she suffered from a slight discharge; this continued, and at the fifth month pain occurred and she sent for Dr. Macdonald, who, on examination, found the foetus lying in the vagina; there was no placenta; the cord was twisted twice around the neck of the foetus. In this case probably there was degeneration of the placenta and death of the foetus in consequence; the placenta probably came away in fragments. The patient made a good recovery.

FLESHY MOLES.

Dr. Macdonald also presented two specimens of blighted ova, one at the second month, the other of three months growth. These moles he looked upon as the changed product of conception. The patients were of poor, weakly constitutions, and the debilitated state of the individual is probably accountable for the condition.

Drs. Baines, Dame, Atherton and Spencer discussed the cases, and Dr. Macdonald replied.

Dr. Dame then showed specimens of

HORNY GROWTHS ON THE PENIS.

These had occurred in a man who had been circumcised. A warty substance occurred six years ago, from which the two horns developed; he pared it regularly. Dr. Dame amputated a portion of the penis with the growths. The horns were situated on the under surface near the glans; the longer was two inches in length.

Dr. Primrose referred to a case of the kind which he had seen under the care of Mr. Pearce Gould. A warty growth had appeared on the margin of an ulcer which existed in the line of an old circumcision wound; a horn developed in front of this. The penis was amputated and several indurated glands in the groin were excised. The ulcer proved to be epitheliomatous in character. Jewitt reports a case in which a horn three inches in length existed. According to Gould, horns may grow (1) from the interior of sebaceous cysts; (2) from the matrix of the nail; (3) from warty growths.

Jan. 22.

The President, Dr. Spencer, in the chair.

Dr. J. D. Thorburn read a paper on

KOCH'S TREATMENT FOR TUBERCULOSIS.

He gave an account of his experience whilst observing the effects of treatment in the Berlin hospitals. He referred to the work which is at

present being done in Koch's laboratory by Professor R. Ramsay Wright. The almost universal distribution of tubercular disease in all parts of the civilized globe was graphically demonstrated by Dr. Thorburn on a map of the world, specially prepared for the purpose. The reaction following the injection of Koch's fluid was described and local effects minutely detailed. Von Bergmann strongly advises that the lungs should be carefully examined in a case of lupus which is to be submitted to treatment. An instance was cited in which a patient was treated for lupus; pulmonary disease was not suspected; a marked reaction, however, subsequently evidenced extensive lung trouble. The necessity of operating, especially in joint disease, is insisted on by some for the removal of necrotic tissue. One of three things may happen if operation be not undertaken: (1) the necrotic tissue may be absorbed and the bacilli carried to other parts of the body; (2) the skin and superficial tissues may slough, and the necrotic tissue may in this way be cast off; (3) if not absorbed nor cast off, it may remain as a cheesy mass, and poison may be absorbed from it.

Dr. Palmer asked if the swelling were alarming or dangerous in the local reaction in the larynx where neither tracheotomy nor intubation had been performed. The swelling, though great, is said by some to be peculiar and sometimes causes an unfolding of the larynx, thus relieving sténosis.

Dr. McPhedran stated that there was a good deal of reaction of opinion in some localities against the Koch treatment. In London, for instance, some of the physicians are very skeptical. The treatment certainly does cause improvement in some cases. At present, Dr. McPhedran would not urge any tubercular patient who is doing well and gaining ground to submit to Koch's treatment. It is a question whether or not the dangers are not greater than we suspect; certain cases of death in early phthisis have been recorded. If we have cases of phthisis in which no improvement is going on, then we are justified in recommending the treatment. The cases at present under Dr. McPhedran's care in the hospital were then briefly referred to.

Dr. Burns and Dr. Price Brown also took part in the discussion, and Dr. Thorburn replied.

GYNECOLOGICAL AND OBSTETRICAL SOCIETY OF BALTIMORE, MD.

December meeting.

Vice-President Dr. Charles H. Riley in the chair.

