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## THE PATHOLOGY AND TREATMENT OF DIABETES MELLITUS, VIEWED BY THE LIGHT OF PRESENT-DAY KNOWLEDGE.

BY

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The last of three lectures delivered before The Royal College of Physicians of London, 24th November, 1908, published under arrangement with the author.

*(Concluded.)*

I will now pass on to the consideration of a subject which holds a position of great importance in connexion with diabetes. I have dealt at some length with the incorporation of carbohydrate in the building up of bioplasm, and, under a redundancy, with its dissociation in the form of glycogen, as a storage material, to be subsequently brought into use when need may arise. As glycogen is of higher construction than the sugar which enters the molecular complex, the transmutation operation must be looked upon as constituting the result of building up.

Dissociation of carbohydrate in another form—that of sugar, occurs in association with the pathological state existing in the severer kind of diabetes. To this kind of diabetes I have applied the term “composite,” because the eliminated sugar is derived from a twofold source. It comes in part from defective assimilation of the food carbohydrate, and in part from the carbohydrate which has been previously put into combination, and is, from a wrong katabolic action, dissociated.

There must be a flaw of some kind or other in the bioplasmic mechanism to lead to this dissociation of sugar. When the normal katabolic procedure passes on to its proper destination, metabolism proceeds to the attainment of an exhaustion of the latent energy contained in the food-stuff products that are being utilised, and the end-products consist of carbon dioxide, water, and ammonia. In the place of progress to this natural destination, it is found, in the condition being referred to, that the chain of continuity in the bioplasmic mechanism is broken in a

manner to lead to the dissociation of sugar. An influence is brought into play which leads to the sugar being broken off from the bioplasmic complex, and, perforce, carrying with it unutilised energy.

This molecular disruption attended with the dissociation of sugar stands, in reality, in the position of a reversed natural action, occurring in place of the line of change that ought to be passed through. The built-up molecule, under the influence of the environment existing, fails to continue functioning in a proper manner, and, instead, undergoes the dissociation observed. As we have already seen, a similar disruption occurs whilst blood impregnated with phloridzin is circulating through the kidney. Away from the kidney, phloridzin exerts no action, but in association with a condition supplied by the kidney structure, it brings about, after the manner of enzymic agency, the severance noticed.

I must here state that, because a certain amount of sugar is being eliminated upon what may be regarded as a properly restricted diet for diabetes, it must not necessarily be taken that the sugar is derived from the wrong breaking-down process being referred to. Meat contains a limited quantity of free carbohydrate. It also contains locked-up carbohydrate in its protein ingredient. From these sources, sugar may pass into the urine in a purely alimentary case of diabetes. If it does, the defect in carbohydrate assimilative power must be of an extreme character and closely border on the next step in which, in addition to the absence of assimilative power, there is a commencing breaking-down of protein in which carbohydrate has been previously locked up.

Facts can be adduced in substantiation of the proposition just set forth. I have now and then observed in cases of diabetes without the coexistence of associated elimination of the acetone series that, after a large quantity of meat taken at a meal, there has been a show of sugar in the urine, whilst after a moderate quantity, there has been none. Two large helpings of meat, in other words, might give rise to an appearance of sugar when one might fail to do so. This agrees with results that I notice recorded showing the effect of the ingestion of meat on the elimination of sugar in experimental pancreatic diabetes. If reference be made to Hill's "Recent Advances in Physiology and Bio-chemistry," 1906, page 348, it will be found that an experiment on a dog is mentioned in which, in the absence of food, the sugar fell to 0.77 grms. in the 24 hours, and that, after then giving meat, it rose in the first 24 hours to 3.61, and in the next to 6.09.

Allied to the sugar elimination that has its source in a wrong breaking-down of the bioplasmic molecule may, I consider, be classed the elimination of the acetone series of bodies. This constitutes one of the

most important matters connected with diabetes, inasmuch as it is through its instrumentality that the fatal termination of the disease is ordinarily brought about. Sugar, in proportion as it is present in the blood, produces toxic effects of various kinds, which may lead indirectly to death, but it does not, in a direct manner, kill. With regard to the acids belonging to the acetone series, however, the same cannot be said. Their effect in the blood is to interfere with the performance of one of the necessary functions of life.

The continuance of life is dependent upon the elimination of the carbon dioxide produced as a result of vital activity. The sodic carbonate, and, to a certain extent, the sodic phosphate, are the cardinal agents for conveying the carbon dioxide from the tissues to the lungs. If, through any abnormality, there should be an entry of acid into the blood, establishing what is known as a state of acidosis, the alkaline base will be appropriated by it, and thence be prevented contributing to the transport function, as normally designed it should do.

With such circumstances present, and with the acidosis condition growing in intensity, as it will proceed to do, unless it should happen, as where the diabetic state has not attained too advanced a stage, to prove susceptible of being checked, death must sooner or later prove an inevitable result. For vital activity, as for enzyme activity, it is necessary that the products of change should become removed. Their accumulation leads to a stoppage of action, and the issue in connexion with diabetes resolves itself into a question of when the non-removal has attained a sufficient height to reduce the occurrence of action to a point that is no longer consistent with the continuance of life. If, preparatory to this stage being actually reached, a fresh power be given to the blood to take up carbon dioxide, as by the intravenous injection of a sodic carbonate solution or the ingestion of it by the mouth, a speedy restoration of activity temporarily, but unfortunately only more or less temporarily ensues.

The view here detailed was brought forward by me in an article, published in the second volume of the *Lancet* for 1902, entitled "On the Acetone Series of Products in connexion with Diabetic Coma." In this article I state that "writers speak of the altered constitution of the blood by the reduction of its alkalinity as the cause of the coma connected with acidosis and refer the result to the effect of the altered condition of the blood on the tissues of the body. This, however, amounts only to an assertion which gives no insight into the actual pathogeny of the coma." I then open out the view I have mentioned, and in what has preceded, reference is distinctly made to its having been discovered

that, in connexion with the acidosis, a great fall in the carbon dioxide content of the blood has been observed. Not long after the article was published, exception was taken by Drs. Beddard, Pembrey, and Spriggs to the view I had announced, and they have based their conclusions upon the meaning they give to the fall of carbon dioxide in the blood noticed in their experiments. I will avail myself of this opportunity to give an answer to what they have said.

They have furnished an elaborate series of experiments which, judging from appearances, may be credited with having been carried out in a commendably careful and painstaking manner. These experiments, however, throw no additional light upon the subject. They simply give confirmation and precision to what had been ascertained before, rendering it quite evident that the acidosis in diabetes is attended with an advancing fall in the carbon dioxide content of the venous blood, and, secondarily to this, with a fall of it in the pulmonary alveolar air. A major point, in the eyes of the experimentalists, belonging to the matter, is the effect produced upon the blood by the hyperpnœa that shows itself in diabetic coma. I agree with them to the extent that hyperpnœa has something to answer for in connexion with the reduced carbon dioxide content of the blood associated with the coma. It is only naturally to be expected that the exalted respiration should contribute to the reduction that occurs and the hyperpnœa may be looked upon as constituting an effort of Nature to rectify the defect existing in connexion with tissue respiration, by producing, through reduced carbon dioxide tension in the blood, as favorable a condition as possible for promoting the drawing off of carbon dioxide from the tissues.

The conditions belonging to the blood, however, represent only one side of the question. On the other side, there is the tissue production of carbon dioxide as an act concurrent with life, and the necessity of its removal when formed, in order to sustain the continuance of life. The supply of oxygen and the removal of carbon dioxide are necessary factors in connexion with vital activity. Arrest either operation, and vital activity ceases. To give greater security to life, the mechanism of respiration is brought into concerted action with the demand existing for its functional activity. Does anyone say that exalted respiratory action is not induced by a deficient supply of oxygen on the one hand, or a defective removal of carbon on the other? By a stimulus generated within the body by the existence of either of these conditions, a response is called forth from the respiratory nerve centre, which leads automatically to the result that follows. The recent work of Haldane and Priestly

stands in accord with this view, and it is supported by the still later work of Hill and Flack.

According to my opponents, however, things are to stand on a different basis, and a new factor is gratuitously invoked, holding a place altogether outside the realm of respiratory action. Through the prolonged acidosis, they suggest that the protoplasm of the cells is made more sensitive to the stimulating influence of any acid body produced during metabolism, and thus, by the direct action of acid, the diabetic coma becomes evoked. To quote their own words, they say:—"It is suggested that the following process is taking place in the nerve-cells of the medulla: the decreased 'reactivity' of the protoplasm of the cells due to the prolonged acidosis renders their reaction more easily disturbed by, and therefore makes them more sensitive to, the stimulating influence of any acid body, including carbon dioxide and other acids produced during metabolism." (Journal of Physiology, 1908, XXXVII. xli.).

Now, no need exists for going outside respiratory action and bringing in a vague, unsubstantiated, extraneous explanation of the kind here offered. The effect of acidosis is to lessen the carbon dioxide transport service performed by the sodic carbonate and sodic phosphate of the blood, in proportion to the extent to which the base of these salts becomes appropriated by the acids present. Carbon dioxide is constantly being produced as a result of living action, and I think no one will contend that it does not require to be removed *pari passu* with its production. Observation shows that in enzymic and living action the effect of retention of the products generated is to inhibit, in proportion to their retention, the continuation of action, and thus, in the case under consideration, to slow down the production of carbon dioxide. This is the essential point connected with the matter, and to it the experimentalists referred to have given no attention, thereby, I consider, placing themselves upon false ground by taking too narrow a view of the matter. Their work has been limited to a study bearing on the blood, which only plays a secondary part by effecting the removal of the carbon dioxide produced, and obviously, if the alkaline bases of it are placed in a position to be unable to contribute to the removal of the carbon dioxide, the production of it must be interfered with, and the amount entering the blood lowered, as is found to be the case in advancing acidosis. I do not consider that what is said about the blood in diabetic coma still possessing, in the circumstances named, some absorbing power over carbon dioxide, can be taken to invalidate the considerations belonging to the other side. It is not permissible to conceive that all power of taking on carbon dioxide can be lost. Such a condition is not consistent with the

existence of any evidence of life. The question hinges on degree of power, and what is of concern in the matter is the capacity of the blood for drawing off the carbon dioxide from the tissues whilst it is normally traversing the capillaries. There is nothing to show the degree of carbon dioxide tension in the tissues that is detrimental to bioplasmic activity, but I should think it may be taken that the gas, as it is produced, requires at once to be removed.

The agents producing the acidosis are the B-oxybutyric and diacetic acids. The former is the acid started with, and from it the latter is evolved by oxidation, and this, by escape of carbon dioxide, passes into acetone. The bodies are thus linked together into a series, and in speaking of their source, the question resolves itself into that of B-oxybutyric acid.

From chemistry we learn that butyric acid stands as a lower member of the class of fatty acids to the higher members of which belong stearic, palmitic, and oleic acids, components of the fats largely occurring in the human body. In the process of oxidation, it is usually the B-carbon atom of the fatty acids which primarily, both inside and outside the living organism, becomes oxidised. By oxidations in the B position, followed by hydrolytic cleavage, descent can be made from a higher to a lower member of the fatty acid series. A succession of these downward steps leads to the formation of butyric acid, which subsequently passes, by oxidation, into B-oxybutyric acid. In this way, the derivation of B-oxybutyric acid from the normally occurring higher fatty acids can intelligibly be accounted for.

Now, such being the chemical position in which oxybutyric acid stands, ought we not to look to fat for its clinical source? Fat has been already spoken of as a constituent of the bioplasmic complex, thereby standing in the same position as carbohydrate. By wrong katabolism, we have seen that sugar is thrown off, and it is only an analogous procedure for the throwing off of oxybutyric acid likewise to occur. Under the existence of a redundancy, fat and glycogen are thrown off as a normal occurrence for storage purposes, and they are both of them principles suitable for storage on account of the non-diffusible property they possess. In the case, on the other hand, of abnormal dissociation, in each instance alike the product thrown off is of a diffusible nature and thereby incapable of retention in the system. Hence the elimination of both the sugar and the oxybutyric acid with the urine.

Sugar, under the circumstances, is the result of a faulty molecular breaking-down, and must not the same be said of oxybutyric acid? The two seem to stand in a parallel position, carbohydrate in the one case,

and fat in the other, being implicated, but not necessarily acted upon by the same specific cause. As to what stands at the foundation of the intra-molecular error that is attended with dissociation of oxybutyric acid is beyond us at present to discuss. The fact of the dissociation is a point of the deepest clinical interest, and I will proceed to give consideration to it, setting forth the information that we can claim to be possessed of, that will help us in dealing with the matter as medical practitioners.

It would seem, from the fact that acetone is not absolutely a foreign body in connexion with the normal state, that there must be some subsidiary action normally occurring in the direction of its production. Under certain conditions, the acetone bodies come into more prominent view. Amongst these may be mentioned, starvation and other conditions of a nature to lead to inanition, the exclusion of carbohydrate from the food as by restriction to a flesh and fat diet, and some febrile diseases, especially when occurring in children. In these cases, the bodies in question do not show themselves to a sufficient extent to produce any damaging effect; there is, however, another class of case with associated acidosis to which attention has been recently directed—delayed chloroform poisoning and cyclical or recurrent vomiting in children—where fatal effects as in diabetic coma have been observed to occur.

It is ordinarily in connexion with diabetes that the elimination of the acetone group assumes significance. With no other disease is it in a like manner associated. It had been for a very long time known that the urine and breath of diabetics might possess a peculiar odour, since found to be attributed to acetone, but until the recognition of diacetic acid in the urine by Gerhardt of Berlin in 1865, no idea existed of the intrinsic nature of this associated condition from which such serious consequences may arise in connexion with diabetes. Although the course of the process that leads to the appearance of the bodies is virtually independent of that which leads to the dissociation of sugar, yet experience shows that some kind of relationship exists between the two. They seem, in other words, to march on in a parallel manner together.

If a case is dealt with in a way to permit of the elimination of a large quantity of sugar, which implies that the system is being subjected to the toxic influence of the sugar traversing it preparatory to elimination, sooner or later the acetone series may be expected to come into view. It is true it may happen, only very rarely have I seen it do so, that through want of proper attention being given to food, a more or less marked amount of sugar may be voided for several years without the acidosis condition becoming developed. At the same time, however, the



patient, in the circumstances specified, is not exempt from the supervention of the other ill effects arising from the toxic action of the sugar in the system, and has to pay the penalty for the injudicious course taken, by becoming the subject of neuritis, gangrene of the toe, carbuncle, cataract or some other concomitant trouble.

There can, I consider, be no doubt that individuality has much to answer for in connexion with the supervention of the acidosis condition. With a sensitive, high strung nervous organisation, experience leads me to look for a proneness to the appearance of acidosis. In persons of a worrying, restless, dissatisfied nature, its greater liability to show itself gives an increased gravity to this class of case. Perhaps the greater liability to show itself depends upon the nature alluded to constituting an unfavourable factor in connexion with the progress of the disease itself. In children, it is ordinary for it to set in close to the onset of the disease, and for it soon to assume a more or less pronounced character. Moreover, in children it does not usually disappear during the time the sugar is susceptible of removal by dieting, as it may do in the adult.

I have already stated that, apart from diabetes, the acidosis condition is susceptible of being evoked by, amongst other causes, absence of food. Deprivation of carbohydrate alone suffices to act in a similar way. Now, food is found to constitute an influencing factor on the elimination of the acetone bodies in the diabetic, and it here stands in a position to be invested with bearings of the deepest importance. According to the reading adopted of the results producible by food on the acetone bodies in diabetes, the medical practitioner may be conducted along a right or a wrong path in the treatment of the disease. In the interests of medical art, and, through it, of the patient, the subject therefore demands consideration of the broadest, the most substantially grounded, and the most judiciously weighed nature, that can be brought to bear upon it.

Experience shows that the acidosis which occurs in connexion with diabetes is influenced in the manner I will proceed to speak of by the effect of the restriction from carbohydrate food which is put into force in bringing down the sugar in the treatment of the disease. I have been in the habit, for several years past, of having the urine examined for the acetone series in all cases as a part of the examination conducted. The routine plan has been to look for diabetic acid, and this, in a multitude of instances, has been supplemented by looking also for acetone, and, in many instances, likewise for oxybutyric acid. In this way, some thousands of reports upon the point have passed under my notice, and when a positive result has been obtained, a measure of quantity has been inserted by + signs ranging from one up to five or six. It is upon this evidence that what I am about to state is founded.

As a preliminary, it must be mentioned that what is comprised under the term "restricted" diet has a very varied meaning. In reality it should mean the exclusion of carbohydrate as far as can be practically done, and this implies that the substitute for bread-stuff employed should be virtually, and not merely nominally, free from starch. There are foods of this kind to be obtained from establishments in London, whose praiseworthy and successful efforts have been brought to bear to produce it. Unfortunately another side presents itself that nowhere escapes being encountered, and here the foods contain a varying amount of starch, which may even, in some cases, be found to reach that belonging to the bread-stuff food in common use. A moment's reflexion will suffice to show that no proper knowledge can be obtained with reference to the matter under consideration in the absence of a knowledge of the actual state of the food consumed by the patient.

Generally, at an early stage of the disease (children excepted), the effect of putting the patient on a restricted diet is not attended with the production of an appearance of the acetone bodies. At a later stage, in a case where dieting has not been in a proper manner carried out, and where sugar has been all along voided, the effect of cutting off carbohydrate food may be expected to lead to a certain amount of show of the acetone bodies. Afterwards, when, as a result of the dieting, the sugar is reduced and subsequently removed, the acetone bodies may, in the course of a little time, be counted upon to make their disappearance.

In a case of a somewhat advanced nature, where the acetone bodies have already become present, the change of diet leads to a certain amount of augmentation of them, but the augmentation that shows itself does not afterwards intrinsically tend to proceed further. In fact, the issue is dependent upon the stage of disease that has been reached. If too advanced to admit of rectification, the disease and its associated acidosis make headway together till it happens that the acidosis becomes greater than the power of the system to withstand, and death ensues. If not sufficiently advanced to inevitably lead to this issue, and the case should be of a suitable type to subsequently yield, the sugar, as time goes on, may be found to become more and more reduced and ultimately to disappear, and, with this, the acetone bodies may be expected to follow a similar, but more tardy, course. In this way, from what may have seemed at the beginning a very unpromising state, progress may be made through the instrumentality of the influence exerted by the food in bringing down the sugar, for this is at the root of the change induced, to the attainment of a condition in which a totally new aspect is given to the life tenure prospects.

There have been those who have deprecated the strict diet in the treatment of diabetes on account of the relation that has been referred to between acetonuria and the exclusion of carbohydrate from the food. The effect of the acids of the acetone group in leading up to coma should, it has been suggested, demand first consideration and prohibit the cutting off of carbohydrate food. If such a maxim were acted upon, diabetics would stand, as indeed they did in former times, before dieting was applied as it is now, in a very bad position. It is within my own recollection to have known that the outlook in diabetes was vastly different from what it is now. Experience shows that the way to get rid of the acetone bodies is to bring the sugar down, and if this is not done, all that can be looked for is that they will go on increasing. The sugar in the system, by its direct toxic influence, may be regarded as a main factor in causing the acidosis in the first place to set in, and then to grow. In a case where sugar and the acetone bodies have existed, and, by cutting off carbohydrate, have become removed, as long as sugar-free urine is maintained, there is no return of the acetone bodies. Let sugar reappear by relaxation of diet, and a return of the acetone bodies may be expected to likewise occur. I could cite many cases where such has happened, and have further observed a renewed disappearance on a return to the proper food.

The effect of what I have stated is to suggest that, in ordinary cases, the cutting off of carbohydrate should at once be put into force for the purpose of dealing with the acetone bodies. At the same time, however, I do not mean to say that cases may not now and then present themselves in which it would be unwise to adopt such a course. In extreme cases, where the patient is on the brink of the supervention of coma, a little increase in the acidosis existing may suffice to turn the scale and bring about a fatal issue. The sudden withdrawal of carbohydrate has been seen to lead to such increase, and thereby a demand is created for the exercise of judgment in connexion with the course to be adopted.

The frailness of the condition as a whole, in the circumstances under consideration, has also to be taken into account. We know how easily through the general state the acetone bodies may be increased. A severe attack of migraine, for instance, may send them up, and influenza, pneumonia, etc., may do so sufficiently to suddenly bring about the supervention of coma. There is too much instability of system to withstand the influence of sudden change, and thus an added condition presents itself to that emanating from the direct action of the cutting off of carbohydrate on the dissociation process, which tends to promote the augmentation of the acetone bodies passing to the blood.

