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MODERN PATHOLOGY.*

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Medicine at the present moment is passing through its great age of renascence. Never before in its whole history have such advances been made as are being recorded month after month in these last years of the nineteenth century, and with these great advances and their bearing it is that I wish to deal this afternoon. For they have been gained, not by chance speculation, but by purely scientific methods,—they are the results of experimental pathology—and, thanks to them, we are beginning to see our way out of that empiricism which has been the bane of our profession all these centuries, an empiricism which has made us the laughing stock of the wits and the butt of the cynics of every age.

Just in proportion as we gain an accurate series of observations so do we add to the science of medicine, so do we, in fact, establish a true Pathology. For pathology is the scientific study of disease. There still exist those who look upon the dead-house as the be-all and end-all of the pathologist, who regard the careful study of diseased tissues, of their gross and minute lesions, as forming his main function. Certainly this is a most important function, and one that, in English-speaking lands, needs yet further development. Here, in Montreal, much more advantage

* Inaugural Address delivered in the William Molson Hall, January 3rd, 1893.

might be reaped by the earnest student out of the post-mortem room. I fear that we should fall under the condemnation of that intelligent Yankee whose sweeping condemnation of the English medical schools was to the effect, "No, sir, you do not understand scientific medicine in England; you are nowhere near to it. Look at Berlin and compare: there every physician and surgeon considers that he has failed in his duty if he is not able to confirm his diagnosis by means of a post-mortem." We do not go so far as that here; we do not desire to attain to that Utopia; nevertheless we might accomplish more than is accomplished at present.

But the number of pure and simple morbid anatomists is rapidly fading into nothingness. Now at length it is generally acknowledged that inasmuch as pathology is, as its name implies, the study of disease, so is it to be entered into in the widest sense, and that the domain of the pathologist is the investigation not only of the *results* of disease, but also and equally of the *causes* and of the *course*. This is pathology at large; and now with recognition of this elementary truth that in order to advance surely a subject must be studed in all its aspects—origin, equally with progress, and equally with end—medicine has entered upon a new phase. Thus, at last, to combine one of the innumerable rich metaphors with which those of this side have enriched our common language with one of more assured antiquity, pathology is no longer a "one-horse shay," with that poor one horse behind.

If medicine is to be treated as a science it must, like all other natural sciences, be based upon experimental investigation. All the great advance of the present day is due to submitting the problems of our profession to the touchstone of experiment in place of the vague and abundant hypothesising of the past.

I know no better illustration of the truth of this contention than is afforded by the history of the events which led up to Pasteur's great discovery of the method of preventive inoculation against Anthrax, a disease the study of which has formed the starting point of all our later investigations into infectious disorders.

Anthrax is an epizoötic that has for long been recognized in

all quarters of the globe, a disease affecting the domestic animals and occasionally man himself, a disease causing a terrible mortality in the herds of sheep and cattle of districts where it has broken out. But until the sixties, like all other infectious diseases, absolutely nothing, or, not to be extreme, absolutely next to nothing, was known as to its nature, nothing could be done to eradicate it. Most held with Topsy that it "growed," the fatalists consoled themselves by declaring the disease a visitation of Providence and sat down, grimly patient, to await events.

It is true that some went as far as the theorising stage. Thus in France, in the district that may be termed the classic land of anthrax, the fertile district known as the Beauce, lying around Chartres, the farmers spoke of it as a disease of the blood. These Beauceron farmers are a curious observant race. It has been their custom to open and examine the carcases of the sheep that die. They held that the full-blooded, best nourished animals were those most easily attacked. Doubtless the natural habit of looking upon that which is lost as best had much to do with this notion of theirs. Still so it was, they held that anthrax was a "maladie du sang," and this view, translated into scientific language by Delafond, held sway for some years in France; that is to say, plethora was indicated as the main predisposing cause of the disease. The rich succulent pasturage of the Beauce region as compared with the neighbouring sterile but anthrax-free Sologne spoke in favour of this view. Indeed it became the custom when an anthrax outbreak occurred to transfer the sheep from the Beauce to the Sologne. A further argument in favour of the same was the fact that from time to time certain farms and in these certain meadow lands were specially attacked. The mortality was appalling: as many as fifty to one hundred sheep might be found dead in the morning. Now these affected meadows were apparently the most fertile, the grass was a lush dark green; but for years they were unsafe. The farmers called them sadly the "champs maudits"—the accursed fields.

The first step forward was made by a French Governmental

commission in 1852. This determined the infectious nature of the disease, showed that one and the same malady affected horse, sheep and cow, that inoculation with the blood of an animal dead of the disease induces the disease in another, and, what for our present purpose is of especial importance, showed that small animals, rabbits and guinea-pigs—animals which can be employed for purposes of research—can be inoculated and will manifest the symptoms associated with anthrax.