Dr. Wm. E. Moseby related the following case:

Mrs. Maggie G., a light-colored woman, about thirty years of age, twice married; had had two children by her first husband. Had suffered much during the past twelve years from dysmenorrhœa; had been unable to do ordinary work.

Examination showed the uterus to be retroflexed and firmly bound down, but the character of the adhesions could not be definitely made out. There was an irregular-shaped elastic mass in the position of either tube diagnosed as cystic ovaries, together with chronically inflamed tubes. All the pelvic tissues were very sensitive to pressure. There was a deep, double laceration of the cervix and a lacerated perineum, with very lax vaginal wall, but only slight rectocele.

When the abdomen was opened the mass on either side of the pelvis was found to be composed of a cystic ovary and the corresponding tube firmly matted together by old organized adhesions, each mass being firmly bound down to the pelvic wall by numerous strong and many more recent adhesions. There were also adhesions to the omentum. The left ovary ruptured before it could be removed. The mass in the right side appeared to be a large hematosalpinx, but examination showed it to be an ovarian cyst into which blood had entered from a ruptured Graafian follicle. The adhesions behind the uterus were very broad, strong bands, and were pulled off the uterine wall. All possible care was used to secure the patient against hemorrhage, and the abdomen was douched out with hot-boiled water until the return flow was practically colorless. A perforated glass drainage-tube was introduced to the bottom of the *cul de sac*, and the incision closed about it. The extreme difficulty of separating the adhesions and the douching prolonged the operation to about an hour and a half.

Although stimulants and artificial heat were freely used no reaction could be obtained, the temperature never reaching 95°, and the patient died about six hours after the operation,

apparently from shock. At no time was there any discharge of blood or over-bloody fluid from the drainage-tube. Dr. N. G. Keirle, however, kindly examined the pelvic cavity *post mortem*, and reported that death was due to hemorrhage, the exact source of which could not be made out. Dr. J. Whitridge Williams kindly furnished the pathological report, which is given below.

Dr. Thomas Opie exhibited a placenta that he had gotten a few hours before the meeting from

A CASE OF PLACENTA PRÆVIA.

The patient was thirty-five years of age, and had borne one child previously. When he saw her first she was blanched and exsanguined. The blood flow began three days before with a loss of a quart, and continued with more or less rapidity up to the time of operation. Her confinement was not expected for two weeks. When first seen by him there were some rythmical pains and some dilatation. The cervix was dilated with the fingers and cone of the hand; the placenta was detached with a sweep of the fore-finger around the cervix, the bag of waters was artificially ruptured and traction-rod forceps applied. The child was delivered in fifteen minutes without further loss of blood, the placenta coming away simultaneously with the birth of the child. Though the position was occiput posterior, there was no laceration of the perineum, and the child was unscathed. Both mother and child were doing well.

Dr. Opie also exhibited a

SPECIMEN OF AN OVARIAN TUMOR

which he had recently removed. The tumor had developed into the epigastric region, and the abdomen was about as large as it would have been at the full term of pregnancy. It took two hours to break up the adhesions, which were very dense between the tumor and the intestines, and between the tumor and the omentum. The second tumor was taken from the pelvis. It was ovoidal in form, about seven inches in length by five inches high, and four inches thick. It was removed entire, and upon section it proved to be a dermoid growth. There was no history of peritonitis to account for the extensive adhesions. The patient had never had a day's discomfort other than from the size of the cyst. She did not know until

four months ago that she had a tumor. The material in the large cyst was colloid. Notwithstanding the extensive adhesions, the length of time consumed in breaking them up, and the injury resulting from the operation, the patient has made a good recovery, this being the seventeenth day after the operation.

Dr. Howard A. Kelly: The term colloid is often used in two senses. An incorrect use, describing the yellowish, more or less opalescent, thick, viscid material often found in ovarian cysts; it is employed in such cases as more or less synonymous with gluey. The other use of the term is to describe a rare condition, in which the contents of the cyst are more like calf's-foot jelly, and have a vitreous fracture; they are with great difficulty removed, clinging to everything. This latter is true colloid, and when found such tumors are of a suspiciously malignant character. We should limit the use of the word to the latter condition.