I have entered at some length into the question of acidosis and its bearings on food treatment in Diabetes in my work on "Carbohydrate Metabolism and Diabetes," and at pages 117-122 will be found what I have said upon the subject.

Mal-application of carbohydrate food within the system constitutes the essential error existing in diabetes, and what is wanted to be effected by treatment is to bring conditions back into line for it to be again turned to proper account. In a state of health, carbohydrate is taken and can be followed to the seat of absorption belonging to the alimentary canal. Here, however, it becomes lost to view. Neither the blood nor the urine affords evidence of the absorption that manifestly occurs. Physiologists say, notwithstanding they are confronted with this fact, that passage of the absorbed sugar into the blood occurs, but that its removal by the tissues prevents its coming into view. This constitutes an assumption which is not only unsupported, but is opposed by experimental evidence. In former times, when it was held that a tangible disappearance of sugar took place in the passage of the blood from the arteries to the veins, some show of foundation presented itself for the entrance of sugar into the circulation, but now that it is admitted that no such disappearance is to be recognised, the entrance could only lead to presence in the blood and outflow with the urine, which represents the state of things existing in diabetes. For sugar to reach the circulation without showing itself in the urine, it would be necessary for a capacity to exist for effecting its instantaneous removal, and that no such capacity is present is made evident by the experimental injection of minimum quantities intravenously.

To keep sugar out of the urine, it must be kept out of the circulation, and any hypothesis explanatory of the procedure connected with the physiological application of carbohydrate must be based upon this principle. To regard it as falling within the natural course of events that the food carbohydrate should pass through the circulation to the tissues in the form of sugar is tantamount to taking the pathological as representative of the physiological state, seeing that it is just such passage which is productive of diabetes.

To escape from diabetes, the food carbohydrate must not be allowed to reach the general circulation in the form of sugar. This is the basis to proceed upon, and the view that has been enunciated in these lectures fits in with it. It is contended that the absorbed sugar becomes dealt with at the seat of absorption, and, in company with the products derived from protein digestion, is, by the bioplasmic action attending lymphocyte

growth, put into an elaborated state. The process falls under the denomination of assimilation, and when the sugar is thus disposed of and incorporated as a constituent of the large newly-constructed molecule, it is placed in a position to be secure from running off with the urine during the transit of the blood through the kidney.

Looked at in this way, the error in diabetes consists of a faulty assimilation of the sugar absorbed from the alimentary canal. Digestion prepares for absorption, and assimilation, which follows immediately upon it, puts the absorbed digestion products into an elaborated state, in which form they pass through the thoracic duct into the blood and there constitute the *pabulum* from which the tissues draw their nutrient supply. This agrees with what used to be the idea entertained by physiologists with regard to the course taken by the food principles—namely, that they became elaborated into chyle which served as a feeder to the blood. It is only in more recent years, as I have previously notified, that the shunting on to a wrong track has occurred, and that, notwithstanding chyle so obviously consists of elaborated food, the food principles have been looked upon as reaching the circulation in an uncombined, small-molecular state, for transmission in such state to the tissues.

The view I am advocating not only fits in consistently from beginning to end with what I take to be properly read physiological considerations, but supplies a working basis for the treatment of diabetes which places us upon perfectly intelligible ground. It provides for the occurrence of assimilative action preparatory to the circulation being reached, and thereby for conveyance in an assimilated state to the seat of utilisation. Thus circumstanced, the digested food products are not in a condition to run off with the urine in traversing the kidney, but stand in the blood as a retainable reserve, ready to be drawn upon by the tissues as need may arise.

To mal-assimilation of carbohydrate food is to be assigned the error existing in diabetes, and what is wanted to be effected by treatment is restoration of the defective assimilative power. Restore this power, and the patient will be no longer diabetic. Assimilative power being restored, he will then be able to take carbohydrate, and this, meeting with the power to assimilate it, will no longer pass through the system as sugar to be thrown out as waste material with the urine.

Restoration of carbohydrate assimilative power, then, is the goal of the medical practitioner in the treatment of diabetes. For the attainment of his object, it may without hesitation be said that food is the prime factor to be brought into play. Sugar in the system is the baneful

agent, and unless this is brought down, no real good can be expected to arrive. The toxicity of the sugar will nullify every other effort. If the sugar can be reduced and removed, the foundation is laid for the desired restoration of assimilative power, whilst in the absence of its reduction, the condition, with varying degrees of rapidity in different cases, may be reckoned upon to grow worse and worse.

This being found from practical experience to be the case, any hypothesis which encourages the idea that it is a natural condition for free sugar to be reaching the circulatory system from carbohydrate food acts detrimentally to the interests of the art of medicine by suggesting the absence of the need for putting into force the one measure that the circumstances show to be essential for beneficial treatment. Upon these premises, the medical practitioner has ground for exclaiming against the misguiding influence derived from his physiological training. But physiologists see nothing of the testing of the truth of their teaching that is afforded by the application of endeavours to correct the physiological error constituting the source of diabetes, and they thereby are shut off from the enlightenment contributed by practical medical experience.

It will be seen that under this view the error is located in the first link of the metabolic chain instead of the last, which is assigned as its seat under the glycogenic doctrine. There is absolutely nothing to lend support to the view that the fault in diabetes (the simple or alimentary form of it) consists of a non-consumption of carbohydrate, except in so far as consumption is prevented by exit in the form of sugar with the urine in consequence of being permitted to enter the circulation as free sugar.

With faulty assimilation, as represented, at the foundation of diabetes, it is perfectly intelligible how it is that sugar finds its way into the urine from the food. From the principle of action involved, it is also explicable how the blood and urine under normal conditions remain unaffected, in relation to sugar content, in the face of the very varying quantities of carbohydrate ingested. Under all circumstances, alike in health and diabetes, as long as the carbohydrate taken is within the assimilative power existing, it becomes assimilated, and hence fails to reach the blood and urine as sugar, whilst should it surpass in amount the power existing to assimilate it, that which is not assimilated will find its way as sugar into both blood and urine.

This harmonises with what is observed in cases of diabetes in which the curtailment of carbohydrate tolerating power is susceptible of being displayed—that is, in the alimentary type of the disease. Experience

tells us that varied extents of tolerating capacity are found to exist and that they are associated with a very sharp boundary line. The ingestion of carbohydrate within a given quantity fails to produce any visible effect, whilst if the given quantity is exceeded by ever so little, sugar is discoverable in the urine in proportion to the extent to which the line has been overstepped. It is easy to understand that there may be a definitely limited assimilative power—indeed, there is a definite limit to the assimilative power existing in the healthy state—and that whilst the power that exists surpasses that required for the work to be performed, there will be no show of anything wrong, whereas if the work to be performed—the amount of carbohydrate to be assimilated—should exceed the power to perform it, the effect will be that the work which escapes being performed will render itself manifest by the unassimilated carbohydrate passing into the circulation as sugar, and thence flowing off with the urine.

Regarded in this way our position in relation to the management of diabetes becomes one of great clearness and simplicity. We know precisely the ground upon which we stand, and can shape our measures into form with great definiteness to bring about a desired result. How, on the other hand, does the matter stand under the supposition that it is natural for the food carbohydrate to traverse the circulation in the form of sugar? Not the slightest relationship is discoverable between the proposition and the working result obtained. Whatever may be effected by the measure adopted, no rational explanation applicable to it can be deduced from the proposition.

Restoration of carbohydrate assimilative power is, as I have said, the great object to be attained in the treatment of diabetes, and, as I have further said, for the attainment of the object, it is necessary to reduce and remove, by dietetic measures, the sugar which is acting perniciously in traversing the system. The primary effect of the removal of sugar from the urine is to bring back health and strength to the patient. Virtually, as long as the urine is maintained in a sugar-free state, there is nothing to exert a damaging influence upon the system, and the only perceptible difference existing between a patient in this state and a healthy person is the different effect arising from partaking of carbohydrate food. Only when such food is taken beyond the power existing to assimilate it does the patient betray the existence of anything wrong.

Restoration of health is something for the patient to be thankful for, but this is by no means all that the removal of sugar from the urine by proper dieting effects. By maintaining a sugar-free state of urine—in

other words, by maintaining a normal state of things within the system, Nature steps in and starts mending what is wrong by re-instating the lost assimilative power. In this way there occurs a reversal of what happens whilst sugar is pervading the system, for here there is an advancing loss taking place, as is testified by the progressive growth of a case previous to the disease being recognised and dietetic treatment adopted. At first the condition may be mild, so much so as not to excite attention, when it may be taken that a considerable amount of assimilative power exists, and then, with the growth of the symptoms, a fall of power must obviously be taking place.

When a case falls under observation, everything depends, with respect to capacity for and speediness of restoration of assimilative power, upon the extent to which the disease has become developed. If only of recent onset, a few days may suffice for the removal of the sugar and, shortly after, for signs to present themselves of returning power; whilst if the disease has, through faulty management, been allowed to run on into the development of a thoroughly established condition associated with marked acidosis, the prospect is a poor one of being able to bring about the requisite control of sugar elimination to permit of anything otherwise than continued progress in a wrong direction being looked for. The presence of acidosis is not by any means, it may be said, to be regarded as a bar to subsequent satisfactory progress. It is the point to which it has advanced that determines the issue. I have known cases where more or less return of assimilative power has occurred notwithstanding a considerable amount of acidosis has existed to start upon.

When the setting in of restoration of carbohydrate assimilative power will occur, cannot in any case be predicted. It may be within a few weeks, or a few months, or it may be delayed for a few years. A striking instance of long delay is afforded by the following case of a patient who was 42 years of age when he came to me on October 14, 1904. He was then in a very broken-down state and his urine contained 7.4 per 1,000 of sugar associated with a certain amount of diacetic acid and acetone. Notwithstanding close attention to a properly restricted diet, the declining sugar did not disappear till the end of about six weeks. On November 18, there was a little sugar present, but on December 3rd there was none. The patient became restored to health and continued on the strict diet till July 28, 1906, before any sign was afforded of returning assimilative power. Acting upon the indication then presented, 3 ozs. of ordinary wheaten bread per diem were tried and found to be tolerated. At the end of September there were indications of further restoration of



assimilative power, and the bread was increased to  $4\frac{1}{2}$  ozs. per diem without any return of sugar. No further indication of growth of power has since appeared, and the patient, who was last seen on November 14, 1908, is so far maintaining a perfect state of health with food adjusted to the curtailed carbohydrate assimilative power that he possesses.

When restoration of assimilative power sets in, there are unmistakable signs to denote its occurrence. These consist of a fall in weight and a bodily feeling of sinking or food want. As long as there is no return of assimilative power, the restricted diet suffices to meet the requirements of the system, and a proper state of equilibrium exists within. Not so, however, when restoration of carbohydrate assimilative power has taken place. Here the restricted diet, as in the healthy person, fails to meet the demands of life, and Nature reveals it through the signs that have been referred to. Whilst no carbohydrate assimilative power exists, a supply of carbohydrate cannot be needed by the system. When, however, the power becomes restored, carbohydrate becomes immediately, to a proportionate extent, wanted,—a fact that is plainly shown by Nature's revelations. These revelations, associated with sugar-free urine, may be safely read as meaning the setting in of returning assimilative power, and that action should be taken accordingly. The action needed is the supply at first of a limited amount of starchy food. The urine immediately tells the tale if too much is given. Afterwards, the starchy food must be increased as the restoration of power advances, always keeping the amount given within the boundary line of the power existing.

The extent to which the assimilative power becomes restored varies greatly in different cases, but in each a sharply defined line throughout the progress exists. In a certain number of instances it becomes restored completely, which implies that the attack of diabetes has been in a direct manner thrown off. It is usual for it to become restored up to a certain point and there to remain more or less fixed. If a patient, in the presence of these circumstances, should take carbohydrate food beyond the power existing to assimilate it, and thus give rise to a renewal of the transit of sugar through the system, the assimilative power will again decrease, and should a marked amount be allowed to pass through, the condition may be expected in the course of time to become as bad as it was at the beginning, if not even worse. It is in this way that relapses take place, and for the maintenance of a satisfactory state, the maintenance of sugar-free urine constitutes a *sine quâ non*. The sugar that pervades the system as an effect of the disease thus becomes a paramount factor in relation to issue.

The class of case upon which the remarks I have made have been based is that belonging to the "alimentary type" of the disease, which is the common form met with in patients above the middle period of life. Ordinarily in young subjects the case at the beginning runs upon the lines that have been depicted, but later, on account of the existence of an inherent progressiveness, the treatment which at one time succeeded in keeping things right fails any longer to do so, and a relentless onward march ensues. Progress into the "composite" or bad type is not always to be averted in later years, but it is very much accelerated by improper dietetic management, and when the advanced stage is reached, the case, on account of sugar being derived from tissue disintegration as well as from food, is naturally no longer susceptible of being influenced by diet in the same manner as whilst belonging to the "alimentary" type. Unfortunately for the training of the student, the class of case that is usually met with in the hospital belongs to the "composite," and not to the "alimentary" type.

It is something to achieve to be able to effect a restoration of assimilative power that will permit of a certain quantity of starchy food being taken. The patient is placed in a much less difficult and much more comfortable position. I first hinted at the recovery of the power in the Supplementary Croonian Lecture delivered by me at the Royal College of Physicians of London in November, 1897. (*British Medical Journal*, 1897, vol. ii), and subsequently, in a communication published in the first volume of the *Lancet* for 1900 under the title of "Differentiation in Diabetes," I entered into a detailed consideration of the matter. The subject has also been fully dealt with in the section on Diabetes contained in my work on "Carbohydrate Metabolism and Diabetes," 1906, and notwithstanding the light that the information throws upon the whole question concerned, and the satisfactory basis that it supplies in connexion with the application of treatment, I have not seen that the attention it deserves has been given to it.

It follows from what has preceded that the physician is dependent for success in treatment upon the character of the food that is available for his patient. Through the incentive that has been created for meeting the demand that has arisen for a pure and palatable diabetic bread-stuff food, endeavours have been successfully applied in the direction needed, and there are now establishments in London from which the diabetic can reliably obtain his required supply. At the same time, there are foods produced and sold under the name of diabetic foods that are not entitled to receive this designation, and they constitute the source of

a great amount of harm. Many are no better than ordinary domestic foods, and others contain too much carbohydrate to permit of its being possible, in the usual run of case, to get rid of the sugar from the system.

In illustration, I may refer to some foods that fell under my notice about a year ago in the following way. In connexion with the case of a boy patient, a physician from Paris on a visit to London was, at the request of an uncle, permitted to see the patient, and on his return home, he despatched a parcel of diabetic foods obtained from a well-known supply establishment in Paris. Here are the figures showing the percentage of carbohydrate expressed as starch that they contained in the water-free state, into which, for apt comparison, all foods should be brought previous to analysis.

	Carbohydrate as starch.
Gluten flour . . . . .	72.0 per cent.
“ bread . . . . .	50.0 “ “
“ biscuit . . . . .	54.7 “ “
Granulated gluten . . . . .	63.9 “ “

Food supplied by Rodemann, of Frankfurt, was referred to when I was speaking of Professor von Noorden's "oat" cure. Samples were obtained from this maker at the early part of the present year, and the figures here given show the percentage of starch present in the articles in the water-free state. An improvement is perceptible upon the figures previously yielded.

	Carbohydrate as starch.
Diabetic White Bread . . . . .	47.2 per cent.
Diabetic Graham Bread . . . . .	40.8 “ “
Diabetic Black Bread . . . . .	41.09 “ “
“ D. K.” Bread . . . . .	74.5 “ “
Diabetic Rusks . . . . .	46.5 “ “
Diabetic Cakes . . . . .	44.4 “ “
Diabetic Meal . . . . .	62.0 “ “

I have said nothing about drug treatment, and for the reason that I know of nothing that by itself exerts a direct and immediate arresting influence over the elimination of sugar. In reality, something is wanted to set metabolism right, in like manner as it is set right by the thyroid extract in myxœdema. This something has not yet been discovered, but science ought sooner or later to place us in a better position than we at

present stand in relation to the matter. We, however, know that metabolism is influenced through the instrumentality of the nervous system, and this is suggestive that it may, in connexion with the point under consideration be likewise open to being influenced in a collateral way by drug agency. I am a strong believer in benefit being conferred by the administration of opium and some of its derivatives.

## NOTES ON ANTERIOR METATARSALGIA.

BY

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The subjects about which I desire to speak to-night are Morton's Disease and Anterior Metatarsalgia. Although the principal symptoms of the chronically strained or weak foot have been more frequently, in the past, described as due to a pathological condition of the posterior or longitudinal arch, it is really not rare to find very similar symptoms depending upon an abnormal anterior, or transverse, arch.

As long ago as 1876 Dr. T. G. Morton, of Philadelphia, described that series of symptoms, which have since borne his name. His description preceded by many years our present knowledge of their predisposing and exciting causes. Typical cases as described by him were characterized by sudden cramp-like pain *in the region of the fourth metatarso-phalangeal articulation*. This pain may begin as a burning sensation beneath the toe, as a numbness or tingling feeling, as a sudden cramp, or as a peculiar feeling of discomfort that increases in severity until it almost becomes unbearable. This pain may first be localized in the neighbourhood of the joint affected, but frequently radiates widely.

You are all familiar with Morton's explanation of the causation of the condition which he described. The plantar nerves were said to be pinched by the adjoining fourth and fifth metatarso-phalangeal joints. Morton advocated excision of the head of the fourth metatarsal bone to relieve the possibility of such compression.

The more comprehensive term *anterior metatarsalgia* was suggested by Pollosson, of Lyons, in 1889, who quite fully described the affection, drawing attention to the fact that little or no pain is felt when the foot is at rest; that greater pain is felt in going down a hill than in climbing up; that the wearing of a boot or shoe conduces to pain which often comes on suddenly, accompanied by the sensation that something has

given away. He mentions that the patient may suffer from the sensation of grating of the bones and makes the observation which has been confirmed so many times since, viz.: that pain may be relieved by supporting the sides of the foot, especially if this be accompanied by pressing upwards the heads of the middle metatarsal bones. Pollosson advanced the theory that is now accepted that the affection is due to a "certain laxity of the transverse metatarsal ligament" and recommended the application of pads to support the arch—a treatment universally accepted at the present day.

An Englishman, Roughton, wrote to the *Lancet* almost immediately after the publication of Pollosson's observations: "I think that there can be no doubt of the reality of the affection described under the name of Anterior Metatarsalgia by Doctor Auguste Pollosson of Lyons." Roughton further gave the history of several cases, drawing attention to the burning pain so often felt in the fore part of the sole of the foot being "sometimes so severe as to cause him (the patient) to remove his boot and grasp the sole of his foot in his hand," thus confirming Pollosson's observation as to its relief. He suggested the appearance of a convexity of the normally concave anterior arch and recommended for the strengthening of the same such exercises as raising the body weight on the toes. With the theory of such treatment, however, I cannot agree.

In 1892, Guthrie wrote to the *Lancet* about the same subject, calling the affection, however, after one of its symptoms "a form of painful toe." He himself was a martyr to the affection and consequently writes a vivid description of its symptoms as they affected him. He writes that suddenly while standing in a theatre he was seized by a "most severe shooting and burning pain in the fourth toe." He remarks "the boring of a hot iron into the flesh might have caused similar pain. It extended up the nerves of the outer side of the foot and leg into the sciatic with a numbing, sickening sensation" . . . . . On taking off his boot he found that the last phalynx of the fourth toe was extended, while the head of the second was slightly displaced downwards. Mr. Guthrie, perhaps, did not fully realize that muscular spasm is a not uncommon symptom of anterior metatarsalgia. He, however, recommended that the condition should be treated by the use of proper boots and reported cures by such treatment.

Thirteen years after Morton's monograph, Goldthwait, in 1894, suggested that the term Anterior Metatarsalgia should be employed to include Morton's Disease and other similar conditions about the anterior arch.

Whitman, of New York, in 1898, concurred in this suggestion in a

most interesting article in which the following statistics are quoted:— In 78 cases of Anterior Metatarsalgia pain was referred to the fourth metatarso-phalangeal articulation in 60; to the third and fourth in 6; to the second, third and fourth in 6, and in but six was the fourth articulation free from pain.