Next in order of time may be placed Davaine's advances upon his early observations, observations confirmed by Pollender and others, that the blood of animals suffering from anthrax contains innumerable minute and characteristic rodlets. Davaine and Rayer had first found these in 1850 and had described them very accurately. But not until Pasteur's papers in '60 and '62 upon the micro-organisms causing lactic and butyric acid fermentations could any explanation be given of their significance. If microbes be the cause of fermentation, it might be that they could also cause disease; there might be truth in the old vague fermentative theory which time after time had been brought forward to explain epidemic and other maladies, and thus Davaine was led to renew his studies upon anthrax. He found the rodlets constantly present in the blood of animals suffering from anthrax, that whenever they were present the blood was virulent, that fœtal blood from animals that had died of anthrax did not contain them and was not virulent. To Koch, however, we owe the complete proof that the bacilli are the active agents in the production of anthrax. He was the first to gain pure growths of them outside the body, and to show that the most minute quantities of such growths could cause the disease, and, again, by discovering that the bacilli formed spores, he cleared up a large array of difficulties in connection with the propagation of anthrax. These spores are little bodies—germs—formed within the bacilli; they can stand heat and cold and drying up, and can retain their vitality under conditions which are rapidly fatal to the bacilli containing them.

All this was a very great advance. We were taught that an infectious disease might be due to the entrance into the system

and propagation in the same of a minute vegetable organism. This was one great truth learned by experimental research. But still this was only a stage from which more might be learnt.

To some sufferers from toothache it brings a certain amount of consolation to be informed by the dentist that the pain is due to, say, an abscess at the root of the tooth; but even to these, as to all others, the important question is: How is that abscess formation to be brought to an end? What treatment can the dentist employ which will surely stop the pain? Now, this subject of research into the nature and prevention of disease is so young that there are still at this moment many infectious diseases and those the commonest—for instance scarlatina, measles, small-pox,—of which as yet we do not know the cause of the ache, if I may so express it, and very many others, notably tuberculosis, of which we know the cause, but have not yet arrived at the stage of operative prevention. With anthrax, however, thanks to Pasteur, we have arrived at the preventive stage.

It is in the highest degree instructive to follow the steps whereby Pasteur gradually arrived at his great discovery of preventive inoculation. Koch was unable to give anthrax to guinea-pigs and rabbits by feeding them with large quantities of spores. Pasteur, with the aid of the Beauceron farmers, solved the difficulty. He made huge growths of the anthrax bacilli, and, with these, watered certain fenced-off patches of meadow-land; a small flock of sheep was turned on to the watered patches, another small flock on to the adjacent unwatered meadow. Among the former there was a fair mortality; the latter were unaffected. It was clear, therefore, that at times the disease can be produced by the ingestion of the bacilli or their spores. When, now, Pasteur fed the animals on spore-containing bacilli, together with the thorns and substances liable to cause irritation and laceration of the mucous membrane of the intestine, the mortality became enormous. Intestinal lesions are, therefore, an important predisposing cause.

Next, Pasteur turned his attention to the earth of the

" champs maudits." He washed some of this in water, allowed the poured-off supernatant water to deposit the finer particles held in suspension, decanted off the water and heated the fine deposit in order to kill off all the ordinary microbes of the soil, leaving only the resistant spores alive ; inoculated a series of animals with the material so gained, and of these a certain number died of veritable anthrax. Thus he was enabled to show that the earth of the affected meadows contained anthrax spores. The proof was absolute. The theory of plethora fell to pieces before the results of experiment.

It is unnecessary here to describe in full how Pasteur demonstrated that the meadows had become " accursed" by the previous burial in them of animals that had died of the disease, or how years before Darwin brought out his admirable monograph upon the earthworm, Pasteur proved that it was this great fertilizer that, peculiarly frequent in the rich pastures, brings up to the surface the spores from round the carcases of the buried animals. Time forbids. I must pass on to the more direct researches of this keenest of observers into the production of immunity.

When Pasteur and Chamberland inoculated a number of cows with pure growths of anthrax they found that some died and some simply suffered from a transient malaise and survived, and, what is of special importance, that the survivors were immune to further attacks of the induced disease. And now the question arose : could the virus be so attenuated that all the cattle could be given a mild form of anthrax, and so be rendered refractory.