I wish to refer to two minor matters of interest suggested by this specimen of placenta prævia. The position which the placenta has occupied in the uterus can accurately be determined by the position of the opening in the membranes made by the passage of the child, inasmuch as the fundus uteri must of necessity be just opposite to this perforation. We can, therefore, by reconstructing the membranes, see just in what part of the uterus the placenta lay. In one of my placenta prævia cases there was no hole at all in the membrane, as I had extracted the dead child through a perforation in the placenta. We can do still more than this in the way of a diagnosis with the membranes. By allowing them to be expelled untouched into the bed and carefully noticing their exact position, we can tell as well on which side of the uterus the placenta was attached.

The second point is, that we may have placenta prævia hemorrhage without being able to detect a placental margin, owing to a low attachment of part of the placenta near the internal os below the contraction ring, but not over the whole of the cervical canal. The lower part of a placenta thus attached is separated by the opening up of the lower uterine segment.

Dr. L. E. Neale said: Although Dr. Kelly had alluded to a point of some interest, it is of far more practical importance to recognize

placenta prævia prior to the expulsion, and as far as he knew this could only be done with certainty by digital examination. Partial placental separation and rupture of the membranes during labor in cases of placenta prævia was outlined by Maricean as early as 1668, but was fully described by Puzos in 1759. He saw nothing in the history of the present case as related by Dr. Opie that contra-indicated the method of Broxton Hicks, a method that up to the present time had given by far the best results, viz., $4\frac{1}{2}$ per cent. maternal mortality. If this method when practicable could be performed earlier than delivery by any other method, and was not difficult and gave the best results, why not have applied it in the present case?

Dr. Wilmer Brinton asked why Dr. Opie objected to the tampon in cases of placenta prævia; he thought no arbitrary law could be applied.

Dr. Opie said, in closing the discussion, that results of operative procedure depended largely upon the skill and familiarity of the operator with the special operation resorted to; in his first case of placenta prævia he had attended, he had turned and lost both mother and child; with rapid dilatation and forceps he feels that he has command of the situation, and having resorted to that method repeatedly, had gained greater skill and done better work. While Dr. Neaie might do better by some other method, he is fully satisfied that he does best himself with the forceps; he is opposed to the use of the tampon because it conceals what is going on; it is best not to wait for pains; he is in favor of rapid dilatation and delivery in placenta prævia, in puerperal eclampsia, and in abortion; to put in a tampon and go away is hazardous; the tampon is of very little help in hemorrhage.

Dr. Kelly read a paper upon

THE EXAMINATION OF THE NORMAL PELVIC
VISCERA,

describing various carcinoma and trimanual methods of palpating the normal ovary.

Dr. Wm. P. Chunod: When speaking of what should be found or can be found at an examination, it is necessary to consider the circumstances under which the examination is made. Office examinations are the most usual, and all the facilities are not usually at our command, and this circumstance should be specified

and taken into account. Certain advantages in methods give certain advantages in results. Of course where the woman has no ovaries, or where the ovaries are not in the pelvic cavity, they cannot be palpated.

Dr. Hunter Robb: I thoroughly agree with Dr. Kelly that the normal ovary can always be palpated under an anesthetic, and also that in a large number of patients the ovary can be outlined without anesthesia. Four years ago Dr. Kelly taught me the method of examining the ovary by invaginating the perineum, and I can testify to its utility. This lengthens out the examiner's finger, and thus enables the practitioner who has a short finger to accomplish it with almost the same facility as a longer one. The corrugated tenaculum, devised by Dr. Kelly, may be used to advantage with nulliparous patients to define the uterus and its appendages still further. No one, of course, would think of using it in inflammatory conditions of the pelvic cavity.