Whitman also drew attention to the paper of Robert Jones, of Liverpool, written in 1897, which recognized the depression of the anterior arch as the cause of the symptoms and also that "the characteristic pain was not caused by pinching of the nerves, Morton's theory, because the metatarsal bones are separated when the arch is depressed; but it was the result of the direct downward pressure of the misplaced bones upon the plantar nerves in the sole of the foot, practically Woodruff's theory" or, I believe, Pollosson's and Roughton's theory.

In the last edition of his text-book Whitman points out that Anterior Metatarsalgia is far more common in private than in hospital practice. The average age of the patient in the reported cases was thirty years. It is more frequent in females than in males, and frequently the sufferers are of an extremely neurotic type. The affection is frequently extremely chronic in duration.

An interesting feature is that pain, when referred, usually extends up the dorsum of the foot towards the sciatic. Doctor Guthrie described it as extending up the nerves of the outer side of the foot and leg to the sciatic. This is of interest, demonstrating as it does, that the affection is evidently not a "plantar neuralgia" in that it is not primarily an affection of the plantar nerves.

*Anatomy.*—The anterior or transverse metatarsal arch is shown on the outer side of the foot as a depression on the outer side of the great toe. When weight is borne this depression is obliterated. When the arch is weakened or broken down the natural resiliency is lost. The centre of the arch may not only be normally depressed, but, perhaps, fixed in a position of depression.

Weakness of the anterior arch is like weakness of the posterior arch in that great pain may be seen with little deformity, or conversely, little pain with great deformity.

Depression of the anterior arch predisposes to pain because of pressure on a persistently depressed articulation and, as Whitman has explained, because the metatarso-phalangeal joints of an habitually depressed arch are exposed to the direct lateral compression of a narrow or ill-shaped shoe.

*Pathology.*—Morton and his immediate successors, as we have seen, believed that the symptoms were due to a pinching of the interosseous

nerve. This is probably partially true in such cases of anterior metatarsalgia as were described by Morton, but the primary lesion in all, however, is a weakening of the anterior arch with a depression of one or more bones. This depression causes pain in two ways: firstly, by the fact that the head of the sunken metatarsal bone acts as a foreign body lying between the neighbouring bones, which form the arch, and the integument; secondly, because the depressed bone, especially if it be the fourth, may be overridden by its neighbour or neighbours if lateral pressure is applied to the latter. This is probably the cause of the spasmodic and more or less characteristic pain described by Morton. To prove this statement examine a hand, which is analogous to a foot, place the fingers in the position of extreme flexion, a transverse or anterior metacarpal arch is clearly demonstrated. Grasp this suddenly making firm lateral pressure. Little pain is felt. Extend the fingers.—The arch disappears. A similar condition is produced to that seen in a chronically strained anterior arch of the foot. The heads of the metacarpals are closely opposed to the integument of the palm. The heads of the middle metacarpals sink to the same level as the outer metacarpals. Theoretically, now, the fifth metacarpal bone of the hand bears the same relationship to the fourth as does the fifth metatarsal to the fourth of the foot. The relationship now being similar, let us suddenly apply lateral pressure by grasping the hand, when pain, severe pain, results,—pain of a neuralgic character similar to that found in Morton's disease, and which may be similarly produced by making lateral pressure on a foot affected with this condition. This, then, is a characteristic point in the diagnosis of Morton's disease.

*Causation.*—The causes are general and specific.

*General.*—(1) General debility. (2) Excessive body weight in proportion to the strength of the foot.

*Specific.*—(a) The use of too short or too narrow a boot causing extension of the proximal phalanges with flexion of the distal phalanges. This results in a depression of the arch as is clearly demonstrated in casts of feet in this condition. (b) The use of a boot with too high a heel throwing the body weight on the anterior arch. (c) The use of a boot with too thin a sole, which insufficiently protects the anterior arch.

*Symptoms.*—The symptoms have been noted in the history of these affections, but may be specified thus:—

1. Pain.—Either of a constant character or spasmodic as described by Morton.

2. Tenderness.—Is found on pressure from beneath over the heads of one or more metatarsal bones, or on lateral pressure in the region of the metatarso-phalangeal joint.

3. Swelling and œdema.—May be so severe as to simulate gigantism, erythro-melalgia or acromegaly.

4. Extension of Phalanges.—Often postural and accompanied by flexion of middle and distal phalanges. This may simulate deformities following anterior poliomyelitis.

5. Muscular spasm. Usually of extensors of toes. It is reflex in nature and causes a deformity resembling No. 4.

6. Callosities on sole.—Frequent and adding to the pain of a mild case.

*Diagnosis.*—Anterior Metatarsalgia must be diagnosed: firstly, from the chronically strained posterior arch, and, secondly, from tuberculosis of the tarsus: from rheumatism, the so-called rheumatoid diseases, and gout; thirdly, from gigantism, erythro-melalgia or acromegaly; fourthly, from postural deformities, and fifthly, from deformities resulting from the paralyses.

*Treatment.*—Naturally resolves itself into: firstly, protection against lateral pressure, and secondly, the elevation or support of a depressed arch with suitable protection of the plantar surfaces of the heads of the bones. The elevation of the forepart of the foot is also of importance.

Operative treatment as suggested by Morton should be applied only in those chronic or resistant cases where conservative treatment has failed.

In certain cases characterized by great spasm of the extensors, causing extension of the toes the transference of the attachment of these tendons to the dorsal aspect of the heads of the metatarsals may be considered.

*Prognosis.*—In all cases good.

## DIPHTHERITIC PARALYSIS.

BY

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The comparative infrequency of the graver forms of diphtheritic paralysis gives a reason for reporting the two following cases:—

*Case I.*—G. W., a lad of 7 years, was taken ill on December 2nd 1906, complaining of pain in the stomach and sore throat, with swelling of the neck. A physician was called on the following day who diagnosed diphtheria and sent him into the Alexandra Hospital.

On admission, his temperature was 100° F., pulse 120, small and of low tension, and respirations 28. The breath was offensively characteristic of diphtheria, there was a profuse mucopurulent discharge from the nose, the neck was greatly swollen and on examining the throat, the tonsils almost met at the middle line and were covered by a dirty gray membrane which extended on to the soft palate.



The heart, lungs, kidneys and nervous system were normal. The boy received 6,000 units of antitoxin, and, as the membrane showed no change, on the following day this dose was repeated. The throat gradually cleared up, the temperature came gradually down, and the general condition improved until December 5th, when the pulse fell rapidly from 120 to 60, where it remained for some days. The boy vomited on several occasions and on December 13th, ten days after admission, there was noticed a nasal twang to his voice and the soft palate failed to move on phonation. Regurgitation of fluids through the nose now set in and the patient's condition was one of moderate weakness and pallor, but was otherwise unchanged until January 4th, 1907, when excessive amounts of mucus were noticed to appear in the mouth and throat, which the child seemed unable to expectorate properly, and the cough with which he attempted to do so, was gurgling and toneless. Swallowing became so difficult that his nutrition was rapidly failing and on January 14, it was necessary to begin feeding by a stomach tube in spite of a marked cardiac dilatation which had appeared in the meantime. Diaphragmatic breathing now became greatly diminished, and for some days indeed the breathing was entirely costal.

The note made on January 14th is as follows:—"His condition is one of extreme weakness, with severe vomiting and a variable pulse, and the diaphragm seems to be gradually losing its action."

Feeding by the stomach tube was continued until January 29th—15 days—and on this day he began to swallow a little food naturally. Improvement gradually came on until on March 28th he was discharged 'Well.' There was at no time any ocular palsy, definite paralysis of the extremities, nor loss of knee jerks.

*Case II* was a boy of 5 years, referred from the Outdoor Department of the Montreal General Hospital on September 21st, 1908, with the history that for two weeks he had had a discharging sore behind his right ear, and on September 19th began to complain of sore throat and swelling of the neck, which rapidly increased, and on the 20th he was taken to the General Hospital where a diagnosis of diphtheria was made and he was transferred to the Alexandra Hospital.

When seen on the 21st he presented a picture of extremely severe diphtheria. His neck and parotid regions were greatly swollen and œdematous; the tonsils, faucial pillars and pharynx were covered with membrane. The nostrils were discharging a large amount of purulent secretion; membrane was seen in the left nostril, and on the upper lip, and behind the right ear were grayish sloughs. The sores on the skin and the membrane in the nose and the throat all yielded diphtheria bacilli on culture.

The pulse was rapid and the temperature was  $102^{\circ}$  F. The urine was albuminous and contained casts and leucocytes. The heart and nervous system were negative.

He received 15,000 units of antitoxin on admission, and, as the condition was unaltered, the following morning 10,000 more, and again on the following two days 10,000 each, when the temperature fell and the membrane disappeared by October 3rd.

Improvement from this date was continuous until October 20th, one month after admission, when the voice became nasal and food was regurgitated through the nostrils.

Shortly afterward a double internal squint developed and weakness of both arms and legs appeared. It was noticed also that it was impossible for the patient to maintain his head in the erect position, as it would wobble from front to back or from side to side. There was no facial paralysis nor paralysis of the tongue. The pupils reacted, though sluggishly to light and accommodation, and the knee jerks were absent.

Early in November there appeared the same filling up of the pharynx with mucus, toneless cough, and inability to swallow, which were present in the first case. Later, there was complete loss of power in the diaphragm, indicated by recession of the epigastrium during inspiration, and death occurred on November 13th, the 67th day of the disease.

The autopsy performed eight hours after death revealed no microscopical changes in the pneumogastric or phrenic nerves—the heart was not dilated and but slightly fatty, but one interesting feature was a condition of evidently acute dilatation of the stomach—the organ holding over one litre and occupying a large part of the abdomen.

No examination of the central nervous system was obtained.

The comparative infrequency of the severe forms of paralysis after diphtheria is shown by Rolleston's figures of 1,500 cases of diphtheria in *The Practitioner* of January, 1909, among whom there were 335 cases or 22.3 per cent. of some form of paralysis. Of these 0.6 per cent. were of the diaphragm, 1.4 per cent. pharyngeal and 3.6 per cent. cardiac. Palatal paralysis, the most common form, occurred in 15.2 per cent.

All of the diaphragmatic paralysees set in after the fourth week.

In the matter of prognosis, Rolleston found that cases developing paralysis of the palate before the third week, showed a mortality of 35.00 per cent. from cardiac failure, as compared with a mortality of 1.4 per cent. in cases developing it at a later date.

Although in large statistics the prevalence of paralysis is as great in cases receiving antitoxin as it was before antitoxin was used, which is due to the fact that so many more recover to have paralysis; the follow-

ing figures indicate that the use of antitoxin has a very real value in the prevention of paralysis.

The percentage of cases to develop paralysis who were injected on the first day was 0, on the second 4, the third 8, the fourth 11, and the fifth 16.

In reference to the effect of antitoxin upon the development of diphtheritic paralysis, Rosenau and Anderson made some interesting observations upon guinea pigs.

They found that a certain dose of diphtheria toxin when injected would cause first paralysis and then death, another dose of less strength would cause paralysis with recovery.

Using animals thus injected it was found that antitoxin given after the onset of paralysis had no effect upon its course. Given shortly before its onset it was also without effect.

Given within 20 hours of the toxin injection it prevented paralysis. Given from 24 to 48 hours after, it had some but little effect, and a small dose, as little as one unit, given 24 hours before the toxin invariably prevented paralysis.

As Rolleston points out, one can with some degree of assurance prophesy what cases of diphtheria will likely develop paralysis, this occurring with much greater frequency and severity in those with extensive pharyngeal and nasal involvement, with much cedema of the neck, glandular swelling and nasal discharge.

And in this class may be placed the two cases just reported.

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## PATHOLOGICAL DISCOVERY AND ITS BEARING UPON PREVENTIVE MEDICINE.

BY

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Up to within quite recent times the term pathology has commonly been regarded as synonymous with morbid anatomy and histology, the study of the gross and microscopical changes in diseased tissues. There may be those here present who still retain this conception. It would be difficult to demonstrate on a broad scale that the great discoveries in pathological anatomy had been of direct influence upon the problems of hygiene, though much might be said regarding their indirect influence. Happily there is an increasing recognition all over the continent, and all over the world, that pathology is very much more than this, that it

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includes the study of disease in all its aspects, that it is the science of medicine, or all of that science, save the not unimportant portion which deals with treatment; that thus it is the exact study of the causes of disease, the processes of disease, the results of disease. And undoubtedly it is the last thirty-five years that have seen the great medical renaissance, and this new birth has followed upon exact observation and the employment of the experimental method. Advance in medicine has to a very remarkable degree been the outcome of exact experimental investigations conducted in the laboratory, to a remarkably slight extent has it been due to empiricism or the employment of methods of treatment upon no settled plan but on the chance that they might succeed. It matters not whether these investigations have been conducted by a chemist, like Pasteur, by a zoologist like Metchnikoff, by country practitioners, like Jenner and Koch, by army surgeons like Laveran, Ronald Ross, Walter Reed, or by those who in these later years have devoted their lives to medical research like von Behring and Roux and Flexner; all these investigations are pathological.

If thus it can be shown that the extraordinary advances made in medicine within the last generation are essentially based upon the data afforded by exact observation and pathological research, this is equally the case—indeed it is true to an even greater extent—with the special branch, preventive medicine. For if we seek to determine wherein pathological research has led to the most remarkable discoveries, and medicine has undergone the greatest revolution, there can be no hesitation in deciding that it is in connection with infectious diseases: it is the discovery of the causative agents of so many diseases, of the mode of action of pathogenic microbes in producing the symptoms of disease, and the conditions of life of these agents, that has been the great outstanding triumph of the end of the nineteenth century. Now it is precisely along the lines of controlling these infectious diseases that preventive medicine has its dominant activities—and it, therefore, even more than medicine in general, has profited from these discoveries. Regarded from this point of view it is self evident that the great pathological discoveries have had an untold influence upon public health and its problems. I fear that were I this afternoon merely to pass in reviewing these discoveries—fascinating as they are—with comments upon their bearings upon public health work, I should not tell you anything of which you were not already convinced. It would be a work of supererogation. It will be of greater service if I attempt to trace in a certain number of instances how far we were able to advance in the prevention of disease under general hygienic principles, and what has been our advance once research has afforded us a certain knowledge of causative agents.

As a preliminary let me say that we may take the period 1880 to 1887 as that in which, thanks to the work of those great founders of bacteriological science, Pasteur and Koch, the principle became surely established and accepted, that infectious disease is due to the presence and growth within the system of pathogenic micro-organisms. Some were discovered before that period, others since, yet others have still to be discovered; but by the year 1887 it may be laid down that the lay world had come to accept the new doctrine, so that from now on legislation and public health ordinances based upon it became established and began to bear fruit.

Let me begin, therefore, by discussing the general decrease in the death rate during what, as regards statistics, we may speak of as historical times, during, that is, the last three centuries.

Finklenburg, of Bonn, has estimated that the average human life in the sixteenth century was only eighteen to twenty years. In the middle of the nineteenth century it has risen to over thirty-seven years, to-day it is well over forty years. We obtain possibly a better idea of the improvement in hygiene by a study of the mortality tables of a great city like London. There from 1620 to 1643 the mortality has been estimated as *seventy* in a thousand; from 1660 to 1679, a period including the great plague, it rose to *eighty* in a thousand. After the last traces of the oriental plague left England, the improved hygiene which coping with the same had rendered imperative further reduced the mortality so that this in the period of 1728 to 1757 fell to *fifty-two* in a thousand. From this period to the end of the eighteenth century it progressively rose, owing largely, it would seem, to the continued increase in the mortality from smallpox, an increase favored instead of hindered by the first serious attempt at prevention by immunization, by the extension of the practice of inoculation of the healthy with material from those suffering from the disease. If that practice afforded a mild disease and subsequent immunity to the inoculated one, it at the same time spread the disease among his entourage. The inoculated disease was so mild that the patient was apt to take no precautions. Thus the death rate from smallpox rose until it is estimated that 3,000 persons in each million died annually from the disease. It is in 1796 that we have the first great practical discovery in preventive medicine, that, namely, of vaccination by Jenner. Here let me be just. Prior to that date it was a popular belief in several districts that cowpox shielded the individual from subsequent smallpox, and there is clear evidence that others had performed the artificial inoculation for preventive purposes. But Jenner it was who by his researches proved the correctness of this belief and demonstrated by exact experiment that inoculation with matter from cowpox protected for long periods

from smallpox. And, his observations being exact and incontrovertible, in the first years of the nineteenth century vaccination became widespread in London with, as a result, a rapid fall in the death rate to 29.2 in a thousand. Again there came a period of latency until 1835, the average from 1813 to 1835 being *twenty-nine* in a thousand. From now on the British Parliament passed a succession of acts bearing upon municipal administration, child labor, ventilation of workshops, artisans' dwellings, etc., while the water supply and drainage of the great city were greatly improved. As a result we find a period of steady fall inaugurated, with death rate of *twenty-five* in a thousand in the decade from 1841 to 50, a little over *twenty-four* during the score of years 1851 to 70, 22.5 in the decade 1871 to 80.

During the intermediate period, 1880 to 1887, already noted, the death rate sank to 20.4 and in the year 1887 for the first time fell below 20 (19.5). Broadly speaking, we may say that in eighty-seven years it fell 9.7 points or 0.11 per annum. Now during the last twenty-one years it has fallen another 5.2 points, or 0.25 per annum, until last year it stood at 14.3 in a thousand. In other words, since the discoveries of the causative agents of disease, and the utilization of the knowledge thus gained *the diminution of the death rate in London has been accelerated more than twofold.*

Weighing everything carefully, it is impossible to ascribe this greatly accelerated reduction to causes other than the advance in public and personal hygiene brought about by the outburst of research and discovery regarding the causative agents of infective disease during the late seventies and early eighties of the last century, and the increasing knowledge of the modes of infection and of prevention of the same that these discoveries brought in their train. That this reduction in mortality will continue at the same rate is not possible. Only a certain proportion of the total mortality is due to the infectious diseases. Could we wholly banish these, we should not banish death from our midst; the individual still must die from other causes. But these same considerations render the figures I have given all the more remarkable. As we reduce the number of deaths from preventable causes each reduction of one in a thousand means a greater and greater effort, and, notwithstanding this, the reduction has during the last twenty years continued at twice the rate of that occurring during the previous eighty years.

I have taken London as my example, because it is the city which combines greatest population with best hygienic conditions and lowest mortality during the greater part of the last hundred years. Had I selected New York, the figures would have been yet more striking. Here the de-

crease in the death rate during the last twenty-five years has been practically six in a thousand (5.89). But at the same time they would have been somewhat exaggerated in that there was not in New York to anything like the same extent that preparatory period of progressive reduction in mortality due to an enlightened enforcement of legislation, which, while primarily directed to secure the increased well-being of the individual citizen, simultaneously reduced his liability to infection.

#### TUBERCULOSIS.

But it is when we come to consider individual infectious diseases and the means now being taken to arrest them, that we obtain the most striking demonstration of the beneficent influence of the great pathological discoveries of the nineteenth century upon preventive medicine. Naturally the first of these to be considered is that which among us stands pre-eminent both as causing disease and swelling the death rate. It is a matter of familiar knowledge to all of you that among the peoples of the temperate zone tuberculosis stands *facile princeps* among the enemies of mankind, causing roughly one seventh of the deaths from all causes. It is well known to you also that post mortem examination reveals naked eye evidence of either active or arrested tuberculosis in the majority of autopsies at all periods of life and in those who have died from all forms of disease, the frequency increasing progressively until the thirteenth year is reached. My own series of autopsies at the Royal Victoria Hospital at Montreal upon some 1,400 cases has yielded to Dr. Landry evidence that 45.5 of the total afford such macroscopical evidence of the disease, or, if certain doubtful indications are also included, a little over 51 per cent. Statistics from the crowded industrial centres in the old world give yet higher figures, as do also routine microscopical examinations of the peribronchial, peritracheal, and mesenteric lymph glands; so that it would seem that there is no very great exaggeration in the statement that every one has his bit of tuberculosis: even if fortunately for us the majority are able successfully to hedge in that bit and render it harmless.