But here a great difficulty presented itself : the bacilli, as I have said, form resistant germs, or spores, and so long as such spores are developed, so long is there a succession of bacilli possessing a virulence equal to that of the spores from which they sprang. So long, therefore, as there are spores, so long is it impossible to gain attenuation. To gain attenuation the first step must be to prevent the development of these resistant germs. This was accomplished by keeping growths of the bacilli at a temperature so high—42-43°C.—that their development is hin-

dered and almost stopped. The longer the cultures are kept at this temperature the weaker do they become until in forty days or so they die off, and, no spores being formed, a small number of the bacilli taken any day after the eighth and grown in fresh culture fluids at blood heat, preserve for long the grade of attenuation implanted upon them. Thus a series of races of the bacilli can be gained of different degrees of virulence. In these Pasteur obtained what he wanted.

Upon May 5th, 1881, before the Agricultural Society of Melun, Pasteur inoculated 24 sheep with growths of weak, attenuated anthrax bacilli. Upon May 17th he inoculated the same 24 with his stronger second vaccine. On the 31st he inoculated all these and 24 unvaccinated sheep with the most virulent cultures of anthrax that he possessed. Within 72 hours all the unvaccinated sheep were dead from anthrax. Of those vaccinated but one died, and this one at the autopsy was found to have suffered from a serious ailment of another nature.

This was Pasteur's first attempt at preventive inoculation against anthrax upon any considerable scale, and, whereas, in the affected regions of France the mortality from anthrax had been, among sheep, nine per cent., and, among oxen, seven per cent., now it is below one per cent. In the first year alone of general employment of the method, among the eighty thousand or so sheep inoculated, the mortality would, under the old conditions, have been over seven thousand; it was reduced to a little over five huudred. Six thousand five hundred animals were thus saved to France in that year alone. As a consequence of the process the price of land in the affected districts has increased in value, for now the "champs maudits" can be freely used; the most important insurance companies decline to insure animals upon farms where the vaccination is not practised, and indeed themselves undertake the preventive inoculation of animals.

I have here given, at what may be held to be too great a length for this occasion, the account of the advances that have been made in our knowledge of one infective disease. I have done so purposely, for I wish you to realize how these advances have been accomplished not by mere superficial observation, or

by lucky haphazardings, but by a sequence of ably-conceived researches, the one leading onwards rationally to the other until, step by step, not only the primary cause, but also the means of prevention of a terrible disease has been satisfactorily attained.

This is but one of the triumphs of modern medicine, and now I doubt whether I can better employ the next few minutes than by bringing before you, in rapid review, the more prominent and telling achievements of recent pathology. I doubt whether I can better bring home to you the vital importance of my subject than by such a review, or can better "seize the day" than by so impressing you with the magnitude of the progress that has of late been made that I may stir up in you the earnest desire to see Montreal and Canada participating worthily in this great new birth of medicine.

Thanks to experiment, we have gained a knowledge during the last twenty years, and more especially during the last twelve years, of the bacteria associated with a host of the diseases affecting man and the lower animals; of the microbes causing suppuration and erysipelas, pneumonia, typhoid, diphtheria, influenza, tuberculosis, leprosy, glanders, relapsing fever, tetanus and malignant œdema, of microbes not bacteria associated with malaria, actino-mycosis, ringworm and favus. These and others are the infectious agents in man alone. If we take into account the lower animals also, I see that a recent able and laborious worker, Dr. Sternberg, gives a table of no less than one hundred and fifty-eight definitely recognized pathogenic micro-organisms, and the number, like that of the minor planets, is being increased month by month. It may alarm sundry here present to learn that there exist so many infectious diseases under the sun. I must ask you to take it for granted that there are so many, and, it may be, as many more, for I will not seek to entertain you by reading out Dr. Sternberg's list, together with the names of the investigators who have studied each of these 158 maladies.

With not a few diseases has the stage been reached of satisfactory preventive inoculation in the laboratory, the smaller mammals being employed. Pneumonia and typhoid may be cited as good examples. Tuberculosis and diphtheria are

doubtful examples. With these our success so far must, for practical purposes, be considered incomplete. Still, in these cases, the prospect fills us with hope.

With a few diseases already great success has been attained. First in place, as it was first in time, I may mention fowl cholera —a malady common in France, where at times it decimated the poultry yards. Pasteur was able to isolate the bacillus causing this, to grow it, to attenuate it, with the attenuated virus to give a mild form of the disease to the fowls, and thus to protect them from contracting the fatal form. Thereby the disease has been overcome, if not eradicated, in various parts of France.