Dr. B. B. Browne said that he had listened with much pleasure to Dr. Kelly's paper, and congratulated him upon the admirable manner in which he had systematized these valuable methods of pelvic examination—methods which most of us had been using in our gynecological practice for several years. He generally preferred the use of two fingers in the vagina, as he could thus make a more satisfactory examination of the tubes and ovaries than with one finger; in many cases a more accurate idea of the adhesions can be had by getting the finger above the ovary and fixing it between the finger and the spinal column; pulling down the uterus aids diagnosis very much.

Dr. Opie said that there were few objections to Dr. Kelly's paper, but it seemed that the elbow on the hip is incompatible with delicacy of touch; the law as expressed by Martin being—"The more lightly the parts are touched the easier the goal is reached, and the less force that is employed the more distinctly things are felt." He thinks it a cruel sort of thing to drag an organ out of its position, and would like to know how much displacement can be made with the tenaculum without producing dangerous trouble; for example, cellulitis, metritis, and injuries to the peri-uterine tissue; he had met a number of cases in which he had not been able

to make out the ovaries; Dr. A. Martin says he can palpate normal tubes, but Dr. Opie has never been able to reach that degree of perfection.

Dr. Neale referred to the possibility of tracing out the ureters through the anterior vaginal wall, as had been demonstrated to him by Dr. Kelly at the Hopkins Hospital clinic; he had no doubt that in a large majority of cases the normal ovary could be displaced out of its normal position and palpated or touched with ease through the vaginal walls; he believed that a great deal of difficulty in an ordinary gynecological examination was due to the fact of neglecting to empty the bladder or to employ the rectal touch.

Dr. H. P. C. Wilson said there were a large number of women in whom he was sure he could not palpate the ovaries, and he was doubtful if any one could do so. The uterus is often found fixed in the pelvis as in a mass of putty, and no definite outlines can be made out; in other cases the abdominal walls are from two to four inches thick with fat, and in such cases he had failed to find the ovaries.

Dr. J. Whitridge Williams said that he could certainly feel the ovaries in four cases out of five, and that he had succeeded occasionally in finding the ureter.

Dr. Moseby: The old teaching is that the ovaries can not be palpated in their normal position. When an ovary can be found by an ordinary examination, its location may fairly be considered as abnormal. If Dr. Kelly's idea, that all men who cannot make out normal ovaries should be thrown out of the specialty, should be enforced, a large number of experienced and thoroughly informed specialists would be excluded from practice. It is practically impossible to examine every patient thoroughly enough to make out the normal ovaries in office examinations. In dispensary and more especially in hospital practice, the case is very different.

Dr. Browne thinks that the cases in which the ovaries can not be felt are the abnormal cases; if the symptoms point to an examination of the ovaries they can be made out, but if necessary an anesthetic should be given.

Dr. Kelly, in closing the discussion, said that he examines every case coming to him, vulva to

ovaries, making a special note of every important organ. When the patient complains of persistent pelvic pain the examination is never considered complete, or the diagnosis sure, without a special note as to the condition of the ovaries. I have been asked about examining the ureters by palpation. They can be felt in almost all cases, being distinctly traced from the anterior part of the pelvic back to the side of the uterus. Pressing upon a diseased ureter causes a desire to pass water, often irresistible. I prove that this structure is a ureter by catheterizing it. The catheter can be felt through the vaginal wall outside the bladder in the ureter, and the urine collected as it comes down from the kidney drop by drop. The Fallopian tube can often but not always be made out.

The amount of displacement of the uterus which can be made without injury is considerable. In normal cases it can easily and without harm or pain be brought down to the vaginal outlet. When there is fixation, gentle traction can be made until pain is felt. In these cases I use traction with the corrugated tenaculum, and then pushing up the fundus with the finger practise massage, stretching the adhesions. I am sure that the downward traction to the vulva without pain never does any harm.

Dr. J. Whitridge Williams' remarks upon

THE PATHOLOGICAL SPECIMENS

submitted to him by Dr. Moseby, Dr. Wilson and Dr. Opie.