Lastly, every medical student knows that Koch announced the discovery of the tubercle bacillus in March, 1882, and that in 1884 he published the extended account of the researches which led up to and confirmed that discovery. I doubt if in any science there exists a more elaborate, more painstaking and thorough research than is revealed in those pages of the second volume of the *Mittheilungen aus dem kaiserlichen Gesundheitsamte* with their demonstrations of how the tubercle bacillus is to be detected in the tissues, how the germ may be grown in pure culture, and the record of the hundreds and hundreds of animals of different

species laboriously studied, both for the presence of the tubercle bacilli in their tissues and for the results of inoculation of pure cultures of the bacillus in setting up the disease.

So elaborate and so thorough a research it was as to establish instant conviction. From 1884 onwards the medical world was forced to accept the tubercle bacillus.

But very few of us know, and still fewer realize nowadays what that discovery has meant not merely for the medical outlook upon phthisis and other forms of tuberculosis, but also for practice. One has not to be very old to be able to remember the medical events of the eighties, but I take it that others have the impression (as I had) that the discovery came naturally enough with years of preparation in which the medical profession was coming round to the opinion that tuberculosis is an infectious disease. Secure in the possession of this exact knowledge that the tubercle bacillus is to be gained from every focus of active tuberculosis, we are only too ready to admit the evidence that earlier workers had brought forward regarding the infectiousness of the disease. We cite Isocrates, Avicenna, Fracastorius, Morgagni, and others as recognizing the fact, we quote the disinfection ordinances of Naples and other Italian towns in the eighteenth century. We adduce Villemin's masterly experiments of 1865 upon the experimental production of tuberculosis in rabbits, and the even more convincing observations of Cohnheim and Salomonsen in 1877, in which, placing tuberculous material from man in the anterior chamber of the rabbit's eye, they were able to see and follow the development of tubercles upon the iris. We forget that all these observations had practically no abiding influence upon medical action; that accepted by some they nevertheless did not become part of general teaching and general belief; that others afforded other explanations; that as regards Villemin's and Cohnheim's experiments, Talma, Aufrecht, and Lebert in Germany, Empis and Metzquer in France, Burdon Sanderson, Wilson Fox, and Klein in England brought forward such apparently overwhelming evidence to show that pus and foreign matter, and portions of healthy and diseased tissues of a nontuberculous nature would produce identically the same effects; that accurate as we now know these experiments to have been they did not convince. We know now that prior to Koch, Baumgarten had seen the tubercle bacillus, but he had not isolated and grown it and studied its properties. Not one of these observers had afforded a demonstration absolutely conclusive. That was left to Koch and his must be the credit.

As a matter of curiosity I have during the last few days glanced through a series of the more popular textbooks of medicine, published



between 1845 and 1882 to recall what was the ordinary and accepted teaching regarding tuberculosis. And I can say that until one does this it is impossible for any one to realize the revolution that Koch's discovery has brought about. Whether it be in regard to ætiology, to the morbid anatomy of the process, the relationship of the different manifestations of the disease, or the treatment, everything is vague and hesitating, where it is not, as we now know, incorrect. If anything, there is increasing vagueness as we approach 1882 and that largely because of Virchow's erroneous teaching regarding the nonidentity of caseous pneumonia with other forms of tuberculosis and its intimate relationship to catarrhal pneumonia. One and all, English and French and German textbooks lay stress upon hereditary diathesis and the strumous constitution as the basis of the disease. Most admit that it may arise *de novo* in an individual showing no signs of the diathesis, and then in consequence of improper nourishment, impure air, unhealthy occupations, low temperature, and want of sunlight. In the very year of Koch's discovery, Peters, professor of internal pathology in the Paris Faculty and member of the Academy of Medicine (*Leçons de clinique médicale*, second edition, II, 1882), who devotes some 600 pages to the subject of tuberculosis and more than 200 to its ætiology, denies its contagious nature, regards Villemin's conclusions as non-proven, and the tubercle as an evidence of a vice of nutrition and due to the loss of vital energy.

It is, you will find, always the case that when the exciting cause of a disease is not known, teachers and writers dwell with emphasis on all the predisposing causes. And on the whole these writers of the first three quarters of the nineteenth century were quite accurate in their enumeration of these predisposing causes, even if they were uncertain as to the relationship of the one to the other and to the actual disease. Nay, more in these latter days from the attitude naturally assumed after the first discovery of the specific pathogenic microbes—the attitude of ascribing everything to the microbic factor—we are coming round to realize more and more that it is the summation of *two* factors, of the resisting powers or relative susceptibility of the organism and the relative virulence of the micro-organism, together with the number of the latter gaining entrance into a particular area at a particular time, that determines whether infection is lighted up. We must be prepared to see, therefore, that progressive improvement in general hygiene during the nineteenth century had a definite effect upon the mortality from tuberculosis. Better housing, better conditions of work, better ventilation of house and workshop, better wages and consequent better nourishment of the mass of the people, all had their effect in reducing the ravages of the disease. But

think what it has meant to preventive medicine to know with absolute certainty that the disease is infectious; that infection is conveyed by the bacillus; that these bacilli cannot thrive and multiply at the ordinary temperature, but only at body temperature or thereabouts, and that consequently every case of the disease must be derived directly from a previous case; that the bacilli are discharged in enormous numbers in the expectoration of phthisical patients and may be found in teeming millions in the milk of cattle with tuberculosis of the udder; and that, consequently, infection is from man to man or from cattle to the drinkers of raw milk, namely to young children; that the bacilli easily killed by sunlight may persist for long weeks and months in a virulent state in dark rooms. These facts supply the data for prevention of the disease; they afford the data for the solution of the problem.

We see here the same process at work as was noted in connection with the general mortality rate. You must forgive me if as a British subject I tend to refer to British statistics best known to me. With improved general hygienic conditions of the mass of the people, even before anything was known regarding the tubercle bacillus, the reduction in the death rate from phthisis in England and Wales was very remarkable: that dropped from over *thirty-eight* in 10,000 living in 1838 to slightly over *eighteen* in 1884. Since then there has been a distinct acceleration in the fall. What can be accomplished is, however, best shown not by taking the general death rate but by employing the figures from cities in which the disease has been combatted systematically by measures logically deduced from the data furnished by Koch's discovery.

You all know New York's proud showing under the firm but beneficent mastery of your Board of Health led by Dr. Hermann M. Biggs, and how greatly the death rate for tuberculosis has dropped during the last ten years. But even more striking figures are afforded by Edinburgh, a much smaller city, where the populace is more homogeneous, more docile, and more intelligent than the illiterate swarming mixture of European immigrants, which crowding into the tenement house quarters of your city, forms so hard a nut for your health officials to crack. To Dr. Philip is due the thorough development of the Edinburgh dispensary system, which, acting in conjunction with the city health authorities and strengthened by compulsory notification makes it possible to deal with every recognized case of tuberculosis. There the deaths from tuberculosis have fallen to a remarkable extent. As Dr. Philip points out, we may divide the last twenty years—or more accurately the period from 1886 to 1906—into two equal periods. During the first of these necessarily the direction of antituberculosis effort was rather in-

definite. Even when, as in Edinburgh, the antituberculosis effort had assumed more definite shape, time was needed before the effect of effort began to register itself in the death rate. In brief, whereas from 1887 to 1896, the fall in 10,000 was 2.5 (from 19.5 to seventeen in 10,000), from 1897 to 1896 the fall in 10,000 was from nineteen to eleven. The percentage reduction in the first ten years was 12.82, in the second ten years 42.1. These are remarkable figures. We may say that in the last ten years there has been a greater reduction than in the previous fifty years. The effect of exact knowledge, gained by experiment, is indubitable.

#### MALARIA.

The other great scourge of mankind affords even more striking testimony as to the effect upon the public health of pathological discovery. We, living in the temperate zone, are apt to forget that there is a disease more terrible and more widespread than tuberculosis. It has been said that one half the mortality of the human race is due to malaria. This, I am inclined to think, is an exaggeration, but when, as Shipley points out, in 1892 out of a total population of India of 217,255,655, the deaths from all causes reached the figure of 6,980,785, and of these 7,000,000 odd 5,000,000 were due to "fever," and fever in the tropics is known to indicate most often malaria, the statement is perchance not so very far from the truth. This same would seem true of China, with its innumerable millions, and all here know the ravages of the disease in Central and South America. Nor need I remind you as next door neighbors of Long Island and New Jersey that the disease is not confined to the tropics. What is of equal importance is that where the disease is not fatal it profoundly affects the vitality and power of the individual for long periods, causing listlessness and incapacity for active work. Of the British soldiers in India three out of seven suffer from an annual attack of malaria; of those on the west coast of Africa each individual suffers on the average from two attacks each year; the loss in energy and effectiveness is singularly great. A recent writer, W. H. T. Jones, of Cambridge, has brought forward a considerable body of evidence from classical writers that, more particularly, the decay of Greece and probably also the decline and fall of the Roman Empire coincided with the introduction and general spread of malaria through the valleys of the Grecian peninsula and through the hitherto fertile and populous Campagna around Rome and other low lying regions of Central and Southern Italy. The striking alteration in the character of the inhabitants of the Island of Mauritius, brought about by the introduction and universal spread of malaria since the year 1866 strongly supports this view.

That the disease especially haunted low lying and swampy districts had been known for centuries—as also that it was particularly dangerous to travel about in marshy regions after nightfall. As the name malaria implies, a miasm was supposed to be exhaled from the damp ground and to cause the disease. It was found that drainage of an infected district materially reduced the incidence of the disease. When I was an undergraduate at Cambridge there were still those living who remembered when it was dangerous to open the windows at night in certain parts of the town on account of the ague from the neighboring fen which might result. That fen, like all the marshes of the fen country, has long been drained and neither in that district nor in any other part of England has there been endemic malaria for long years. Drainage similarly and occupation of land has banished the endemic ague from regions in the northern States, like that around Detroit, where within living memory malaria was terribly rife. Procedures of this order coupled with the proper and adequate use of quinine have in various parts of the world materially reduced the incidence of ague, although where these precautions have not been adopted the disease has continued its depredations with undiminished vigor. Here again we see that prior to the remarkable series of discoveries bearing upon the causative agent of the disease preventive measures were in force and had a definite effect. The discovery by Laveran in 1880 of the hæmamoeba in the corpuscles, important as it was, had little immediate effect, save to afford indications for treatment of the individual case; for studying the cycle of forms it was found that quinine was most effective upon the free amœbæ as they developed from the liberated spores. That discovery did not throw light upon the mode of infection. Nor again, deserving it is of remembrance, did the admirable paper of Dr. King, of Washington, in 1883, in which he showed that the mosquito must be the carrier of malarial infection, have any influence upon preventive medicine, save as preparing us to accept the absolute demonstration when it came. Only in 1897 did the researches of Captain Ronald Ross solve the mystery and immediately demonstrate how the disease is to be prevented. All are now familiar with Ross's work—how first he found peculiar accumulations of parasitic type in the walls of the stomachs of mosquitoes of a particular species that had fed upon the blood of malarial patients; how unable to procure sufficient human material he turned his attention to a similar blood parasite in birds and found here that another species of mosquito acted as the intermediate host; how making use of W. G. MacCallum's observations upon the sexual union between the flagella and free amœboid corpuscles of an allied parasite in the removed blood of Canadian crows

he recognized the existence of a sexual cycle of these parasites in the stomach and tissues of the mosquito, and concluded that a similar series of stages occur with the malarial organism of man sucked up by the anopheles; how, in short, he concluded that this mosquito was the carrier of the disease from individual to individual; how the correctness of these conclusions was demonstrated by the younger Manson, who in malaria free London, became infected by the bites of mosquitoes which had fed upon ague patients in Rome, and had been transported for the purposes of this experiment; and how the life history of the human malarial parasite, or more accurately, parasites, was worked out along the same lines by Grassi, Bastianelli, and Bignami, in Italy. Here now, at last, we knew how malaria was acquired and immediately it became evident how it was to be prevented—either by guarding the individual from being bitten by the anopheles, or better by preventing the anopheles from breeding, and so exterminating these mosquitoes.

The results of preventive measures adequately carried out have been extraordinary. Take for example the instance of Ismailia in Egypt. This is a town of about 6,000 inhabitants, principally employees of the Suez Canal Company, founded by Baron de Lesseps in 1862. In 1877 a fresh water canal was constructed to supply the town with drinking water, and with that an ideal breeding ground for mosquitoes was afforded. Almost at once malaria appeared and since then malaria has steadily increased until 1902 when the mosquito campaign was inaugurated. We obtain the following figures:

Year .....	1898	1899	1900	1901	1902	1903	1904	1905
Cases .....	1545	1784	2250	1900	1548	214	90	37

In this last year Dr. Pressat the official medical officer reported that there were no new cases of infection; the disease was killed out, and that by one mosquito brigade consisting of four men acting under the medical officer.

In Port Swettenham, in the Malay States, the same brilliant results have been obtained. Here was a new tropical settlement, surrounded by marshes in which the anopheles bred abundantly, and having an annual rainfall of 100 inches—a swamp of the most pernicious order. Within two months of the opening of the port in 1902 forty-one out of the forty-nine government quarters were infected and 118 out of 196 government servants were ill. Now, after filling up all pools in the settlement for a space of 440 yards all around, and clearing the jungle, no single officer has suffered from malaria since 1904, and the number of cases among children fell from 34.8 to 0.77 per cent. The only sufferer at the present time is the enthusiastic District Surgeon, Dr. Malcolm Watson, whose

main income gained from attending malarial cases has dropped to practically zero.

But what is the use of telling to an American audience what drainage, oiling the water, and the use of mosquito netting has brought about? You all must be familiar with the triumphant work of the mosquito brigade at Havana, the yet greater triumph if possible—as regards malaria—of Colonel Gorgas and his corps on the Panama Canal. It was not graft—bad as that was—but the mosquito that brought ruin to the French project. You know that long as is the national purse and great as is the national pride it would be impossible for the States to carry the canal through the Isthmus but for the preventive guard of the medical department, and for that noble army of trained workers which has brought it to pass that the death rate in the Canal zone is now less than that of the city of New York. That the daily sick rate is only about seventeen in a thousand has been obtained almost wholly through mosquito prophylaxis.

#### YELLOW FEVER.

Speaking of mosquitoes one naturally passes to consider that other scourge which has been absolutely proved to be conveyed by this insect, namely yellow fever. Here again the facts must be so familiar to you that it is only necessary to refer to one or two salient points. Once more we see that observation led thoughtful members of the profession, like Finlay, to recognize the mode of infection, but that it was well conceived experiments and the absolute data gained therefrom that preceded advance. There is no nobler chapter in the history of American medicine, no more triumphant demonstration of the beneficent results of pathological discovery than the history of the suppression of yellow fever in Havana. The episodes follow one another with such dramatic rapidity and inevitableness: First, Walter Reed's deduction that maturation of the unknown parasite in an intermediate host is necessary to explain the delay between the first and subsequent cases of the fever in a new locality; then by a process of exclusion the narrowing down of the probability to the mosquito; next the conclusive experiments by Carroll, Lazear, and the members of the Cuban commission when, under conditions excluding fallacy of observation, they allowed themselves to be bitten by mosquitoes (*stegomyia*), which days before had bitten yellow fever patients; the development of the disease in those so bitten; the death of Lazear; the establishment of the mosquito brigade in Havana; the immediate fall in the incidence of the disease, so that whereas in 1900 there were 302 deaths in the city, in 1901 there were only five, in 1902 none. Never, surely, has preventive medicine won so decisive a victory,

a victory, let me repeat, based upon the results of pathological research.

#### OTHER INFECTIONS.

In this way I might continue discussing disease after disease in which the results gained by scientific investigation have led to the adoption of practical means of prevention. I might, for example, discuss the extent to which a knowledge of the mode of life of the typhoid and colon bacilli have influenced the problems of pure water supply to great cities and give you figures showing the effect of filtration and what bacteriological studies have accomplished in this respect; might turn to the prevention of infectious disease among domestic animals and give you that early but fascinating story of how Pasteur and the French Commission, by studying the life history of the anthrax bacillus, solved the mode of infection, gained a means of causing immunity, and reduced the mortality from this disease in French sheep from over ten to under one per cent.; might discuss rabies and the studies upon preventive inoculation against this dread disease and the results of the same; might take up Fourth of July tetanus and how this is being reduced thanks to a knowledge and employment of methods of preventive inoculation gained from laboratory experiments; and, perhaps of wider significance than all of these, might dwell upon the reduction in diphtheria mortality directly due to the brilliant researches of Behring and Roux upon diphtheria antitoxine. But after this recital of the effects of pathological discovery as applied to the extirpation of yellow fever, the data that I could give you, important as they are, would, I imagine, produce somewhat the effects of an anticlimax. Let me, instead, in conclusion attempt to draw together the reins of this discourse.

#### CONCLUSIONS.

We see in the first place that in ignorance of the causative agents of infective diseases earlier generations sought after the predisposing causes; that such predisposing causes cannot be neglected as factors determining whether the individual does or does not succumb to infection; and that as a result the more accurately these predisposing causes are determined and the fuller the measures taken to guard against them, the greater in general is the lessening of the death rate.

But, in the second place, it is obvious that once through exact research the exciting cause of a disease becomes determined, its life history studied, and mode of conveyance from individual to individual becomes worked out, the public health problems in connection with the prevention of that disease assume a totally new aspect—vagueness gives way to clearness of vision; direct methods become possible, and the previous slowly progressive diminution of incidence and mortality have given

place to a rapid and in some cases dramatic arrest and extirpation of the disease.

Next, developing out of this, pathological discovery has taught the further lesson, that while certain general principles govern the incidence of all infections and so certain general measures must tend to lessen the incidence of infectious disease in general, nevertheless, the more exact have been the studies into the life histories of pathogenic organisms, the more surely have we learned that each specific micro-organism has characters distinguishing it from all the rest, and as a result that each disease induced by these agencies must be proceeded against by special means; each disease brings with it particular problems to be solved. It is these special means rather than broad general measures that afford the ultimate complete triumph.

Finally, I would urge that there is a danger that must be guarded against in a recital such as this: The danger of satisfaction, the danger when contemplating what has been accomplished, of neglecting to consider the abundance of what is still to be achieved. To you at Columbia I would emphasize that what has already been done is as it were little more than the fringe of what is still to be done. Even in this one department of public health work—that of controlling the incidence of infectious disease—there is a vast achievement still possible. In 1907 (and I doubt whether 1908 showed any very striking differences) 30,000 individuals died in New York from preventable causes. It is not too late for some at least of those here present to determine to devote themselves not to the treatment, but to the prevention of disease, confident that their efforts, by research and by the application of the results of research to the problems of public health, they can during their life time save a greater number of lives by following the profession of Preventive Medicine than they could by direct treatment of those already the victims of disease.

Do not think that in making this reference to New York I am seeking to disparage what has already been accomplished in this city. That is very far from being my object. The work accomplished by Dr. Hermann M. Biggs and his coadjutors during the last fifteen years is in every respect remarkable, and is worthy of, and is receiving, the attention and commendation of those interested in public health throughout the civilized world. But let me impress upon you that it is carried out under most unsatisfactory conditions. These conditions, it is true, make the personal triumph of your health officials all the greater. With problems so vast to be tackled, you in this city, if I am informed aright, do not pay a single one of the heads of your Health Department sufficient to



allow him to devote his life and energies to this great work. On the contrary you afford stipends so miserable that perforce, those on whom you depend have to engage in private practice to keep body and soul together. What has been accomplished has been due to the self-sacrificing enthusiasm of these men. Nay, your present policy absolutely prevents any sane man undertaking preventive medicine as his life's work. Who would seriously prepare himself to make preventive medicine his profession, when being appointed to office his continuance in the same is dependent upon the continuance of one or other political party in power. These matters, I hold, must be taken out of the field of party. Permanency of position is essential for the development of a properly qualified body of health officials and, for the good of the country as a whole, the matter should not be left to the separate States but should, at least, be directed and controlled by the federal authorities.

I am no socialist, but, on the contrary, appreciate that point of view which leads the individual to select some work in life in which he can regard himself as his own master, independent of trusts, and can strive to do good work in the world along his own lines. I have little sympathy with the aspirations of those who would see the state becoming the monopolist in all forms of industry and endeavour. Nevertheless, looking to the future, I cannot but see the time approaching, and approaching rapidly, when the good of the community will demand that an important section of the members of our profession shall be servants of the state, their work devoted, not to the cure, but to the prevention of disease. That time has already come in Great Britain, where since 1892, there has been developed a body of trained and certificated specialists, of medical men whose whole time is devoted to preventive medicine.