Another malady, very fatal in certain districts, affecting especially cattle, and known as "black-leg," "quarter evil" or "symptomatic anthrax" has been most successfully combatted by preventive inoculation, the method suggested by Arloing, Cornevin and Thomas, and elaborated further by Kitt, being extensively employed in Switzerland and lower Austria. Here the experience gained from years of study of the disease has shown that a different method of attenuating the microbe is best employed. The flesh of an affected animal is taken, is dried and powdered and subjected to heat. By this means the spores of the contained microbes are attenuated, and a two-fold inoculation of the powdered muscle under the skin induces a mild disease and is protective. There is but one disadvantage in the method. Every endeavour has so far not succeeded in rendering it absolutely safe. After a series of, it may be, ten thousand successful vaccinations, suddenly, out of a batch of ten animals inoculated, eight may die as an immediate consequence of the treatment. The loss may thus fall terribly upon a single owner. This, however, has not prevented the Bernese farmers from employing the method, for they freely recognize its immense economic value to themselves. To obviate the difficulty and minimise the possible individual loss they have constituted themselves into a syndicate, and any loss is distributed all over the members of the same.

So, too, the disease known here as "swine erysipelas," in France as "rouget," in which the mortality is very high, often

twenty-five per cent., at times sixty per cent. and over, has been hindered in its spread by the researches of Pasteur, Roux, Chamberland and Thuillier. These observers found that if they injected a pure culture of the minute bacillus causing this disease into a pigeon, and passed the disease from pigeon to pigeon, the disease became more and more severe. The microbe gained in virulence. If, on the other hand, they made a similar series of passages through the rabbit, the microbe, while becoming more fatal to the rabbit, became less and less harmful to the pig. So that vaccines have been prepared with which vaccination against the malady is extensively practiced in France, Germany and Switzerland.

Here, then, we have a small series of diseases in which protective inoculation or vaccination has already been of enormous economic benefit. Thousands of sheep, of cattle, of swine, and of fowls are yearly being saved by these protective means. What, now, had been done for man himself? Well, man is an animal that it is difficult to treat satisfactorily. Having a will of his own and ideas of his own, the latter very often very wild, it is difficult to compel every individual in a district or country to comform to what the majority is assured for common good. We know this only too well in connection with the one form of vaccination that is already practised. That being so we can reduce the ravages of, but cannot hope to stamp out, a given disease. When further there is added the fact that a certain proportion of individuals are peculiarly susceptible to any given disease, and thus, that a certain number of individuals are liable to succumb in direct consequence of inoculation with an attenuated virus, a further difficulty is added. And, above all, there is the terrible responsibility attached to the initial investigation. We are playing with edged tools with a vengeance, and where the inoculation of a given virus will lead to one of two alternatives, and one of these two alternatives is the death of a fellow human being, then even the boldest may well shrink from initiating a procedure of this nature, however much he feels confident that the procedure is for the welfare of his race.

But, despite all this, the pathologist has succeeded in certain

cases. In one that can scarcely be considered as coming under the head of modern pathology (I refer to the true Jennerian vaccination), the inoculation, as you know, has already been carried on for nearly a century, though it is only now that the medical world is becoming convinced that what we produce in vaccination is a very mild attenuated small-pox, the virus being greatly modified by passage through the cow.

In the other cases we have had to depend for success either upon the fact that there is a very long period of incubation, during which preventive measures may be taken, or upon a modification of the procedure. A very long series of investigations by a great number of observers has shown us that the pathogenic bacteria in their growth produce certain toxic substances, and that to the poisons produced by them are due the symptoms of the disease. To give one example: By filtering the bacilli from the fluid in which they have been grown, Roux and Yersin have succeeded in isolating from cultures of the bacillus, associated with diphtheria, a substance which, inoculated into dogs, will in due time produce the same transient paralyses of the muscles of the eye, the soft palate, and the body generally, which in the human being so frequently manifest themselves in consequence of this disease. But, what is more, it has been found that in some cases by injecting the culture fluids, or substances gained from such fluids into animals, immunity can be produced, so that, if now large quantities of the microbes of the disease be injected the animals so treated remain refractory and show no symptom. It was two American observers (Salmon and Smith) who first in 1886 demonstrated this fact in connection with hog cholera. Since then the same has been proved for quite a host of diseases.

Again, a series of observations—started originally in connection with what might be thought to be the quite academic and unpractical subject of the exact nature of immunity—has led us to discover that certain tissues of the body, and certain cells of the blood itself, develop substances (defensive proteids or antitoxines) which antagonize the poisons produced by the pathogenic microbes, substances which can be shown to be

present in the serum, or fluid part of the blood. I should like to describe to you the latest demonstration of this fact given by my friends, Dr. Kanthack and Mr. Hardy—a demonstration as beautiful and ingenious as it is convincing, but to do so would lead me off my main path. So we arrive at this: if we take the blood serum of an animal that has been rendered immune against a given disease and inject it, or the antitoxine that it contains, into another but susceptible animal, that animal is rendered refractory to the disease. This principal has been employed by Tizzoni, of Bologna, to cure tetanus or lockjaw in man. That awful malady, as you may know, is not common, and opportunities do not often present themselves in which a supply of tetanus antitoxine is at hand to inject into the sufferer from lockjaw; nevertheless, during the last few months several cases have been recorded in which the disease has been cured and life saved by this means.