The specimens submitted by Dr. Moseby are of considerable interest, and consist of the uterine appendages from both sides. The specimen from the left side consists of the Fallopian tube, ovary, and part of the broad ligament. The tube was completely occluded at its fimbriated end, but otherwise presenting nothing abnormal, except numerous small adhesions. It contained a very small amount of dirty yellow fluid, consisting of columnar ciliated epithelial cells and numerous disintegrated cells. The ovary was considerably torn and covered by very dense adhesions, while the broad ligament presented nothing of note. The specimen from the right side was an irregular mass of tissue about $5 \times 4 \times 1\frac{1}{2}$ cm., consisting of the tube and ovary imbedded in dense adhesions. At first glance the mass appeared to be composed of two parts, a large solid anterior portion

covered by dense adhesions, and posterior to it a cystic structure about $4 \times 1\frac{1}{2}$ cm. in size. This had a bluish color, thin wall, and was intimately connected with the rest of the mass. Imbedded in adhesions a piece of the ampullar end of the tube was found, which could be traced for about 4 cm., and then lost itself in the mass, and appeared to have no connection with the above mentioned cystic portion. The main portion of the mass or section was shown to be composed of ovarian tissue, which was covered and completely hidden from view by very dense adhesions. It contained two tolerably fresh corpora lutea about $1\frac{1}{2}$ cm. in diameter. The larger of these corpora lutea communicated by a small opening with the cystic portion above mentioned, which contained a thin, reddish, watery fluid containing blood cells. On cutting open this cystic portion its walls were found perfectly smooth, with several smaller cysts projecting into it. These varied in size up to 2 cm. in diameter, and were filled with a clear watery fluid, and arose directly from the ovarian tissue. On examining the scraping from the walls of these cysts, I found that they were lined by a layer of almost flat cuboidal cells, which were distinctly ciliated. These cysts could not have originated in the tube, as was readily demonstrated by their arrangement in relation to the larger cyst, and by the living epithelium, which was totally different from that of the tube. Their smooth interior precluded the idea of a ciliated papillary cystoma; and the only probable thing for them to be were dropsical Graafian follicles, which had been prevented from rupturing by the dense adhesions covering them, and so obtained their large size. The fact that they were lined by ciliated epithelium is not at all opposed to this supposition; for cilia have previously been found in the dropsical Graafian follicle, as was shown by Von Velits, of Budapest, about a year ago; and as I found altogether independently of him last spring. But as yet I have not made a sufficient number of observations to assert that all dropsical follicles are lined by ciliated epithelium. The blood in the large cyst in all probability came from the corpus luteum with which it was connected. The adhesions about the ovary were particularly dense and resisting. The diagnosis from the specimen is pelvic peritonitis, with adhesions binding down the

adnexa on both sides, particularly the right side, with several very large dropsical Graafian follicles.

The specimen submitted to me by Dr. H. P. C. Wilson was a small myoma about 3 cm. in diameter, and bore on one surface a piece of vaginal mucous membrane, the size of a two-cent piece. The tumor was submitted to me to decide whether its origin was from the anterior fornix or from the uterus itself. Sections made through the tumor and the vaginal mucous membrane readily showed it to be a myoma, which was separated from the sub-mucous tissue and epithelium by numerous bands of non-striated muscular tissue. From the presence of muscular fibres between the tumor and epithelium, I think we are justified in concluding that it was not of vaginal origin. Were it of vaginal origin, it should arise from the sub-mucous tissue and be immediately adjacent to the epithelium and not separated from it, as it was in this case, by muscular tissue. Force is lent to this conclusion by the fact that vaginal fibroids are very rare indeed, and many of the reported cases, especially fibroids from the anterior fornix, had their origin in the anterior wall of the uterus instead of the vagina.

The specimen submitted by Dr. Opie was a greatly hypertrophied posterior lip of the cervix, which measured 5 cm. in length, and 2 cm. at its broadest part. Microscopically it was found to consist of almost normal cervical tissue, with only a very slight increase of the connective tissue. Except at its cut surface, the entire mass was covered with the usual stratified epithelium.