Despite the difficulties imposed by State, as distinct from federal, rights and privileges, it is inevitable that a like development shall take place here in America. The national weal demands that either the States combine, or the federal authorities impose common action and a common system, in dealing with zymotic diseases, diseases which do not respect State boundaries and State prerogatives, which, from their liability to become widespread are matters of national concern. It is, for example, a matter of national and not of State concern that the plague has become enzootic in certain Californian rodents. National health is above the prerogatives of the individual States. And as the control and eradication of preventable disease becomes more and more effective, so will the duties and the emoluments of the ordinary medical man become less and less, the responsibilities of the preventive officer greater and greater. There will—there must always—be opportunities for independent en-

deavour: The general practitioner, like the poor, must always be with us; the natural multiplication of the people; accidents calling for the trained surgeon; diseases of special regions demanding the care of the specialist; the common neurasthenic; the chronic invalid; must always demand individual care. But as a community, we shall have to follow the Chinese fashion to a very definite extent, and establish an order of physicians whose remuneration shall be proportioned to their success in keeping us from getting ill.

Nor, when we come to consider the matter, is it one whit less noble or less satisfactory to become a servant of the public than it is to be an independent professional man. In the first place, save as a mental attitude, the independence of the practitioner is largely a fiction. Proud as he is of that independence, in reality he is throughout the twenty-four hours of the day, the servant and, indeed, the slave of the public. No one more so. We professors who appear to have greater freedom are servants of the university, and after long years of experience find that that service is not irksome. Nay, the more loyally we accept the yoke the greater do we find our freedom. And that is always so. We have but to regard the attitude and the standing of those foremost servants of the public, the officers of the army and navy. Where could we find greater self-respect, greater respect for those higher in authority, greater delight in loyal service for the good of the community than in those two services?

There is thus a beneficent future before the Federal Public Health Service. I would urge those before me to recognize that the development of a common health service for the whole Union is an end that all should work earnestly to attain—a service with trained officers in charge of every city and district throughout the length and breadth of the land; a service independent of politics; a "third service" with ideals every whit as noble, if not nobler, and aspirations equally lofty, if not more lofty, than those of the other two—the service of the army of the common weal.

And once again I would urge some at least of my hearers to think seriously of joining the staff and of becoming leaders in this army; of devoting their energies, their talents, and their lives to this great and beneficent department of preventive medicine.

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# SURGICAL TUBERCULOSIS.

BY

E. M. VON EBERTS, M.D., M.R.C.S. (Eng.)

Surgeon to the Out-patient Department of the Montreal General Hospital.

I would preface my remarks upon those clinical forms of tuberculosis which are included in the category of surgical affections, by expressing my thanks to the Executive of the Canadian Hospital Association, for their courtesy in extending to me an invitation to address this meeting.

During the past few years a vast amount of thought and energy has been expended by governments, federal, provincial and municipal, by charity organizations, by church societies and district visitors, by hospital authorities, both lay and professional, and by bacteriologists of note, in the perfection of methods for the detection and care of those suffering from pulmonary tuberculosis and for the protection of the community at large. As the impelling force in this vast movement had its origin in the recognition of the transmissibility of this disease, I have been impressed with the scant attention generally accorded tuberculous infections other than pulmonary, viewed in the light of their undoubtedly infectious nature.

The treatment of pulmonary disease depends for its success primarily upon the carrying out of certain hygienic and dietetic measures, and, owing to the vivid illumination of this phase of tuberculosis, the results achieved have probably been better on the whole than those obtained in the treatment of such surgical infections as have not been subjected to radical operative measures. I can not help thinking that this lesser success is a direct result of the failure to apply to the treatment of surgical infections the regimen prescribed for pulmonary cases,—rest, forced feeding and outside air.

As a starting point let us review the ward history of the average case of tuberculous hip-joint disease complicated with sinus formation. As soon as possible after admission to the general surgical ward, that is as soon as the routine connected with acute operative cases and the exactions of emergencies permit, the individual is skiagraphed and the necessary fixation or extension apparatus ordered by the attending surgeon: dressing of the sinus is carried out and the diet prescribed. Then follows an indefinite period of unavoidable delay while the apparatus is being selected or made, during which time the patient lies in the general ward and partakes with his fellows of the best air which the location and season or the ventilating equipment provides—air which is doubtless of a standard of purity sufficiently sustaining to the average individual not suffering from tuberculosis. If it be during the cold weather, the patient

is allowed to remain night and day in the public ward. At other times orders are given for him to be placed on the verandah during the day. To what extent are these orders carried out? Daily dressings are necessary, but, as we all know, in large general wards there is no fixed period at which this function is carried out, especially the dressing of chronic cases. Frequently the residue of daylight is too small to warrant the patient's removal to the gallery that day. In short, instead of the patient being brought in for dressing, he waits in for dressing. During the winter months, owing to the lack of proper protection, he objects to remaining out all day—probably the solitary occupant of the verandah, and the fresh-air order of the chief or house-surgeon is not infrequently waived in the face of these remonstrances. On the other hand, if the order is enforced, the nurse finds the exposure a hardship, especially where a two-hour temperature has to be given, and the practice is no doubt occasionally responsible for minor ailments among the nursing staff,—an additional influence tending to keep the patient indoors.

Again, if the discharge from the sinus or sinuses is profuse and curetting is necessary, for which an anæsthetic is administered, there is a temporary withdrawal of the normal food supply, and for some days, while in a lowered state of resistance, the patient is confined strictly to the ward.

The appetite gradually dwindles in spite of an extensive list of extras upon his diet card. Still later he becomes anæmic and a chalybeate is prescribed. Interest in his own progress perceptibly wanes until the chief joy of this "chronic" is the advent of an "acute" and only an ambulance case can rouse him to an elbow posture,—a state of mind which reflects very truly his depleted physical condition. In this way the winter months are dragged through,—with improvement, it is true, but an improvement which does not balance with our actual knowledge of how such cases should be treated nor our skill in treating them.

The patient whose history we have reviewed is much in the position of one of the impounded herd of Tolstoi's parable, in which a multiplicity of ukases enjoining the sowing of grass seed, the building of protecting sheds, the washing of udders and, finally, daily grooming, proved ineffectual in staying the gradual decrease in the milk supply, as the cardinal essential—the levelling of the palings—was withheld. What these animals required was fresh browse; what our patients require is outside air.

This element, air, is concerned in all forms of ventilation; it is to be found of a standard of purity suitable for therapeutic purposes, however, only on the outside of the four walls of the hospital ward. There it is

“outside air,” not to be enticed through the ventilators of double windows or forced through ventilating shafts. In other words “outside air” can not be imported.

How can we best eliminate in the conduct of cases of this kind, the odd ends which tend to invite failure or at least a postponement of recovery? I am of the opinion that the first step in this direction must be the provision of special and separate accommodation—an open pavilion or veranda equipped with canvas shields for protection against unsuitable weather conditions, where patients will be constantly in the fresh air. During the colder months dressings should be carried out in a heated apartment adjoining. The nurses in charge of the ward should be suitably clad for the season. An orderly should be always promptly available for the shifting of beds.

At night the patients should be moved into a comfortably heated ward, as it is probably Utopian to hope that the average individual may be induced to spend a winter's night in the open air, although I am personally convinced that with a proper equipment only comfort and an exalted feeling of well-being result from this procedure. As a matter of fact, to carry out open air treatment during the day in cold weather requires such a special equipment—that is an impervious mattress (preferably of felt), flannelette blankets instead of sheets, a Jaeger or four-point blanket, an eiderdown duvet and, occasionally, a hot-water bottle. The patient should wear flannelette pyjamas, warm socks, a warm bed-jacket and a light woollen tuque. It is very essential that the coverings should be light. A weight of bed-clothing is most irksome and detracts from the benefits of the treatment. If protection against wind is provided, the equipment described is ample for winter weather where the temperature is 10° F. or higher. Too much stress can not be laid upon the quality of the mattress. The patient can heat only one surface, and with most economy of heat energy that which lies beneath. Light coverings provide for proper body ventilation. The bed-clothing should be secured by means of blanket safety pins along one side and across the foot of the bed.

Under the plan proposed the serving of the mid-day meal during the colder months would be the chief obstacle. At this season breakfast and the evening meal could be served indoors, and with sufficient assistance I am satisfied that the serving of the mid-day meal would not present insuperable difficulties.

Every precaution should be taken in the disposal of infected material. How frequently one sees tuberculous material, such as caseating glands, joint curettings, or dressings from sinus cases, treated as ordinary in-

fect material and disposed of in the ordinary dressing tins, instead of being destroyed in the furnace! These casual methods show as little regard for the community, inside and outside, as the disposal of sputum by way of the sink.

In the regulation of the diet of this class of patient we are far in the wake of the physician. It is not enough to prescribe a liberal diet; it is necessary to see that the patient gets what is prescribed, that it is served in a palatable form, and finally that it is consumed. I am confident that a liberal Providence, through the medium of a generous public, leaves little to be desired in the quality of the raw food stuffs supplied to our hospitals, but the source of cooks is a moot question, and I have often felt on inspecting the product of his or her art, as served in the hospital ward, that the patients partaking thereof were trusting largely to the uncovenanted mercies. There are undoubtedly good cooks abroad in the land, but unfortunately Hospital Boards of Management are apt to hold that a high-priced cook is out of place in a charitable institution. Tuberculous subjects, above all others, require not only food in abundance but food which is properly cooked and served in an attractive form. These patients, in addition to three full meals a day, should be given at least half a pint of milk between breakfast and the mid-day meal, at three o'clock in the afternoon, and before lights out. Raw eggs may be added. Such an extensive diet can be accepted only by those who are constantly in the outside air.

In order to appreciate the effect of fresh air and liberal feeding, the patients should be weighed once a week, and, as suggested by Doctor Joseph Pratt of Boston, improvements in weight should be posted at regular intervals as an incentive to those who cavil at the forced feeding and out-door regimen.

While apart from operative measures the essentials in the conduct of these cases are rest, liberal feeding and fresh air, we have in tuberculin a valuable adjuvant. My personal experience has been chiefly with the use of that form known as Tuberculin Rest or the T. R. of commerce, administered in doses varying from 1/3000 to 1/800 of a milligram, according to the body weight, at intervals of ten days to two weeks,—the treatment extending over a period of six months to one year.

Where sinuses exist, there is always superadded pyogenic infection, which can best be combatted by the administration of an homologous bacterial vaccine.

With the expansion of a knowledge of the use of tuberculin there has been a marked diminution in the number of localized surgical infections subjected to operative interference. Until comparatively recently exten-

sive resection of tuberculous glands was practised as a routine method. At the present time a large proportion of these cases are selected for treatment by more conservative methods and with decidedly better results. In the treatment of joint infections the resections of yore have been largely replaced by the employment of fixation apparatus, the use of Bier's bandage, puncture followed by the application of Klapp's suction cups, and the routine administration of tuberculin. Tuberculous peritonitis is now less frequently treated by incision and drainage; rest in the open air and a liberal diet offering in the majority of these cases a less unfavorable outlook.

In the treatment of surgical tuberculosis it is only a question of time when our hospitals will have to grapple with the family side of the problem. Bread-winners will not progress favorably if their minds are not relieved as to the maintenance of those dependent upon them. Mothers also must know that their children are not being neglected.

When discharged from the ward all patients, whether receiving tuberculin or not, should be instructed to report regularly at the out-patient department, and those failing to do so should be referred to the district nurse for investigation. Cases of localized tuberculosis should not be allowed to return to the conditions under which the disease was contracted without an effort being made to discover and eliminate the source of infection or the predisposing factor, whether the latter be an undesirable occupation, insanitary housing, or a defective food supply. I would go a step further in expressing the opinion that all cases of localized tuberculosis should be reported. If such a process were legally enforced, these cases would be brought immediately under the eye of the civic authorities and the Tuberculosis League, and I am confident that in many instances evidence of infection in other members of the household would be detected. It is only by such careful supervision that relapses or metastases may be recognized early, that permanent cures may be effected, and that that millenium may be looked forward to when tuberculosis shall have become a comparatively rare affection.

There is nothing new in what I have put before you. The various ideas here assembled have all passed through the crucible of criticism and emerged as truths which may now be safely engrafted upon the Tree of the Art of Healing. The method of treatment outlined would, I believe, lead to a very material curtailment of the average time of retention of these patients—an achievement most urgently to be desired and yielding a three-fold blessing; a lessening of the tale of suffering; an earlier resumption of wage-earning, education or domestic duties; and a broadening of an institution's scope of usefulness.

# A STUDY OF ORGANIZED PLEURAL ADHESIONS AND THEIR RELATIONSHIP TO TUBERCULOSIS, BASED ON AN ANALYSIS OF 1374 CONSECUTIVE AUTOPSIES.

BY

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There has, of late years, been very considerable discussion regarding the diagnostic value of acute pleurisy as an indication of tuberculosis, there being those who hold that the majority of cases are tuberculous, while others dispute the point.

Pleural adhesions, it need scarce be said, have their starting point in an acute or subacute pleurisy, and following the suggestion of Professor Adami, I made a full analysis of 1374 consecutive autopsies performed at the Royal Victoria Hospital to determine, as far as the material permitted, the incidence of systemic tuberculosis and other relationships to pleural adhesions.

That material was peculiarly favourable for the purpose. In the first place it constitutes a larger series than any I have been able to encounter in the literature. The number of autopsies performed at the Royal Victoria Hospital is not great, averaging less than three a week, so that there is leisure for thorough study and record of individual cases; the whole series has been performed under the supervision, and according to the methods of one pathologist: these methods have, from the first, involved a conscientious tabulation of every recognized deviation from the normal, common conditions, such as pleural and peritoneal adhesions, being recorded with as much care as are the more obvious causes of death. The notes are made by a student or interne at the time of autopsy, and then dictated in a definite order to a stenographer. The material is that of a large general city hospital, consisting in the main of medical and surgical cases. The notes to be made regarding that material as an adequate or defective index of Canadian mortality are that the number of infants and children under twelve, admitted to the hospital, is well below the average of the general population; that chronic long-standing cases of disease gain entrance to the wards with difficulty, and that cases of active pulmonary tuberculosis are not knowingly admitted into the medical wards, save rarely, and then, as it were, under protest. The majority of cases coming to autopsy are derived from the medical and surgical wards: the gynæcological, ophthalmological and laryngological beds supply very few. In all these respects the hospital is a typical city hospital: the majority of patients come from the city of Montreal, but



a large minority gain entrance from the surrounding country. Some come from distant parts of Canada, and from across the border from the neighbouring states of Vermont, New York and New Hampshire. Lastly, as a seaport town, there is somewhat above the usual percentage of "outlanders" of both sexes.

In 1895, when the series of autopsies began, there were admitted into the Royal Victoria Hospital 1,841 patients, and in 1907 4,044 were admitted. In 1907 the average number of patients in the Hospital per diem was 224, of which 74 were medical, 95 surgical, 28 gynæcological, and the remaining 27 are accounted for in the ophthalmological, laryngological and otological wards.

I have carefully gone through the notes of the 1,374 cases, and before discussing my results, it is necessary to call attention to the limits to be placed on the accuracy of the record. In the first place only organized adhesions are taken into account. Of these one group is not included in the table, and this because I could not convince myself that they had always been noted: I refer to interlobar adhesions. The time for viewing pleural adhesions in general is when making the preliminary inspection of the thorax. When interlobar adhesions alone are present, these may easily be overlooked at this period, and fail to be recorded later. Secondly, it must be kept in mind that the diagnosis of tuberculosis has been based purely on naked eye examination. It is true that in a large percentage of cases in the routine examination of sections of, on the average eight, major organs of the body the diagnosis has been confirmed under the microscope and again in a smaller proportion by bacteriological methods. This table, however, is based upon gross appearances. Those tubercles, invisible to the naked eye, in the cervical and other lymph glands, have not been taken into account. Similarly, mere puckering of the apex of the lung, with no other gross sign of tuberculosis, has been ruled out from our main tables of tuberculosis, unless associated with this there were definite underlying calcified or fibroid tubercles. The minute subpleural fibroid tubercle-like bodies, which Hodenpyl regards as true arrested tubercles, have also been placed under a separate column. We are inclined to believe that both of these conditions are indications of old pleural tuberculosis, but as they represent border line conditions, have thought it wiser to place these cases in a definite category, along, we would add, with those cases in which rare solitary small shot-like calcareous bodies in the liver or spleen have been the only tubercle-like lesions discovered.

Yet another point deserves attention. It is evident and natural that certain observations at first made perfunctorily, are later pursued more

keenly, as their importance becomes more fully realized. Thus, for example, in the early half of this period, when attention had not been drawn to the frequency of a solitary focus of tuberculosis in the peribronchial glands, or more particularly in the gland cluster at the bifurcation of the trachea, it is probable, nay certain, that the presence of the disease there was not infrequently overlooked. To the more frequent detection of obsolete tuberculosis in these regions, we would, in the main, ascribe the difference between our figures and those contributed to this Congress by Dr. J. McCrae: our series embraces some four hundred more recent reports, which are not included in his analysis.

It goes without saying, that our figures are well below what we are convinced is the actual amount of tuberculosis in our material. Lastly, the notes have permitted us to recognize the following main orders of pleural adhesions:—

- (1) Generalized, affecting one or other lung.
- (2) Scattered or sporadic.
- (3) Apex free, but occasional adhesions below and in front.
- (4) Adhesions separated with difficulty.
- (5) Those that the slightest touch would destroy.

Next, as to the results obtained.

I. *Frequency of old pleural adhesions.*—Of the 1,374 cases, 990 or 72.1 per cent. are recorded as exhibiting adhesions of one or other degree. 384 or 27.8 per cent, as free from adhesions. These figures, I may note, are closely in accord with those determined by Lord, of Boston. In a smaller series of 215 autopsies (about  $\frac{1}{5}$  of ours), he found 74.4 per cent. of adhesions. In other words, *in the North Eastern portion of North America, it may safely be laid down that seven out of ten adults exhibit indications of a previous pleurisy.*

II. *Age incidence.*—I hope later to publish this table in full. Here I would merely quote McCrae's figures: viz., that the average age of case coming to autopsy was 46 for the first 1,000 cases, and there is no reason to believe that our next 374 cases would change this average to any material extent.

III. *Relationship of systemic tuberculosis to old pleural adhesions.*—Of this series of 1,374 cases, 558 (40.6) afforded definite macroscopic evidence of tuberculosis in the thoracic cavity. 68 other cases showed evidence of tuberculosis, according to our classification, either in the mesenteric glands, liver, spleen or bones.

Thus summing up, we have undoubted tuberculosis in thorax:—558 cases (40.6 per cent.). Cases showing tuberculosis elsewhere than in the thorax, 68 cases, or 626 cases of tuberculosis in 1,374 cases (45.5 per cent.).

If fibrosis of the apex with no other macroscopic lesion be counted as tuberculous (89 cases), we then have 715 cases of tuberculosis, a percentage of 52.

Here it must be understood that we deal with obsolete or obsolescent tuberculosis, or again with progressive tuberculosis of a more chronic type. It is scarce necessary to point out that acute miliary tuberculosis, devoid of any sign of an older focus of primary infection, is not here taken into consideration, and that because an acute tuberculosis cannot account for old pleural adhesions.

A. *Cases of tuberculosis (other than acute miliary), showing no old pleural adhesions.* — Of the 384 cases showing no adhesions (27.8 per cent. of total), evidence of tuberculosis was found in 103 cases (26.8 per cent.). In other words, in cases without adhesions, 26.8 per cent. have suffered to some degree from tuberculosis. Of these, the site of the tuberculosis was in the thorax in 82 cases (21.3 per cent.), not including 9 which showed merely apical fibrosis, and the tuberculosis was elsewhere (alone) in 21 cases.

In other words, as might be expected, not all cases of thoracic tuberculosis are accompanied by pleural adhesions.

It must be obvious to all that the 27.8 per cent. does not represent merely those that never suffered from pleurisy, for there may be perfect resolution of an acute pleurisy, no adhesions being formed, nay more, we have to recognize, and in this view we are confirmed by experiments on the dog, that organized adhesions may undergo eventual absorption. On the other hand, it is worthy of note that characteristically the more extensive the pulmonary tuberculosis the more common are the pleural adhesions, and that absence of adhesions especially characterizes limited and obsolete tuberculosis, and more particularly these cases in which the peribronchial or peritrochial lymph nodes alone are recognizably involved.