This brings me to the consideration of a yet more dread malady, one that it is difficult for me to classify, in that we have not as yet discovered the microbe which is its prime cause, and cannot absolutely declare that the preventive method devised by Pasteur and his associates acts in consequence of the attenuation of the living virus, or in consequence of the introduction into the system of the products of the growth of that virus, or, again, through the medium of antitoxines contained in the emulsified tissues that are introduced subcutaneously. Very possibly there is a conjunction of all these three. I refer to that most awful of all diseases—Rabies, or, as it is most commonly termed, Hydrophobia—most awful, not only in the bodily agony of the attack, but in the mental agony of the suspense attendant upon the long and very variable period that may elapse between the bite that induces the disease and the manifestation of the same.

The account of how Pasteur and his lieutenants advanced step by step towards the solution of this most difficult problem of the prevention of rabies of those already bitten, forms, perhaps, the most entrancing page in the whole history of modern medicine. But here I can only tell you that they discovered a

virus of constant strength in the spinal cord of the rabbit after the disease had been passed with due precaution through a long series of rabbits and the virus had been gradually intensified, that having gained this fixed virus they modify it by the action of moderate heat and of air, and that into the person that has been bitten is made a series of daily subcutaneous injections, beginning with an emulsion of rabbit's spinal cord that has been dried for fourteen days so as to be harmless, and ending with an emulsion of a portion of cord that has been dried three, or it may be only two, days, which is in consequence of extreme potency.

What have been the results of this treatment?

Whereas the most reliable statistics for the last twenty or thirty years give a mortality of more than 12 per cent. in those bitten by rabid animals, amongst the thousands that have undergone Pasteur's treatment the death rate is considerably under one per cent. At Paris alone over ten thousand patients have been inoculated since 1885, and over a score of "anti-rabic" institutes have been established in various parts of the world from Damascus to Rio.

It may be—and has been—objected that it is not just to argue from the total number of those treated, inasmuch as very frequently the evidence is of the slightest that the animal inflicting the bite suffered from rabies, and that too often the dog it is that dies—dies immediately without a care being taken to study his symptoms, while the man in his panic rushes off to Paris or elsewhere, and if there be any reasonable suspicion as to the dog's sanity he must perforce be allowed to undergo inoculation and swell the list of patients. In this connection, however, it is noteworthy that in the year 1887, for example, in Paris alone 350 persons were bitten by rabid animals; 306 of these were inoculated by M. Pasteur's method's, and 3 died: mortality =0.97 per cent. 44 neglected to present themselves at the Institute and trusted to luck; of these 7 died: mortality =15.9 per cent. These results, I may add, were gleaned, not for the benefit of the Institute Pasteur, but for the Parisian Prefect of Police. But I will give you a yet more convincing series of statistics.

From 1886 to 1890 inclusive 1757 patients presented themselves for treatment at the Institut Pasteur at Paris, in which cases it was absolutely certain that the animal that had caused the bite was rabid, inasmuch as either portions of the brain of such animal inoculated into another had produced rabies, or some person or animal bitten at the same time had succumbed to the disease. Of these 1757 patients but 18 have died, giving a mortality of only 1.02 per cent.

Of all bites, the most dangerous are those upon the face and head. In France, from 1862 to 1872 (that is, long before Pasteur began to study the subject), fifty cases of this nature were collected and recorded by the Comité d'Hygiene. Of these fifty, forty-four resulted in death. And in general it would seem that the mortality from such bites is more than 80 per cent. The records of the Institut Pasteur at Paris show that from 1886 to 1889 inclusive, among the cases in which the rabid condition of the attacking animal was satisfactorily determined, 593 patients presented themselves for treatment after bites upon the head. Of these, instead of more than 450 succumbing to the disease, only 14 died. Instead of a mortality of 80 per cent. there was one of only 2.36 per cent.

It is unnecessary, I think, to say more upon this subject (for those figures are themselves sufficiently eloquent) save to notice the latest development in this relationship. Only very lately Tizzoni and Schwartz, following successfully upon the studies of the former in connection with tetanus, have shown that rabies can be prevented, and indeed stopped in mid-career, by injections of the blood serum of an animal that has been rendered refractory to the disease. We are waiting to learn how far such treatment will be successful with human patients.