Generally speaking, we may distinguish two forms of hypertrophy of the portio-vaginalis—follicular and diffuse or simple hypertrophy. The first form is due to an increase in the number and size of the cervical glands, with frequent retention of their contents, and is quite frequent but never attains a very great size, and is readily distinguished by its nodular appearance. The diffuse or simple form of hypertrophy is far more important. In this there is a general increase in all the elements that compose the cervix, though there may be a slight increase in the amount of connective tissue, as there was in this case.

Dr. Howard A. Kelly read a paper upon the palpation of

THE NORMAL UTERINE APPENDAGES

(published in full in the February number of the *American Journal of Obstetrics*). He stated that the normal uterine appendages could always be palpated. There are two avenues of approach, by the vagina and by the rectum, and three ways of utilizing these avenues. First, with one hand; second, with two hands employed bimanually, either by vagina or rectum; and third, the trimanual method, by vagina and by rectum.

First, the examination with one hand is unsatisfactory, and the ovary cannot even be felt unless abnormally displaced downward into the recto-uterine pouch. Second, the success of the bimanual examination depends upon the downward pressure with the external hand displacing the abdominal walls in the direction of the ovary to be palpated, and thus affording a resistant plane against which the ovary can be felt by the internal hand. The internal hand must be used to invaginate the perineum, which is thus displaced upward into the pelvis. This invagination gives the examining finger, even though it be a short one, the necessary length. One, often even two inches, are thus gained to the palpating finger. Care must be taken in making the pressure necessary to produce this invagination not to stiffen all the muscles of the forearm, thus impairing the tactile sense.

The rectum is, of all others, the best avenue for approaching the structures lateral to the uterus, affording as it does a wide open channel throughout the whole length of the pelvis.

Where the structures cannot be reached at once through the rectum, they are brought within easy touch by bringing the uterus and ovaries into an artificial retroposed anteflexion, the mechanism of which was carefully described by diagrams.

Dr. Kelly had, in this way, palpated fibroid tumors on the posterior surface of the uterus near the fundus, not as large as a pea.

Third, the trimanual examination is conducted either by the vagina or by the rectum and vagina, assisted with the hand above. The peculiarity of this method is an artificial descensus uteri. The uterus is grasped with a pair of bullet forceps, and drawn downward until the cervix is seen at the vaginal outlet; and while an assistant holds it in this position, the gynecologist uses

his hands bimanually. To obviate the employment of an assistant, Dr. Kelly has invented an instrument, which he calls the corrugated tenaculum, flattened and roughened, so that it can be readily held between the last phalanges of the third and fourth fingers and the ball of the thumb, while the index finger of the same hand, assisted by the abdominal hand above, is engaged in making a vaginal or rectal examination.

By one or the other of these methods, the uterus, broad ligaments, and ovaries and tubes, are within reach of a most thorough and searching examination, revealing at once the smallest abnormalities.

WILLIAM S. GARDNER, M.D.,
Secretary.

BATHURST AND RIDEAU TERRITORIAL ASSOCIATION.

This association held its regular semi-annual meeting at Ottawa on Wednesday, Jan. 21st. Among those present were: Dr. Dixon, Pembroke; Dr. Mann, Renfrew; Dr. Ermes, Carp; Drs. Morrow and Wallace, Metcalf; Drs. Agnes Craine and Easton, Smith Falls; Drs. Preston and McEwan, Carleton Place. The city was represented by some thirty-five members. The president, Dr. H. F. Rogers, occupied the chair. Dr. Small acted as secretary.

The president opened the meeting with a short address, and a discussion followed upon the action of the Council in raising the annual registration fee to \$2, and its inability to collect its dues. Many members thought that this district contributed more than others.

Dr. Cousens read a paper on

MEMBRANOUS CROUP.

He held the view that this was a distinct disease from diphtheria, and extolled aconite as a remedy, particularly in the early stages.