B. *Cases of tuberculosis that were accompanied by old adhesions.*— Of 626 cases of tuberculosis or 45.5 per cent. of the 1,374 cases, 523 showed adhesions, or 83.5 per cent., as against 990 cases of adhesions, in which 523 showed tuberculosis variously distributed (52.3 per cent.).

If, on the one hand, 103 cases showed evidence of tuberculosis with no coincident adhesions, there were, on the other hand, 523 or 83.5 per cent. of cases of tuberculosis with adhesions, or, roughly, five cases of tuberculosis are accompanied by adhesions to every one that shows none. The only figures bearing upon this point that I can refer to are those of Dr. J. McCrae, communicated to this Congress, based upon the first thousand of this series from which similar conclusions are drawn. (Out of

these 1,000 cases, Dr. McCrae found 24 without adhesions, but with pulmonary tuberculosis, 11 with tuberculosis of the thoracic nodes lymph nodes, 6 with tuberculosis of both lung and thoracic nodes. He does not consider the cases in which the only recognizable tubercular lesions were outside the thoracic cavity.) He concludes that out of 239 cases of early (or more accurately slight obsolete and latent) tuberculous infection, 50 cases show no direct relationship between the lesion and pleural adhesions, or, roughly, nearly one out of every five.

There is another and converse ratio to be determined, namely, the proportion of cases of old adhesions that afford coincident indications of tuberculosis in some region of the body. Of the total number of 990 cases of adhesions, 523 showed tuberculosis, or 52.3 per cent. *Or to put this clearly, while five out of every six cases of visible tuberculosis are accompanied by adhesions, only one out of every two cases of adhesions exhibits obvious coincident tuberculosis.*

This is a very striking result, and one that must be taken into serious account in the estimation of the diagnostic value of pleurisy in general. If we can draw any conclusion from the analysis, it is that there are two roughly equal groups of cases of pleural adhesions and presumably, therefore, of pleurisy: one in which there is associated tuberculosis; the other in which all naked eye indications of that disease are wanting.

What value is to be placed upon this conclusion? Can it be said with any plausibility that they cannot be accepted: that in the cases showing no tuberculosis there had been a primary tuberculosis which had undergone complete absorption, the pleural adhesions being the sole remaining indication: or again that microscopical examination would have revealed tuberculosis in these cases and that the naked eye diagnosis is valueless? This second argument may be dismissed rapidly. It has been shown that the adhesions are characteristically related to the extent of the tuberculosis and that small foci, but still recognizable to the naked eye, are time and again present with absence of adhesions. We may safely conclude that a tuberculosis so slight and so latent as not to reveal itself to the naked eye is little likely to be the cause of the often extensive adhesions met with in this group of cases. And as regards the first argument it is difficult to believe that the absorptive process sufficiently active to remove all obvious foci of the specific disease, would not, in general, remove even more effectively these non-specific organizations.

Making every allowance, it is still, I am inclined to think, impossible to escape the conclusion that there are approximately two equal orders of cases of pleural adhesions.

How are we to harmonize these results with those that have been gained

by clinicians and bacteriologists bearing upon the tuberculous nature of the majority of cases of clinically recognizable acute pleurisy? I am of the opinion that if they cannot be reconciled, at least, an explanation is possible of the divergence.

Let us note first that clinically recognizable acute pleurisy is a relatively rare event, compared with the occurrence of pleural adhesions. To give an example: at the Massachusetts' General Hospital, Lord, out of 18,543 patients examined, found that acute pleurisies had been diagnosed only in 2.4 per cent. of the cases. Osler quotes a series of 19,396 cases at the Pennsylvania General Hospital, as affording 505 cases of this condition, or 2.6 per cent. Similarly, Würzburg clinics in thirteen years had an incidence of 3.4 per cent., although the Charité, at Berlin, in 8 years gave 9 per cent. Compare these figures with our 72.1 per cent. of observed old adhesions in 1,374 autopsies. Clinical notes are generally silent where the adhesions over one or both lungs are at autopsy found dense or universal.

The conclusion is inevitable that only a small proportion of cases of pleurisy in the acute stage yield clinical symptoms. It is this proportion, I would suggest, that is represented by the cases of acute pleurisy with effusion. We must freely accept such figures as those of Aschoff, that 68 per cent. of the animals inoculated from cases of serous pleurisy succumbed to tuberculosis, and even those of Le Damany, that 86 per cent., or even 96 per cent. of the exudates in such cases induce tuberculosis in the animals of the laboratory.

We admit thus that acute serous pleurisy is most often of tuberculous origin—and that it leaves adhesions behind it. But just as in the peritoneum we recognize that adhesions may follow acute inflammation set up by *B. coli* or streptococcus infections, just as we believe that appendicitis with its subsequent adhesions is by no means necessarily of tuberculous origin, so here on broad general principles we must accept these figures and the conclusions to be derived from them that *pleural adhesions are surely of more than one origin, and that they fall into two groups: the tuberculous and the non-tuberculous.*

THE

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## THE CAUSATION OF SEX.

Dr. E. Rumley Dawson has recently published a book from the house of Mr. Lewis, London, in which a new theory of the causation of sex is put forward. It must be admitted that the book is written in a spirit of polemic.

It is dedicated to the memory of "a medical martyr," and is inscribed with the legend, "there is nothing more thankless than the attempt to influence any field of public opinion." This is too gloomy. Indeed, it is not true. To become a martyr does not prove that a man is right. Dr. Dawson's claim that he has "discovered Nature's secret," must be determined not by the persecution at which he hints, but by other considerations entirely.

This new theory is that the sex of the foetus is not due to the male parent, but depends on which ovary supplied the ovum which was fertilised and so became that foetus. It is claimed that a male foetus is due to the fertilisation of an ovum that came from the right ovary and a female foetus is due to the fertilisation of an ovum that came from the left ovary. The book contains an amplification of this view and maintains that the male parent has no influence whatever in the causation of sex, which rests entirely with the female.

According to this theory the female has in the right ovary ova which are already sexed male, and in the left ovary ova which will produce only females after fertilisation. So far as we are aware this is the first occa-

sion upon which the male has been entirely dissociated from any influence in determining the sex, though there has always been a tradition that the right side was in some way concerned with the production of males, and the left with the production of females. It was the opinion of Hippocrates that the secretions from the right sides of both parents produced males and Galen offered the explanation of this phenomenon that the right side was warmer. Upon this assumption various mechanical devices have been suggested to govern the sex from the time of Avicenna to that of a writer in the "*Lancet*," in 1870, who signs himself T. B. The case for the right side, however, is not strengthened by the citation of the fact that men have better sight with the right eye, women better with the left, that men have the buttons on the right whereas women have them on the left side of their clothes, and that men usually put their right arm first into their coats whilst women usually begin with the left. In the desire to be comprehensive, something of the trivial has been admitted.

The evidence is both precise and voluminous, and is clearly presented. A series of cases is given in which ten women gave birth to 48 female children and no males, each woman having had two husbands. This is followed by a series in which only males were produced, the conclusion being that the women were unilaterally sterile. There is a body of anatomical evidence to show that menstruation takes place from each side alternately accompanied by the discharge of a male and female ovum respectively, and the results of operations for extra-uterine pregnancy are cited to prove that the foetus on the right side is always male and that on the left side female. The results of child-bearing after operation for the removal of one ovary would appear to yield confirmatory evidence. Dr. Dawson also affirms that in the bicornuate uterus the sexes occupy different sides. This view is now before the medical profession and it will doubtless be subjected to that relentless analysis which all new theories invite.

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#### THE PROPOSED MEDICAL ACT.

The draft of the Medical Act of 1909 has been published, and while it is yet under consideration we might point out the manner in which the Province proposes to carry out our reciprocity agreements with the United Kingdom. Section 78 sets forth that a graduate of a university of France, upon showing evidence of having passed a proper preliminary examination may try his final examination for a license. Section 79 then goes on to say what follows:—

79.—Those whose names are inscribed in the Medical Register of the

United Kingdom of Great Britain and Ireland in pursuance of Imperial Medical or other acts amending the same shall be entitled, upon producing sufficient proof of such inscription and of their good conduct, and upon paying the fees then exigible for the obtaining of the practising license, to such license, without having to undergo any examination; provided they establish to the satisfaction of the Provincial Medical Board:

(1) That they have obtained from the Provincial Medical Board a "Brevet" or certificate of admission to the study of medicine, five years at least before their inscription in the Medical Register of the United Kingdom.

(2) That they were registered in the Medical Register of the United Kingdom and became qualified to exercise their profession in the said United Kingdom in the course of a period of not less than five years, during which time they must have resided without interruption in the United Kingdom.

We need scarcely say that should the enactment be made thus, the entire idea of reciprocity will be defeated.

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#### LARGE WHITE OR SOAPY KIDNEY.

Dr. Oskar Klotz has published from the laboratory of the Royal Victoria Hospital in the "Journal of Medical Research," an important contribution entitled "On the Large White or Soapy Kidney." With him were associated in the work of investigation, Professor Adami and Dr. Bruère, to whom due recognition is made. The essence of the contribution is contained in the last paragraph in which Dr. Klotz explains, "that the large white kidney which we have usually considered as the fatty kidney is in reality a soapy kidney, and that part at least of the myelins contained therein consists of soaps of oleic acid." These fatty compounds are not readily demonstrated by the ordinary staining with Sudan III. They may, however, be recovered by chemical methods, and appear to be some of the so-called myelins. The communication is an interesting and useful one, not only as indicating a misapprehension under which we have labored with regard to the form of kidney lesion in question, but also as casting light upon the composition of the myelin bodies about which so little has been known. We are glad that this work of Dr. Klotz, as well as the previously published Harvey lecture of Professor Adami upon the myelins, came from our own laboratories.



## Reviews and Notices of Books.

SCHULTZE, LEHRBUCH DER HEBAMMENKUNST, VIERZEHNTE, VERBESSERTE AUFLAGE, mit 103 ABBILDUNGEN. Leipzig, 1908, Wilhelm Engelmann.

The first edition of Schultze's Manual for Midwives appeared in 1860; for 48 years it has been the standard work of its kind and has gone through 14 editions. The prefaces to eight of the editions are given, and enable us to judge how greatly the instruction given to midwives in Germany has improved in half a century. Many medical students use Schultze's book as a short compend of the essentials of practical obstetrics. It seems to have been brought fairly well up to date, and for the most part the directions given are clear and sound. Some new illustrations have been added from the text-books of Bumm and Piskacek. It is a pity that figures 57 and 58 illustrating the method of supporting the perineum in the dorsal and lateral positions have not been omitted; modern practice is decidedly opposed to such manipulations as being useless and sometimes harmful. Schultze's swinging for the resuscitation of asphyxiated infants, as shown in figures 93 and 94, is far too dangerous a method to recommend to midwives. The simpler measures in general use in this country are better and safer. Exception must be taken also to the Douche Nozzle shown in figure 101; it has the grave defect of having an opening at the point, and unless used with great care might readily do damage.

APPLIED SURGICAL ANATOMY. For the use of students and practitioners of medicine, by GEORGE WOOLSEY, A.B., M.D., second edition; enlarged and thoroughly revised, with 200 illustrations, including 59 plates, mostly coloured. Lea & Febiger, New York and Philadelphia, 1908.

The increasing number of works upon this subject and the newer editions of the older works, show that teaching bodies as well as practitioners have come to recognize the necessity for their existence. Woolsey's Surgical Anatomy is well printed and bound, and a credit to his printer. The author has not hesitated to borrow freely in his illustrations from the well known works on anatomy, viz: Meckel, Zuckerkandl, Gerrish, and others; he has also added many excellent radiographs. The days of the "cut and tie" surgeon are fortunately disappearing. Surgical applied anatomy is becoming more and more popular as a compulsory course in the medical curriculum, and it is well that it is so. Professor Woolsey is entitled to our thanks for a very good book.

J. A. S.

700 SURGICAL SUGGESTIONS. By WALTER M. BRICKNER, B.S., M.D., ELI MOSCHCOWITZ, A.B., M.D., HAROLD M. HAYS, M.A., M.D. Third edition. The Surgery Publishing Co., 92 William Street, New York, 1909.

This little work contains not only the pith of the larger surgical works, but also the result of careful clinical observations which are, and can only be, picked up almost unconsciously as the result of long varied and faithful hospital experience. It is written in concise, terse, epigrammatic paragraphs which almost speak to the reader from its pages as the clinician does. It is thoroughly up-to-date and is concisely indexed under the various regional and subjective headings which is invaluable to the busy general practitioner, serving him accurately as a ready reference handbook upon questions of diagnosis, clinical signs and symptoms, prognosis and treatment, of exactly the very cases he is dealing with and aiding him in a practical way in the various difficulties which confront him from day to day.

THE OPERATIONS OF AURAL SURGERY, TOGETHER WITH THOSE FOR THE RELIEF OF THE INTRACRANIAL COMPLICATIONS OF SUPPURATIVE OTITIS MEDIA. By C. ERNEST WEST, F.R.C.S., Aural Surgeon to St. Bartholomew's Hospital, and STONEY R. SCOTT, M.S., F.R.C.S., Assistant Aural Surgeon to St. Bartholomew's Hospital. H. K. Lewis, London. With fifteen plates and other illustrations. Demy 8vo. 7s. 6d. net.

The reasons for writing this book, the authors explain, are:—

1. The lack of anything like sufficient detail on the subject of operative procedures in the works on diseases of the ear hitherto published.
2. They have elaborated, in their practice, a number of original methods which having been found of such value by actual experience, they have taken this method of introducing them to others.
3. The want of an account of the methods which they themselves follow for the use of their students and house-surgeons.

In the introduction the subject of nystagmus is dealt with at some length with the different tests for labyrinthine disease, in connection with which some original observations are recorded. Among the new operations are opening of the labyrinth, styled by them "Vestibulotomy" and the operation for the drainage of the meninges, a procedure which, though adopted in a smaller number of cases, has been attended by sufficient success to warrant the hope that by such means, a proportion of the cases of otitic meningitis may be saved by prompt operation and that a condition hitherto beyond cure may be rendered amenable to treatment.

Limited space precludes more than a brief mention of these operations; for a further description the reader is referred to the volume under review.

The remainder of the book is devoted to the various operations in aural surgery, detail not being lacking; an appendix in which 46 operative cases are tabulated brings this instructive and interesting little volume to a close.

W. H. J.

THEORY AND PRACTICE OF INFANT FEEDING. By HENRY DWIGHT CHAPIN, A.M., M.D., New York. Third edition. Revised. William Wood & Co., Publishers, New York, 1909. Cloth, \$2.25 net.

It is but a little over four years since the second edition of Dr. Chapin's book appeared, that now a third edition is demanded shows the continued popularity of this extremely useful little work. This third edition has been revised and the section on practical feeding has been greatly improved. In this section the author discusses the principle of "top milks"; very simple rules being given for the employment of this means in obtaining a percentage modification.

Section thirty-four, dealing with standardized gruels, is completely new. The effect of these gruel diluents is to soften the curds and thus allow the digestive tract more easily to perform its function. The author in this section endeavors to establish cereal feeding on a scientific basis. He has had a number of different varieties of gruels analysed to determine their composition particularly as regards the relative proportion of tissue-building and heat and energy-producing elements they contain. He has thus succeeded in compiling interesting tables giving the approximate percentage of gruels made from cereals and from flours. This portion of the book is extremely interesting and will repay careful study.

With regard to alkalies the author states that when it is desired to prevent or retard the action of the gastric secretion on the milk, then an alkali is indicated; but for the majority of infants it is desirable and proper to let the stomach perform its function and increase in digestive capacity.

Chapter twenty-two on the "Preparation of Food" is practically new, having been re-arranged and much new matter added. In it whey and cream mixtures, percentage composition, and peptonized milk are dealt with.

In chapter twenty-three, which is also new, the author takes up the subject of foods for difficult cases and for temporary use. For instance,

he gives a series of foods suitable for cases that fail to thrive on fresh milk modifications. References are made in this chapter to condensed milk mixtures, and whey mixtures.

It is a matter of surprise that the author has not said more about buttermilk, having given but two paragraphs in section 153 to the discussion of its employment in infant feeding. He states that it is suitable only for temporary use, which is surprising when one considers the mass of literature which has recently appeared lauding the use of buttermilk in infant feeding.

Formulae for the preparation of food mixtures containing no milk are given in full, suitable for cases where temporary milk intolerance is present. These additions will easily be seen to add not a little to the practical value of Dr. Chapin's interesting and useful manual on infant feeding. It is a work which is undoubtedly one of the most valuable contributions on the subject, and should be carefully read by those called upon to prescribe food mixtures for infants and young children.

**MODERN MEDICINE. ITS THEORY AND PRACTICE.** In original contributions by American and foreign authors. Edited by WILLIAM OSLER, M.D., Regius Professor of Medicine in Oxford University, England; formerly Professor of Medicine in Johns Hopkins University, Baltimore; in the University of Pennsylvania, Philadelphia, and in McGill University, Montreal. Assisted by THOMAS McCRAE, M.D., Associate Professor of Medicine and Clinical Therapeutics in Johns Hopkins University, Baltimore. In seven octavo volumes of about 900 pages each, illustrated. Volume V, Diseases of the Alimentary Tract. Price per volume: cloth, \$6.00, net; leather, \$7.00 net; half morocco, \$7.50 net. Lea & Febiger, Publishers, Philadelphia and New York, 1908.

The fifth volume of this work deals with diseases of the alimentary tract, and is a book of 903 pages, uniform with the preceding ones. The volume very properly begins with a general discussion of the physiological pathology of the processes of secretion by Dr. Charles G. Stockton, of Buffalo. While the title proper is an introductory discussion on the diseases of the digestive apparatus, this takes the form of a brief and readable survey of the most important facts connected with digestion. The article is properly placed, and well written; an occasional lack of definiteness in the paragraph titles is not reflected in the text; for example, 'The Question of Secretion' scarcely conveys enough meaning. These twenty pages well reward the time employed in reading them. Dr.

David Riesman, of Philadelphia, contributes the article upon diseases of the mouth and the salivary glands, and covers much ground in a series of concise paragraphs; such an article as this is eminently suited to the needs of a practitioner. Dr. John McCrac, of Montreal, contributes the paper upon the diseases of the œsophagus, and Dr. Julius Friedenwald, of Baltimore, that upon the functional diseases of the stomach. The writer deplors the very unwieldy classification of these diseases that at present exists, but must of necessity employ it. The greatest objection that the practitioner can offer to this article is that he cannot tell which of the many subdivisions is the one he requires, unless he be fairly well read on contemporary literature of the diseases of the stomach. We admit that we do not see how Dr. Friedenwald can help this; rather are we all the victims of a refinement of distinction that has become extreme—there are too many functional diseases, so-called, of the stomach, and the lack of variety in the treatment of the various forms of functional derangement shows that no one appreciates this more than the writer.

Dr. C. F. Martin, of Montreal, deals with the organic diseases of the stomach. Gastric and duodenal ulcer and cancer occupy seventy-five pages; the importance of the surgical treatment of these diseases is fully recognized, as is fitting at the present time. In writing upon acute gastritis the author has evidently experienced the difficulty that exists in determining what cases of gastric derangement are cases of acute gastritis, and has adopted a very conservative standpoint. The same is true of his treatment of chronic gastritis, for there is a tendency to use these terms loosely in describing cases where no lesion exists. Gastrectasis, however, rests on a surer basis, and its treatment is especially well described. After the discussion of the comparatively rare diseases congenital pyloric stenosis and cirrhosis of the stomach, there is a practically useful discussion of hæmatemesis.

The diseases of the intestines are dealt with by Dr. Alfred Stengel, of Philadelphia, and this forms an excellent chapter. The physiology of the intestines, of intestinal mobility and of the fæces are followed by a comprehensive but not long description of the bacteriology of the tract. Various deviations from the normal such as hæmorrhage, colic, etc., are followed by a useful chapter on constipation. The various forms of diarrhœa, of enteritis and colitis are given, and a discussion of appendicitis, sensible at all points, follows. The unwieldy subjects of ulceration and obstruction are taken up in a thoroughly adequate way.