I have now given you the leading cases in which research has led to the discovery of methods directly preventing the onset of infectious or contagious diseases; and had experimental investigation but led to these results and to none other, I think that you would all agree that modern pathology had accomplished sufficient to earn the gratitude of mankind.

This, however, is but a small portion of the good work accom-

plished. A large portion is to be found in the enormous advances that have been made in the indirect prevention of infection. The knowledge we now possess that infection is due to the presence of microbes has revolutionized hygiene.

Let me take but one example—and that a disease which in temperate climes swells the death roll to a larger extent than does any other. I mean Tuberculosis in its various manifestations—as phthisis or consumption, tubercular peritonitis, tubercular bone disease, lupus, and so on. Until 1883 the medical world was still doubtful as to the infectious nature of the disease, and so long as it was doubtful, so long was it impossible to do anything to limit its spread; from the moment Koch's great discovery of the bacillus associated with the disease was confirmed and gained acceptance, from that moment the controversy was set at rest.

We know now that the phthisical patient is a possible centre for the infection of others, that the sputum contains the living germs, that these germs are capable of resisting exposure for long periods, and may, if inhaled, set up the phthisical process in the lungs of others.

We know, in fact, that it is criminal not to take strict hygienic precautions in connection with phthisical patients, that the discharge from the lungs must be vigorously destroyed by appropriate means, that handkerchiefs and linen must be thoroughly disinfected, and the patient's room be kept well-ventilated and sunny, for sun and air are the great germicides.

Again, bacteriological observation has amply demonstrated that cows not infrequently suffer from a tubercular disease of the udder, and has proved fully the relationship between such tuberculosis in the cow and the tubercular inflammation of the bowels which attacks young children brought up on cow's milk. The practical deductions to be drawn from this fact are that, where possible, it is the duty of parents to determine that the cows from which is drawn the milk supply of young children are in a sound state, and where this is not possible, it is imperative that the milk should be rendered sterile by cautious scalding.

What is yet of more urgent necessity is that the State should

stamp out tuberculosis in cattle—for now-a-days it is within the power of duly qualified inspectors to recognise tuberculosis in animals. When Koch elaborated tuberculin, and when, under political pressure, he made a mistake of such magnitude as it is only given to a great man to make—as old Chaucer says,

> "For whan a man hath ouergreet a wit
> Full oft him happeth to misusen it,"—

he was very far from working in vain. Leaving out of account the impetus that his researches have given to bacteriological science, his tuberculin has proved itself to be of diagnostic value, so that, injected into man or the lower animals, it indicates the presence or absence of tubercular lesions according as to whether it induces or does not induce high fever. Thus it is a comparatively simple matter to determine the presence of tuberculosis wherever there is doubt. It is for the welfare of the community that suspected animals be isolated from their fellows and prevented from being foci of contamination; and where reinoculation with tuberculin again leads to fever, there the cattle should be sacrificed.

It is, however, one thing to preach to those in authority upon this earth, another to get them to follow the advice of the preacher. In this matter the hygienist must, I fear, stand in the market place and pipe, without setting agog the feet of the rulers of the people. Tuberculosis is so widely diffused among cattle, and affects so particularly the most highly bred and delicate, that the commercial value of the cattle which ought to be destroyed is at present held to be a complete bar to any general action. I will say nothing as to the relative value of man and brute——and the farmer's vote. Perhaps some day this will be more evident than it is now. Nor would I thought to be taking to task the Government of this country, for Canada in this particular respect is neither before nor after other civilized communities. I only say this much in the hope that Canada, whose prosperity depends so greatly upon her four-footed possessions, and whose system of cattle inspection is so admirable, will in this matter of tuberculosis lead the van.

Only within the year that has just ended has a similar means

of diagnosis been determined in connection with another disease —a disease so fatal when it attacks man that it has already caused the death of more than half a dozen bacteriologists engaged in studying it—and the last death that I have seen recorded is that of the first to successfully work out the means of early diagnosis, a young Russian veterinary surgeon, Dr. Kalning. I refer to glanders, which at the present time is causing great ravages in various parts of Europe, but which, I believe, is not widely spread among the horses in this country, although it does exist here. Following upon Kalning, observers in Germany and France have confirmed the fact that the existence of this disease in the horse can be determined by the injection into suspected horses of sterilized glycerinated extracts obtained from pure cultures of the bacillus of glanders. Glanders in many respects resembles tuberculosis, and this fluid "mallein," like tuberculin, causes high fever in the affected animals whereby a diagnosis can be made.