The discussion was chiefly on the relation of the two diseases, the majority looking upon them as distinct.

Dr. Horsey read a paper on

SYPHILIS AS A CAUSE OF EYE DISEASE,

pointing out the frequency with which it was overlooked.

Dr. Prevost read a paper on

VOMITING IN PREGNANCY.

He grouped this symptom into three classes:

(1) When the stomach is simply disturbed; (2) when the disturbance is due to reflex action; and (3) when the whole system is in a state of reflex irritation. The latter, he thought, was only relieved by emptying the uterus.

Dr. O'Brien reported a case—presenting the patient—in which the brachial artery had been tied for a traumatic aneurism at the wrist joint.

In the evening, the visiting members were entertained at a dinner by their *confrères* of Ottawa.

The next meeting takes place at Gananoque in the month of June.

Clinical Notes.

ADHESION OF THE SOFT PALATE TO THE POSTERIOR WALL OF THE PHARYNX.

BY JAS. A. MACMAHON, M.D., ST. CATHARINES.

Miss L—n, Niagara, came to me in Dec., 1890, and said she had catarrh, resulting from diphtheria four years previously. I examined the anterior nares and found hypertrophic rhinitis of both sides. When I proceeded to examine posteriorly, I found the soft palate attached to the posterior wall of the pharynx throughout its entire extent, with the exception of a space the size of a small quill immediately behind the uvula. The uvula was an inch and a half long, swollen, and hung down behind the tongue, causing a constant desire to cough and clear the throat. There was an adhesion also between the right side of the base of the tongue and the side of the pharynx. She breathed habitually through her mouth, and was unable to take two consecutive respirations through her nose. She had difficulty of breathing during deglutition, and could not clear her nose, from which there was a constant slight discharge. The history of the case is as follows: Four years ago she had virulent diphtheria. The membrane covered the pharynx, part of the buccal mucous membrane, and the nares. Several thick pieces of tissue were expectorated, which I take to have been sloughs, and when she recovered, her condition was similar to what it was when I first saw her, though not so bad. As a preliminary step I removed an inch of the uvula, and cauterized the inferior turbinated bones, and at the

end of two weeks I proceeded to cut loose the soft palate from the pharynx. By passing a small rubber tube through the nose, and through the small space behind the uvula, and out of the mouth, I was enabled by drawing on both ends of the tube to put the palate on the stretch, and so cut it away from the pharynx. To do the cutting I used a pair of scissors with the ends of the blades bent at a right angle to the extent of half an inch. I was thus able to make an opening over an inch wide; this was kept open by inserting the ends of the right-angle scissors and forcibly opening the blades and putting the parts on the stretch. This was done daily until the cut tissues were completely healed. Her condition three weeks after the operation was as follows: She breathed constantly through her nose, could eat with comfort, and sleep without snoring; she could clear her nose with ease, and was free from the constant desire to cough and clear the throat.

Pathology.

MICROBES IN LEUKÆMIA.

Fermi has found in the spleen of a dead leukæmic patient an organism which he supposes to have some fixed connection with the disease. The germ, which is a short, thick, round-ended bacillus, and of which he was able to produce colonies by cultivation, was not discovered in any of the organs of twelve bodies subsequently examined, the cause of death in which was not leukæmia. Kelsch and Vaillard have proven the pathogenic properties of this bacillus in relation to rabbits (*Centralblatt für Bakt. u. Parasit.*). Clinical observations by Dr. Cameron, of Montreal, upon leukæmia in pregnant women, tend to confirm the germ theory of its origin. He records (*American Journal Medical Science*) a case in which the grandmother, mother, and brother of the patient under observation had apparently suffered from the disease, whilst two of her own children had certainly developed it. In view of the fact that certain microbes have been shown to be able to pass in the placenta from the blood of the mother to that of the foetus, Dr. Cameron's observations afford support to the idea that some organism is casually connected with leukæmia. J.C.