Dr. H. D. Rolleston, of London, takes up the diseases of the peritoneum and, as would be expected, produces a highly-finished article,

replete with a knowledge of the literature of the subject and with abundant references. The parts dealing with subphrenic abscess and tuberculosis of the peritoneum are to be specially commended. Splanchnoptosis is dealt with by Dr. Thomas R. Brown, of Baltimore, who is well qualified to write upon a subject which has engaged his attention for a long time. As perhaps might be expected, his convictions tend towards the recommendation of medical rather than surgical treatment in the majority of cases. When it is stated that the diseases of the pancreas are in the hands of Dr. Eugene L. Opie, of New York, it will be admitted that no one is better qualified, and the article bears all the marks of first hand information.

The wide subject of the diseases of the liver and of the gall bladder and biliary ducts is in the hands of Dr. A. O. J. Kelly. It occupies less than 175 pages, and the writer has done well in expressing so much in so comparatively small a space. We heartily agree with Dr. Kelly's adoption of the most simple classification that is possible of the cirrhosis of the liver, and pages 757 and 758 contain a very clear exposition of the truth about some of the vexed points. We do Dr. Kelly's article less than justice in saying that it is good. In fine, volume V of the system is first class.

A TREATISE ON THE PRINCIPLES AND PRACTICE OF GYNÆCOLOGY. By E. C. DUDLEY, A.M., M.D., Professor of Gynæcology in the Northwestern University Medical School, Chicago. Fifth edition, thoroughly revised. Octavo, 806 pages, with 431 illustrations, of which 75 are in colors, and 20 full-page colored plates. Cloth, \$5.00 net; leather, \$6.00 net; half-morocco, \$6.50. Lea & Febiger, Publishers, Philadelphia and New York, 1908.

Five editions of Dudley's Gynæcology in ten years is a remarkable showing for the popularity of a text-book. Those who have known this book and recommended it to students did not need this piece of evidence to commend it to them. As the publishers say, "ability to live and thrive despite competition, which is surely abundant" indicates a strong book. In his introductory chapter the author puts forth a strong plea for the existence of gynæcology as a specialty on the grounds that to the gynæcologist was due the first of the modern extensions of abdominal surgery and to the indubitable fact that a long special training in physical diagnosis is necessary to make the safe and successful practitioner in the diseases peculiar to women. These opinions were put forward in the author's presidential address at the Annual Meeting of the American Gynæcological Society in 1905 and were intended to combat an idea be-

ginning to prevail in certain quarters that gynæcology is being merged into general surgery.

There is probably no department of medicine in which so many text-books are written and this book of Dr. Dudley's is certainly one of the best. One important feature of the work claimed by the author to be unique is the consideration together of the infections of the organs and tissues concerned. Thus, vulvo-vaginitis, metritis, salpingitis, oophoritis, peritonitis and cellulitis are considered in succession. Infections of these organs and structures may exist separately or, in a severe case, all may coexist from extension. This method of dealing with the subject we believe to be a distinct advantage over the more usual way of considering all the affections; for instance, infections, neoplasms, displacements, of an organ before proceeding to those of another organ or structure. In noticing a new book, or a new edition of an old book, one naturally turns to the author's discussion of rival methods of procedure to see what he has to say. First, as regards the abdominal and vaginal avenues of approach for operation; after a full discussion of the merits and advantages of each, the author very wisely concludes that "the operator should not permit his prejudice of either route to lead him to pursue it to the extreme, for that part of an operation which is easy by the vagina is often more difficult by the abdomen and *vice versa*." And another debated or debatable question is the modern radical abdominal operation for cancer of the uterus. Here again the author's attitude is remarkably judicial and conservative. Dr. Dudley's book is very copiously illustrated. Obsolete illustrations of previous editions have been eliminated and many new original drawings have been added.

W. G.

STUDIES IN PARANOIA. — Nervous and Mental Disease. Monograph Series No. 2. By N. GURLICH, M.D., and M. FRIEDMANN, M.D. Translated and edited by Smith Ely Jelliffe, M.D., New York, 1908.

This, the second number of this excellent series of monographs, is a worthy successor to its forerunner already reviewed in these pages. It takes up some of the less common forms of paranoid conditions, especially those forms without delusions having a favourable termination. *Ipsa facto* these cases would not be included among the paranoias by Krepelin, but clinically and practically they cannot be distinguished, save perhaps from the fact that there are no delusions—which in the early stages of the disease at least would be an uncertain factor to base a prognosis on. They are well worth reading, the translation being fluent and easy.

**SUGGESTIVE THERAPEUTICS; APPLIED HYPNOTISM; PSYCHIC SCIENCE.**  
By HENRY S. MUNRO, M.D. Americus Georgia, second edition.  
Published by C. V. Mosby, Medical Book and Publishing Company,  
St. Louis.

It is with a distinctly pleasurable disappointment one reads this book, the appearance of the pages with the emphatic words in large heavy type suggest the sensationalism one too often finds in treatises on this subject. It is therefore delightful to find a reasonable, scientific method of dealing with this subject, and if one makes allowance for a certain amount of enthusiasm, recognizing it as a necessary adjunct in the successful practice of psychic science, one reads the book not only with interest, but with considerable profit.

**DISEASES OF THE EYE.** By STEPHEN MORGAN, F.R.C.S. Henry Frowde,  
Oxford University Press, 1908.

The author in his preface presents this little work as one intended to help those who are beginning the study of ophthalmology, or as a short work of reference for the general practitioner. As such this volume will certainly serve its purpose well. It is a clear, concise presentation of the subject following the generally accepted ideas, although several of the more recently introduced methods with which the author has had practical experience are included.

The section on diseases of the fundus oculi is dealt with rather briefly, as in the writer's opinion those who are sufficiently advanced in the subject as to be able to use the ophthalmoscope should consult larger works for reference. Elementary optics and refraction are treated in an unusually interesting manner, the brevity of description assisting naturally in an intelligent appreciation of a rather difficult branch of ophthalmology.

One point in particular in which the work is to be commended is the admirable and liberal manner in which the book is illustrated. Mr. Morgan as a pathologist has appreciated his particular advantage over writers of the majority of small text books, and, in addition to a clinical picture of diseased conditions, has prepared a large series of microscopic sections which very naturally assist in demonstrating the morbid changes occurring in the various ocular disorders. The way in which the book is printed and the number as well as the quality of the illustrations would do credit to a more pretentious volume.

An appendix giving a number of the commoner prescriptions used in eye surgery, as well as the indications calling for their employment, lends an added value to this little book. The work will be welcomed by the



student who wishes a brief yet intelligent conception of ophthalmology before he has occasion to resort to the larger treatises.

F. T. T.

EPOCH-MAKING CONTRIBUTIONS TO MEDICINE, SURGERY, AND THE ALLIED SCIENCES, Collected by C. N. B. CAMAC, A.B., M.D. The W. B. Saunders Company, Philadelphia and London, 1909.

This book contains a series of reprints of those communications which first conveyed most important observations to the scientific world, together with biographical sketches of the observers, with portraits. It is dedicated to Lord Lister, apparently at the recommendation of Professor Osler according to the facsimile letter which forms the frontispiece. The papers which are printed are: "On the Antiseptic Principle of the Practice of Surgery," by Joseph Lister; "Circulation of the Blood," by William Harvey; "On Percussion of the Chest," being a translation of Auenbrugger's treatise by John Forbes, M.D.; "Auscultation and the Stethoscope," by R. T. H. Laennec; "Vaccination Against Small-pox," by Edward Jenner; "Anæsthesia," by W. T. G. Morton; "Puerperal Fever," by O. W. Holmes.

So far as we are aware this is the first occasion upon which the original accounts of the seven cardinal discoveries in medicine have been brought together, and the wonder is that the happy inspiration did not impel some one to do it sooner. There is nothing more important for the student than to go to original sources and this book affords him a ready method. The portraits add to the interest and the biographies are comprehensive and accurate. The book will be a refreshment to all who have much to do with the reading of modern books on medicine.

DISORDERS OF THE BLADDER with Technique of Cystoscopy, by FOLLEN CABOT, M.D., Illustrated. New York, E. B. Treat & Company, 1909.

A book which aims to teach the technique of cystoscopy to the general practitioner is likely to fail. Whilst we may admit that "the recent improvements and reduction in cost of the instruments have brought them within the reach of all," it does not follow that "all" should use them. The practice of cystoscopy is one of the most special in surgery and should only be exercised with great caution to make it safe for the patient or useful to the practitioner. There is, however, much other information in the book dealing with urinary conditions, all well arranged and presented.

**BACTERIAL FOOD POISONING**—the So-called Ptomaine Poisoning, by PROF. DR. A. DIEUDONNÉ, translated and edited with additions by DR. CHARLES FREDERICK ROLDUAN. New York, E. B. Treat & Company, 1909. Price, \$1.00.

Prof. Dieudonné's book on Bacterial Food Poisoning was published less than a year ago and this is the first translation which has been made. The editor has added a description of additional outbreaks of food poisoning in America and has slightly rearranged the material. The book which contains 128 pages is divided into ten sections. A transcription of the titles of these sections will give some notion of the extensive contents of this little book. These headings are: Poisoning Through Fish and Molluscs; Poisoning Through Cheese; Poisoning Through Ice Cream and Puddings; Potato Poisoning; Poisoning Through Canned Goods; Metallic Poisons; Bibliography. This last is especially valuable. The book is a thoroughly scientific one and interesting as well.

**PROGRESSIVE MEDICINE**: Vol. XI, No. 1, March 1st, 1909. Lea and Febiger, Philadelphia.

The contents of this volume are: The surgery of the head, neck and thorax, by Charles H. Frazier, M.D.; Infectious diseases, including acute rheumatism, influenza and croupous pneumonia, by Robert B. Preble, M.D.; The diseases of children, by Floyd M. Crandall, M.D.; Rhinology and laryngology, by D. Braden Kyle, M.D., and Otology, by Arthur B. Duel, M.D. The number contains 270 pages and is a complete record of the progress in the various divisions of medicine which it deals with. We note a reference to Dr. Browne's clinical observations on epidemic Cerebro-Spinal Meningitis, in this JOURNAL, and to Dr. Blackader's tabulation of cases observed in the Montreal hospitals, also in this JOURNAL. We note also a reference to Dr. White's case of death from Typhoid Fever due to excessive toxæmia without ulceration of the intestine. These illustrations are given to show how well the field has been gleaned. The section on Otosclerosis is especially interesting. This series is most valuable and the present number is one of the best we have seen.

**PRACTICAL DIETETICS**, by ALIDA FRANCES PATTEE. Fifth edition. A F. Pattee, publisher, New York, 52 West 39th Street.

On at least three occasions we have mentioned this book and always with praise. It remains now merely to call to the attention of both nurses and physicians that a fifth edition of this very practical Dietetics has just appeared. Many of the prescriptions, or recipes, as one should say, are as alluring to the well as to the sick.

MANUAL OF OPERATIVE SURGERY. By JOHN FAIRBAIRN BINNIE, A.M., C.M. (Aberdeen); Professor of Surgery, Kansas State University, Vol. I, Operations on the Head, Neck, Nerves, Trunk, Genito-Urinary System. Fourth edition with 713 illustrations. Philadelphia: P. Blackiston's Son & Co. Price, \$3.50 net.

In this, the fourth edition of this valuable manual there are marks of thorough revision. The chapter dealing with the nervous system has been re-written; the chapter on renal operations has been enlarged and more freely illustrated; the chapter on orthopedic surgery and the veins will be included in a companion volume which is now in course of preparation. Notwithstanding these enlargements the author believes "that the volume can be slipped into a coat pocket without too much injury to the clothing." It is not clear, however, why one should want to put a manual of surgery in his pocket. In these 800 pages the methods to be employed in the various surgical operations are presented with such singular clearness as convinces one that the author is a surgeon as well as a writer. The letterpress is admirable and the illustrations are fresh, clear, and pleasing.

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## Medical News.

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### JOURNAL CLUB.

The annual general meeting of the McGill University Medical Journal Club was held on January 27th, 1909, in the Peter Redpath Library. There were present: Drs. Archibald, president; Nicholls, secretary; Howard, treasurer; Gardner, Birkett, Fry, Morrow, Klotz, Orr, Turner, Byers, and Russel.

The reports of the secretary and treasurer were received and adopted.

The secretary reported a prosperous year. The Club was going cautiously and its expenditure was well within its income. The usefulness of the Club had, nevertheless, been considerably extended. The following journals were added to the files during 1908: *Folia Hæmatologica*, *Folia Scrologica*, *Folia Urologica*, *Neurological Journal*, and *Archives d'ophthalmologie* (continued).

In view of the fact that the secretary's duties were largely nominal, Dr. Klotz moved, and Dr. Nicholls seconded, that the positions of the secretary and treasurer be united in one person. Carried.

It was decided to subscribe for the *Archiv für Schiffs-und Tropen Hygiene*, and the *Mittheilungen aus der Grenzgebiete für Medizin und Chirurgie*, and the committee were empowered to consider the advisability of adding several other journals to the list.

The election of officers resulted as follows:—

President—Dr. E. W. Archibald.

Sec.-Treas.—Dr. C. Howard.

Executive Committee — Drs. Nichols, Klotz, and Russel, with the president and secretary-treasurer.

The report of the treasurer is appended.

ASSETS.		LIABILITIES.	
Carried forward from 1907....	\$ 72.60	Bal. due for journals 1905.....	\$ 2.92
Subscriptions due from 1906..	14.00	Bal. due for journals, 1906.....	9.27
Subscriptions due from 1907..	24.00	Bal. due for journals, 1907.....	54.32
Subscriptions due from 1908..	96.00	Stamps .....	5.00
New subscriptions for 1908...	28.00	Journals for 1908.....	122.13
Presented journals .....	14.13		
Bank interest.....	3.39	Total .....	\$194.64
	<hr/>	Bal. in bank Jan. 25th, 1909...	57.48
Total .....	\$252.12		<hr/>
			\$252.12

C. P. HOWARD,

*Treasurer.*

### CANADIAN MEDICAL ASSOCIATION.

For the forty-second annual meeting of the Canadian Medical Association in Winnipeg, on the 23rd, 24th, and 25th of August, 1909, transportation arrangements have been completed. For delegates, their wives and their daughters (no others), from points east of Port Arthur the rate will be single fare plus twenty-five cents for round trip tickets, provided fifty or more are present holding Standard Convention Certificates. These tickets will be on sale from August 14th to 21st, final return limit from Winnipeg Sept. 25th. If Ontario Lake route is used payment of the following arbitraries must be paid to the pursers of the Richelieu lines: During August, Toronto to Montreal \$8.00, from Kingston to Montreal \$4.50; during September, from Toronto to Montreal \$6.65, from Kingston to Montreal \$3.50. Upper Lakes: going \$3.50 additional, returning \$8.50 additional. Side trips from Winnipeg one fare for the round trip, August 25th to September 24th inclusive. Alaska-Yukon-Pacific rates will apply for side trips announced in the annual circular issued in June, or July 1st. Local Convention plan arrangements will prevail for the West as far west as Laggan and Coleman, Alberta. Lowest one way first-class fare from B.C., date of sale of tickets being August 16th to 19th inclusive, with final return limit September 25th.

Dr. Max Klotz, of Ottawa, has gone to England and the Continent, where he expects to spend eight or nine months in study.

A new home for tuberculous patients has been opened in the outskirts of Gravenhurst, on the shores of Lake Muskoka. The Minnewaska hotel has been purchased by Mrs. Fournier, a Canadian nurse who has achieved distinction in the United States, and the patients will be under the professional care of Dr. Charles D. Parfitt. They may be assured of intelligent nursing and proper medical supervision.

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Dr. Arthur C. L. Fox, of Montreal, died on March 31st, at the Royal Victoria Hospital, in his 45th year. He graduated from McGill University in 1898, since which time he practiced his profession in the city. During his earlier years he was an oarsman of repute in Western Canada.

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Dr. Dixon, of Frankville, Ont., died of appendicitis on March 18th. He had for some years carried on an extensive surgical practice in the country around, and was noted as one who kept thoroughly abreast of the advances made in his profession.

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J. K. Niven, a graduate of McGill, 1901, passed away at his home in London, Ontario, on the 30th of March.

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### Society Proceedings.

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#### THE MONTREAL MEDICO-CHIRURGICAL SOCIETY.

The eighth regular meeting of the Society was held Friday evening, January 22nd, 1909, Dr. J. Alex. Hutchison, President, in the chair.

D. A. SHIRRES showed a living case of hysteria simulating cerebral tumour.

C. K. RUSSEL, M.D. These extremely interesting cases that Dr. Shirres has just shown make one wonder what the real nature of hysteria is; how these so-called functional diseases can imitate so closely organic lesions of the brain and cord. One often wonders if there is not some underlying really organic condition, although we cannot see it under the microscope, and if it is not some vaso motor condition that from its nature cannot be demonstrated under the microscope. With these hysterical patients one notices that there is usually great vaso-motor instability, they blush readily, and we get dermatographia and other evidences of this and I often wonder if there is not some similar vaso motor disturbance occurring in the brain or cord causing these symptoms that

simulate a definite organic disease so closely. Hysteria in a man of this patient's age is certainly rare in this country.

#### FOREIGN BODY BETWEEN THE INNER AND OUTER TABLES OF THE SKULL TWENTY-FIVE YEARS.

A. R. PENNOYER, M.D. The patient before you to-night is a young man 28 years of age, who came to me last June with a little nodule over his right eye, which felt very much to me like a sebaceous cyst. The history was that for seven or eight years this had bothered him a good deal at different times. During the early part of this period it would disappear for some little time only to reappear again. Since last winter, however, the lump has enlarged in size and has not subsided. On excision of this little lump I found that it ran down apparently into the bone. I ruptured it and there was an escape of a thick, yellow purulent matter much like that from sebaceous cysts and I concluded that it must be of the nature of a dermoid. I cauterised this, but a sinus persisted resisting all methods of cure for about two and half months. Having failed to close the wound I gave him a general anæsthetic, exposed the bone and found in the frontal bone a punched out opening which would readily admit a large sized probe. On exploring this I found it led into a cavity between the outer and inner tables of the skull and contained dark gritty material which we thought to be sand or gravel, at least this was the conclusion which we came to after going further into the history. It seems that 25 years ago when a child three years old he was run over by a heavy loaded wagon and sustained an extensive scalp wound, the scar of which can still be seen, hence the conclusion that for 25 years this road gravel had lain here, causing this abscess which had communicated with the parts underlying the skin. In a week's time the wound was entirely healed. An interesting thing, however, is that there is a little nodule forming a short distance from the former one just as this one did seven or eight years ago and it will be interesting to find if this turns out to be a similar abscess to the one just cleared out. He has a slight anæsthesia in the area of the supra-orbital nerve as the result of the operation.

C. W. DUVAL, M. D. The sand-like material was sent to the pathological laboratory for diagnosis, and at the time I thought it might be a psammoma. However, on subsequent study, together with the fact that the granules were found between the outer and inner tables of the skull, I decided that it was not such a new growth. Still it must be borne in mind that another small swelling similar to the first one has recently appeared, which would argue in favour of its neoplastic nature, instead of sand-inclusions the result of accident received many years ago.

E. W. ARCHIBALD, M.D. I would like to ask one or two questions concerning points which are not clear to me. For instance, I do not quite gather from Dr. Pennoyer's description whether the cavity led only as far as the internal table or under it to the dura mater. Further, one would like to know whether this cavity contained granulation tissue or not, such as one would expect to find from an inclusion of gravel for so long a time in such a situation. With regard to tumours in this situation it is, of course, not common to find dermoids within the skull, but it is common to have dermoids in the outer layers, and in such dermoids it is not so very uncommon to have slight calcification. I would like to ask if there was any evidence of lime salts in the material examined.

A. R. PENNOYER, M.D. I might say that at the time of the operation I took particular pains to see if the cavity communicated in any way with the cranial cavity and we could definitely say that it did not, it lay inside of the outer table. Neither was there any evidence of granulation tissue; the lining of this cavity was almost ivory-like in appearance and a little irregular in outline, but the gravel, or this dark gritty material, came away and left it quite clean. The apparent discrepancy in position I would explain by the natural changes in relative position that would occur in growth, and also that at the time he had this injury there was a fracture of the outer table which was not noticed and some gravel was included. With regard to the lime salts I can only say from its gross appearance that it had not any of the characteristics of this at all.