Lastly, in this connection, it is impossible for me to pass over the most beneficial of all the advances that have been made through pathological research—an advance that will ever be associated with the name of our great countryman, Sir Joseph Lister. It was Lister who first proved that suppuration and foul conditions of wounds was due to the presence in these wounds of micro-organisms, and who showed that suppuration might be minimised and completely prevented by the employment of methods which prevent the access of germs to the wounded surface. This treatment has wrought a revolution in surgery. Operations are now performed securely and deliberately, which, years ago, would have been held to lead to almost certain death. And whereas erysipelas and hospital gangrene were the bane of the surgical wards, and puerperal fever ran like wildfire through the lying-in wards or attacked every woman whose accouchement was attended by some one unfortunate practitioner, and caused a terrible death-roll, now erysipelas never spreads in any well regulated hospital, hospital gangrene is unknown, and puerperal fever condemns utterly the midwife and doctor in whose practice it occurs, while it is practi-

cally banished from the hospital. To what perfection antiseptic treatment may be brought is shown by the fact that during one winter suppuration occurred but three times, as a complication of operations, at the Johns Hopkins Hospital in Baltimore. In olden times it would have been well had three operation wounds healed *without* suppuration in any hospital of the same size. Lister, by setting the example of antiseptic and aseptic treatment, has saved more lives than have been destroyed in all the wars of this century.

I have dwelt so long upon the good work done by experimental pathology in hindering the ravages of the infectious diseases, because it is in relation to these that pathology has of late gained its greatest successes, and because their incidence affects all, so that in discussing them I am the more likely to appeal to all here present. I need not say, however, that there are other forms of disease not directly due to bacterial invasion (indeed many are wholly unconnected with such), and here, also, modern pathology is throwing a flood of light upon their causation and their rational treatment. Let me instance the large class of diseases of the brain and spinal cord. In these, thanks to the researches of Hitzig, Fritsch, Ferrier, and numerous others upon the dog and monkey, we have learnt that there are areas of the surface or cortex of the brain which are intimately connected with the movements of different sets of muscles of the body and limbs, with speech, hearing, sight, and so on. We have learnt also that the nerves connected with these "centres" have definite paths through the brain and spinal cord, and thus, now-a-days, it is very often possible to determine from the symptoms displayed—the tremors, the paralyses, the disturbances of speech, or sight, or hearing—the portion of the brain or spinal cord which is diseased. It is, moreover, possible in many cases to cut down upon the region of disturbance, and, by removing the tumour or abscess pressing upon the brain substance, to save the patient from imminent death. It has been objected that this same knowledge could have been learnt by post-mortem observations without experiment upon animals, but granting, what I cannot honestly admit, that it could, the question is, when should

we have arrived at our present knowledge? Not for years and years. It was not until experimental research was made upon the functions of the brain and spinal cord that any sure knowledge was gained of those functions. And certain it is that our great English operators upon the brain and spinal cord, men like Macewen and Horsley, owe the knowledge they possess of the localisation of nerve centres to the researches of Hitzig and Ferrier.

But, ladies and gentlemen, I might continue far into the night recounting the triumphs and prospective triumphs of the modern pathology. I should like, for instance, to tell you how experiments of a purely academic nature into the functions of the sympathetic system have thrown a flood of light upon the symptomatology of that curious disturbance known as Graves' disease; I should like to tell you how, step by step, investigators like Horsley have worked out the relationship between disorders of the thyroid gland and that remarkable malady described first by Ord and known as myxœdema, until only recently Dr. Murray of Newcastle has discovered that cure may be brought about by injections of the juice of this gland taken from the sheep. But were I to continue, I should, I fear, thoroughly exhaust your patience. No matter how eloquent facts may be—and I have striven throughout to let the facts speak for themselves—there comes a period of mental repletion. I have already given you a sufficiency of facts in this brief review for you to judge of the greatness of my subject, of its high aims, of its noble achievements.

It may be that there are some here present who, through the silence of our profession when attacked by agitators, through ignorance of the facts, and through pleasant cultured habits of life and absence of contact with the huge mass of human suffering, have, until now, in the words of a friend of mine, permitted the philozoic sentiment within them to overpower the philanthropic, and who, thus far, have regarded vivisection and animal research as utterly degrading, if not, in the words of a gentle English divine, as "positively hellish." I ask such, if any there be, to seriously reconsider their position. All the progress that I have