#### PRIMARY TUBERCULOSIS OF THE BREAST.

E. M. VON EBERTS, M.D., read the paper of the evening.

E. W. ARCHIBALD, M.D. It is a little difficult to discuss such an exhaustive paper as that we have just heard, based as it is, too, upon a rather rare condition. Yet the hard work evidenced in its very thoroughness deserves at least an attempt at discussion. One or two points were to me of special interest. First, the classification. It seems to me that the term "primary tuberculosis of the breast" ought to be reserved for such cases as develop inside the gland itself. No case that involves the skin first and the breast secondarily can be counted in. I may be mistaken, but I thought that one or two of the cases quoted were of this nature. Another point concerns the question of tuberculin. I suppose that no one of us here present has such an extensive experience in the therapeutic use of tuberculin as has Dr. von Eberts; certainly I have not. With regard, however, to the question of tuberculin used for diagnosis, and particularly with regard to the point raised as to the local reaction at the

site of disease following subcutaneous administration, I would say that in my experience such a local reaction has been very rarely obtained. In the numerous cases in which I have used tuberculin as a diagnostic agent I have rarely found any inflammatory reaction at the site of the suspected lesion. A further point concerns the question of lactation. It is remarkable that, as Dr. von Eberts says, so many of the cases develop, or become acute, during lactation. Here we have a clear evidence of the ill effect of function upon the tuberculous process, just as clear, for instance, as when we see a latent infection of the knee become an acute one under exercise. There was sent to me some years ago by Dr. Kelly, of Almonte, Ontario, the specimen of a breast, the site of tubercular disease which was as he believed solitary, and primary in that gland. I was able to confirm his clinical diagnosis of primary mammary tuberculosis by microscopical examination. According to the history of the case, the condition became acute in the third week of lactation. It was found out later that she had had a nodule in the breast for some months previously. The removal of the breast cured the patient. There were no points in the history of this case to indicate a tuberculous lesion elsewhere. Tuberculosis of the breast is undoubtedly rare, yet, rare as it is, the necessity of accurate diagnosis is great, because, as Dr. von Eberts says, the treatment must be the same as for cancer in this region.

E. M. VON EBERTS, M.D. With regard to the classification; of course there are a very large number of cases of tuberculosis of the breast recorded, but in the majority there is a history of pre-existing tuberculous disease, and they can not, therefore, be considered primary. The only instance of direct infection which I have included in my series is the case of Pluyette where a wound of the nipple was followed shortly after by tuberculous disease. A similar case of direct infection was reported by Roswell Park.

With regard to the use of tuberculin for diagnostic purposes, I may say that my own experience has necessarily been very limited. What experience I have had has been chiefly in a negative way; that is to say I have administered tuberculin in one or two cases of tumors of the breast (fibromata) which were thought possibly to be of a tuberculous nature, without obtaining a reaction.

#### A CASE OF ANTHRAX.

G. T. STEPHENS, M.D., and O. S. HILLMAN, M.D.

G. GORDON CAMPBELL, M.D. This case came to the dermatological department and made no complaints whatever of being ill; but asked to



have his eruption treated. On looking him over we found that he had lesions very similar to those of erythema nodosum except for their situation. I looked on it as a rare form of erythema multiforme, and knowing it is frequently due to sepsis in some part of the body I took his temperature and discovered that he had fever and a good deal of systemic disturbance. I do not know of any case reported where anthrax has occurred with the large number of lesions which this case presented, such lesions are almost invariably single. The lesions here were much smaller than the ordinary one in anthrax which has a great deal more surrounding induration about it and resembles a carbuncle.

It is possible that in this case the septic condition may have given rise to an erythema multiforme some of the lesions of which had become infected with anthrax.

It seems hardly possible that a man could live with 20 or 30 of these malignant pustules on the body much less walk into hospital and not feel ill in the slightest degree.

F. G. FINLEY, M.D. What struck me about this case was its extremely benign character. The diagnosis depended exclusively upon the bacteriological examination, as none of the lesions corresponded with the clinical features of malignant pustule.

J. ALEX. HUTCHISON, M.D. I had the privilege of seeing this case and I feel very much like Dr. Finley felt in my surprise at the mildness of the type. The experience of many of us is probably limited to the one case of Dr. von Eberts a few years ago; it was of the single type and not such a diffuse nodular one as the present. Text-books refer to the two types and that the solitary pustule is much more serious and gives much more pronounced constitutional disturbances. This case certainly had some oedema when I saw it at a later stage of the disease, but only what one might expect if one were dealing with a local infection associated with some lymphangitis. I was also rather surprised to hear of the favourable termination. I rather expected that convalescence at least would be much more prolonged. I would like to ask if the sore on the animal's back was followed up and any effort made to take bacteriological cultures from it.

G. T. STEPHENS: The address given by the patient was visited but we were told that the horse was not there.

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The ninth regular meeting of the Society was held Friday evening, February 5th, 1909; Dr. J. Alex. Hutchison, President, in the chair.

## PATHOLOGICAL SPECIMENS.

A. LAPHORN SMITH, M.D., showed the following pathological specimens: 1. Fibroid tumour from the left broad ligament; 2. Multiple fibroid tumour; 3. Large fibroid tumour removed for hæmorrhage; 4. Multilocular ovarian cyst in a young girl.

## APPARATUS FOR THE CURE OF PAIN IN FIRST METATARSO-PHALANGEAL JOINT.

J. APPLETON NUTTER, M.D. A description of this apparatus will be found on page 162 of the JOURNAL.

## NEURITIS.

NORMAN VINER, M.D., read the paper of the evening.

A. T. MUSSEN, M.D. I would like to congratulate Dr. Viner on the able way in which he has dealt with this subject. Having treated many of these cases he was in a position to observe the course of the events which he has so clearly described.

In regard to neuritis there is still a great deal that is yet uncertain.

The work of Drs. Head, Rivers and Sherren on traumatic neuritis has explained much that was obscure, and I feel that if their suggestions are followed and a careful and systematic examination of other forms of neuritis carried out, we will be able to eventually account for some of the peculiar conditions found in this disease.

A. G. MORPHY, M.D. One point about the diagnosis is, I think, that it is possible sometimes to get confused between anterior poliomyelitis and multiple neuritis. I remember having the case of a child who suddenly fell ill with intense pain in the four extremities, in fact hypodermic injections of morphia had to be given to allay the pain. It was only a few days later when the trouble began to localize itself in certain groups of muscles in the lower extremities that the diagnosis of anterior poliomyelitis was possible.

N. VINER, M.D. The onset with fever and the suddenness are much more suggestive of acute anterior poliomyelitis than of multiple neuritis. However, cases like Dr. Morphy's do occasionally occur, but a few days of observation rarely fail to clear up the diagnosis, as the case seldom fails to settle down to its permanent characteristics in a very short time.

## PERFORATIVE APPENDICITIS, VENOUS THROMBOSIS, PARATYPHOID, INTESTINAL OBSTRUCTION.

OSKAR KLOTZ, M.D. There are several points in this case of Dr. Bell's which are interesting from a pathological point of view. You will firstly

note that the vena cava in the specimen presented, lies on the left side of the aorta, as far as the right renal vein. The vena cava along with its two main branches, the common iliac veins, are thrombosed throughout, and the thrombi pass into the external and internal iliac veins on both sides. From the appearance of the thrombus, particularly to be noted in the firm grey character, one is led to believe that the iliac veins on the left side were first involved. On the right side the iliac veins contained a more recent adherent clot, which, no doubt, extended downwards from the thrombus as it was advancing from the left iliac vein into the inferior vena cava. It is remarkable that, although both iliac veins were completely plugged there was no evidence of oedema of the lower extremities when the case was seen at autopsy.

As to the paratyphoid infection, the paratyphoid bacillus was isolated from blood cultures some days after the appendix operation. What association this infection had with the appendicitis is not easy to say, and the question arises whether the appendicitis was the original and only lesion at the time of operation, or whether the paratyphoid infection was then present and had its main and most severe site in the appendix. I am rather inclined to take the latter view, and think that the severe symptoms of the appendicitis over-shadowed the manifestations of a general paratyphoid infection. The apparent normal condition of the patient without the signs of paratyphoid fever after the operation is probably to be accounted for by the general effect of the operative procedure on the body during a mild grade of infection.

The paratyphoid organisms were again isolated from cultures of the blood and spleen at autopsy.

F. J. SHEPHERD, M.D. Dr. Klotz's remarks are very fitting. I have seen several cases operated on for appendicitis which contracted typhoid fever in the hospital, but never attributed it to the appendix condition. I was interested in the fact that there was no oedema. Another fact was that the thrombosis commenced in the left iliac vein, why this should be so I do not know. Nearly always when you get a thrombosis after operation you get it on the left side. I think it is probably due to the enemata given in these cases. I am interested in the fact that the patient had a double inferior cava, due, no doubt, to the persistence of the left cardinal vein.

#### DIPHThERITIC PARALYSIS.

A. H. GORDON, M.D. A report of this case will be found on page 237 of this number of the *JOURNAL*.

A. T. MUSSEN, M.D. In looking up an old number of *Brain* of some

20 years ago, I came across a review of 1071 cases of diphtheria, 175 of which developed paralysis. It would be interesting to compare the course and results of the paralysis following diphtheria previous to the use of antitoxines with that which results at the present time. Of the 1071 cases 362 died from the effect of diphtheria before any paralysis had time to develop. 175 cases of paralysis developed from the surviving 709, and of these 17 ended fatally. In over 50 per cent. the paralysis was of the limited form, *i.e.* palate, ciliary muscle and ocular muscle. The onset was most marked on the 3rd, 4th and 5th week. The duration varied from 1—15 weeks, but the majority of the cases lasted from 6 to 7 weeks. Regarding deaths from the paralysis the greatest danger was during the 1st week, then lessening to the 5th week; there were none after this. In no case was the paralysis permanent. This report was of further interest as it showed the connection between the amount of exudation on the fauces and the subsequent paralysis. Three cases of paralysis showed no exudate, in 24 it was slight, in 45 moderate, and in 53 there was much exudate. Gowers states that adults furnish the greater proportion of paralysis in diphtheria; cases are few in infancy, but increase with the age of the patient. Also that paralysis is equally likely to follow mild attacks as those which are severe.

Dr. Gordon's cases appear to show very similar results, only the paralysis was later in developing. I would like to ask, what is the percentage of cases of diphtheria that develop paralysis now that antitoxine treatment is employed?

F. J. SHEPHERD, M.D. I would like to know the percentage of the laryngeal form in this condition.

J. ALEX. HUTCHISON, M.D. Some years ago I was called to a house to see a child who had developed inability to walk and I discovered four cases of diphtheritic paralysis in the family. They had had a little sore throat some three or four weeks previously. These cases got well. Two were of the laryngeal type, one involving the muscles. Within the past two or three months an old patient of mine, living in the country came to my office with a child complaining that he had something the matter with his throat; I referred her to Dr. Hamilton White, who found a typical pharyngeal paralysis of the throat. The child complained of sore throat some two or three weeks previously.

A. H. GORDON, M.D. In reply to Dr. Mussen's question, I think that the percentage of deaths from diphtheritic paralysis as reported by Rolleston, would be from 15 to 17 per cent.; this includes cardiac paralysis. His statistics show a total of 1500 cases, 116 dying from all

causes and 58 from some form of paralysis. There were 335 cases of paralysis altogether. From all I can read, and from my own limited experience in the Alexandra Hospital, I think that paralysis very rarely follows purely laryngeal diphtheria; I have never yet seen a case. Dr. Hutchison's cases were interesting and would suggest a certain amount of susceptibility to the toxine in the family.

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The tenth regular meeting of the Society was held Friday evening, February 19th, 1909, Dr. J. Alex. Hutchison, President, in the chair.

The meeting was addressed by Dr. Henry Christian, Dean of the Faculty of Medicine of Harvard University, Boston. Dr. Christian took as his subject: "Modern methods of investigation in Medicine as illustrated by recent work on nephritis."

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The eleventh regular meeting of the Society was held Friday evening, March 5th, 1909, Dr. W. Grant Stewart, Vice-President, in the Chair.

**PATHOLOGICAL SPECIMENS:—1. INFECTIVE THROMBOSIS; 2. MUCOUS COLITIS.**

OSKAR KLOTZ, M.D. The first specimen I wish to show you is one obtained from a woman, thirty-two years of age, who had been confined at full term on the 30th of January, without medical attendance. The patient had some temperature several days after confinement, and on February 6th had a severe chill with a temperature of 106°. On February 7th she was brought to the Hospital in a very low state, and blood cultures which were obtained gave streptococci. She died on February 17th.

At autopsy the uterus was fairly well contracted, but the cavity was lined by a necrotic slough, although there was no free pus within it. The placental site was on the posterior wall to the right side. The veins passing from the placental site were thrombosed, and they could be traced into the broad ligaments. Here and there the thrombi in the vessels had broken down into a purulent fluid. On the right side the thromboses passed from the veins in the right broad ligament into the ovarian vein on this side. The right ovarian vein was occluded from the broad ligament to its termination in the inferior vena cava. Save for a small thrombus at the mouth of the ovarian vein and projecting into the inferior vena cava this latter vessel was free, while the ovarian vein on the left side was also free from thrombosis. Curiously enough, there was also a thrombus in the left renal vein close to its exit from the kidney. The appearance of this plug in the left renal vein, with its rounded edges,

is not unlike an embolus, and it is possible that this plug is of the nature of a retrograde embolus.

The specimen illustrates very well the process of infective thrombosis beginning in the uterine sinuses and extending into the emissary veins. From the specimen it is evident also how the general blood stream is constantly supplied with fresh infection.

The second specimens are of a bacteriological nature. The bacteria were isolated from a case which has given us much trouble. The patient was a girl of twenty-six, suffering from a mucous colitis. The colitis had existed five months previous to her admission to the Hospital, and blood and mucous were present in the stools. Shortly after admission, cultures were made from the mucous discharge from a colostomy. This culture I show you here in comparison to a culture of *B. typhi* and *B. dysentery*, (Flexner). Culturally the organism resembles the Flexner bacillus very closely, save that it is motile and produces a greater amount of alkali in the litmus milk.

We have now had this culture under observation for over a year, but its reactions on the media are the same as when it was isolated. Briefly, its cultural features are the following:—

The organism is a Gram negative bacillus, about the size of *B. coli*. It is motile; does not produce spores and grows readily on ordinary media. On broth it produces a diffuse cloudiness, without a pellicle. On agar the colony is greyish-white, slightly moist and has a luxuriant growth. On milk there is a transient acidity of very short duration, followed by alkali production. There is no coagulation or digestion of the milk. Dextrose is fermented to acid without gas, as is also mannite. Lactose, saccharose and maltose are not fermented. There is no indol production. The organism was not agglutinated by known Flexner and Shiga dysentery sera.

This organism was isolated several times from the mucous discharge, and once in almost pure culture.

Twenty-one months after the beginning of the illness the patient died. The lesions in the large bowel were those of a chronic dysentery, which, in places, were in the process of healing.

Microscopical sections of the colon showed a loss of the mucosa in many parts of the bowel, with a thick granulation tissue in the sub-mucosa. In other places there was some regeneration of the mucosal layer. We have, therefore, a typical case of dysentery, from which a dysentery-like organism was isolated. I believe that, although this organism is not a typical dysentery bacillus, it produced lesions which are identical with those of true dysentery.

## TUBERCULOSIS OF THE LIP.

GEO. E. ARMSTRONG, M.D.

## THE AFFERENT NERVOUS SYSTEM, PERIPHERY TO CORTEX.

A. T. MUSSEN, M.D., read the paper of the evening.

D. A. SHIRRES, M.D. I think we have to thank Dr. Mussen for bringing this valuable contribution to the meeting to-night. When Dr. Mussen was over in England he was very enthusiastic about this branch of neurology, and in continuing the study in the Montreal General Hospital has been able to bring out many factors which certainly I did not know, and we are hoping that as time goes on the interesting facts which Head has set forth will be confirmed. The paper is a long one and shows how hard Dr. Mussen has worked to get a line of thought from all these different facts.

## EIGHT CASES OF TIC DOULOUREUX: DEEP INJECTION OF ALCOHOL FOR TREATMENT OF.

D. A. SHIRRES, M.D. These cases were treated with a mixture of alcohol, cocaine and chloroform; three were operated on by Dr. Armstrong. The injections were first tried on the cadaver and it was found an easy matter to reach the foramen ovale or the foramen rotundum. There is a certain amount of danger in connection with this operation; two of Patrick's 16 cases had serious results, and in one of ours, a tabetic with the tic douloureux for five years, after three or four injections one optic nerve became destroyed and there was paralysis of the third, seventh and eighth nerve. This patient, however, has since recovered, barring the loss of vision. Loeffler's blue was used on the cadaver to discover how this accident occurred. By a considerable amount of pressure and injecting the fluid rather rapidly it was found that the fluid could be infused up the foramen ovale, round the pituitary body to the medulla and lateral sinus. In another experiment it was inserted slowly and with less force and did not become so widespread. It was thus concluded that more time must be taken in injecting the fluid.

In all the eight cases cures have resulted; and although it is stated that the tic douloureux returns in a year to a year and a half, another injection will cure them again. This treatment has only been in vogue for some two to three years and a half, but I think it is an operation which carefully carried out will bring results. Certainly it is to be preferred to the removal of the ganglion, which should be undertaken only as a last resource.

GEO. E. ARMSTRONG, M.D. I cannot explain the accident which occurred in the case cited by Dr. Shirres. The trouble seemed to follow

the third injection and I am quite at a loss to account for it. My rule in giving these injections is to take ten minutes to inject the solution after the needle is in position and the syringe attached to the needle. I take the clock and just gradually send the piston down, taking up the ten minutes to introduce two centigrammes. Two other cases have been moderately satisfactory and another very satisfactory. This last was a patient between 60 and 70 years of age who had this trouble in a very severe form for over nine years. She has been completely relieved and is in fact a new person. My experience has been limited to some five cases and I feel that it is a remedy that ought to be tried. The operation of removing the ganglion has a considerable mortality even under Sir Victor Horsley and Harvey Cushing, and even if the patients recover there is a very great disability. Some of them have told me that if they had their ganglion back they would cheerfully bear the pain. I think, however, they could not stand the test; at the same time these disabilities are material. If we can go on with these injections without such accidents occurring as has been narrated, it seems to me that the method should be tried even if the relief is only for a year or eighteen months. Of course some time must elapse before the operation must finally take its proper place, but from what I have seen of it up to the present I am very favourably impressed with it; it certainly gives great relief and gives it quickly. I would like to ask Dr. Shirres if he can enlighten us as to the mode of action of the alcohol in these particular cases.

G. D. ROBINS, M.D. Dr. Shirres and Dr. Armstrong are to be congratulated upon the results of their cases. These results appear to be decidedly better than those obtained by excision of the Gasserian ganglion, and the considerable disability that follows it, if one takes into account the high mortality that accompanies this more radical operation. If further results from the injection of alcohol are equally encouraging, this indeed will be a great boon. I was very much interested in hearing Dr. Shirres's account of the unfavourable case, and I would like to ask him whether there were any changes in the fundus of the eye in which vision was destroyed; also whether the loss of vision came on suddenly or if it was of gradual onset.

A. LAPHORN SMITH, M.D. I should like to ask Dr. Shirres if most of his patients were very anæmic and if the alcohol might not produce a temporary and then a permanent congestion of the nerve affected. As tic douloureux is a form of neuralgia and as neuralgia is the cry of the blood for better nourishment, it would seem that the most reasonable treatment should be the maximum of sunlight, fresh air, plain food and sleep, with all the iron that could be assimilated.



D. A. SHIRRES, M.D. We do not know the physiology of the action of the alcohol on the nerve, this has yet to be studied, but strange to say after the injection within an hour the pain is completely gone and yet there is no change in the sensation, they appreciate heat, cold and touch, just as well as on the other side and yet the pain is gone. Long before this, some ten or twelve years ago, different men used osmic acid and other things for destroying the nerve, but here we have simply the loss of the pain sense. As regards the tabetic case, whether the destruction of the optic nerve came on suddenly or not I could not say, as a very large ulcer formed on the eye within 24 hours, and therefore the ophthalmoscope could not be used. After my experiment on the cadaver I could easily see how an accident could occur by using rapidity and force in the injection. I cannot recall any of our patients being anæmic and I cannot see how that could be a cause. Why the right nerve should be so much more often affected in these cases than the left I cannot understand.

#### RUPTURE OF THE UTERUS.

H. M. LITTLE, M. D.

Dr. Little described a rupture of the uterus occurring during a prolonged labor. After admission to the hospital, the case was operated upon.