described to you has been based upon animal research. Many of the advances made have been for the immediate benefit of the lower animals. Surely no one would venture to contend soberly that Pasteur was brutal and deserving of denunciation when, for the ultimate saving of innumerable thousands of sheep, he sacrificed a few score. I would say in all reverence, "How much, then, is a man of more value than a sheep!" Surely we act rightly if in the hope of saving men's lives, of adding to the health and well-being of our fellows, we, taking all care that they suffer a minimum of pain, employ the lower animals. Attack, I say, other painful practices in connection with the lower animals that are performed for our advantage and enjoyment. Wage a crusade against pigeon-shooting, against fishing and fox-hunting; cease maiming cattle, horses and fowls for pecuniary benefit, for comfort, or for purposes of food, and then, and only then, break out against those whose aims are higher and nobler. Thank God, our profession stands above every other in its reputation for humane feeling, and our profession unanimously supports those of its members who devote their lives to research. Perhaps it will be retorted that I am wrong in employing the term "unanimously," for that great physiologist and man of science of the antivivisectors, Mr. Lawson Tait, is dead against us. With all due acknowledgment of Lawson Tait's genius and marvellous power as an operative surgeon, I contend that he is no more a man of science, in the true acceptation of the term, than a skilled carpenter is, in ordinary parlance, a professor of applied mechanics. The very fact that the antivivisectionists fall back upon him as their medical mainstay proves my contention.

It may be also that among my colleagues there are those who would fain have me urge the claims of pure science apart from practical results. I own the rectitude of this plea. Science is not to be pursued for immediate results. The Professor of Chemistry—Pasteur—little thought that in beginning his studies upon the crystallisation of tartaric acid he was starting upon the path that would revolutionize medicine and make him the greatest benefactor that France and the world at large has ever possessed. The insignificant parish doctor in Posen—Koch—

had no idea that his attempts to gain cultures of anthrax bacilli from the cattle that died in his district would result in his becoming the great auxiliary in this revolution. And so it is always—science, to be successfully pursued, must be pursued for itself. To-day, however, I think I do rightly in showing the great results of such a pursuit, and in proving in a way appropriate to the end of the nineteenth century the truth of the old Arabian proverb—"The ink of science is more precious than the blood of the martyrs."

This, however, ladies and gentlemen, is not my ultimate moral. I have throughout this lecture endeavoured to keep that in the background, though to do so has been difficult,—and just as the Irishman in dock, when asked by the judge whether he had anything to say before sentence was pronounced upon him, replied, "Sure, my lord, and I leave it to your conscience," so I, too, would much prefer to leave this moral to the conscience of my audience. We who have the cure of bodies are infinitely more diffident than our brethren of the cure of souls; the miserable condition of the building in which our Faculty is housed—the Faculty that has made McGill what you now know it to be—is, I think, a strong argument in proof of our diffidence.

You will have learnt from the cases that I have put before you that human pathology and human medicine are based upon comparative pathology, that it has been through study of disease in the lower animals that man has thus greatly benefited. And this study of the disease of animals along with that of man ought especially to be pursued in a country like this, a country that depends so largely upon its cattle for its prosperity. Surely Canada ought not to be behind in the beneficent race that is now being run between the civilized nations to discover the prevention and cure of disease. Surely the time has come that due opportunity should be given to the student to study worthily a subject so rich in promise, and fraught with such actual and prospective benefit to our race. And here in this the leading medical school of the country, with its associated veterinary faculty, it is that the start should be made.

ADDRESS IN OBSTETRICS.*

By T. J. Harrison, M.D., Selkirk.

Mr. President and Gentlemen :—I feel rather out of place in undertaking to address you on this subject. Dr. Cameron was to have given the address on Obstetrics, and I was to follow him, leading the discussion. I had no idea of the line Dr. Cameron would take, and could prepare nothing, but was going to follow his lead. As it is, I will take for my theme Operative Midwifery as it was taught to me, and as I practise it.

It is now nearly forty years since I commenced the practice of midwifery, and more than forty since I commenced its study ; and I well remember how elaborately we were taught operative midwifery. The different varieties of forceps were shown, the advantages and disadvantages of each explained, the method of applying them in the different positions, and presentations demonstrated ; and the elaborate preparation of the patient. She was to be brought across the bed, her hips at its edge, her feet on two chairs, an assistant in charge of each limb, and a third to steady the body. Then, as to when we were to apply the forceps, the directions were nearly as short and terse as those of Mr. Punch to those about to commit matrimony—simply "Don't." We were told that it was a very dangerous operation. We were shown by statistics that it was a very fatal one ; and we were told not to apply the forceps until we were positively certain that nature could not complete the delivery. We were, moreover, told that we should never take our forceps with us—that there would be too much temptation to use them. The importunities of the patient, the anxieties of the friends, and the doctor's wish to show that he was really doing something, would induce him to use his forceps before he ought to.

When I went home to practise, the country was new, the roads were bad, and I had to travel on horseback ; and those of you that have ever tried the experiment—and those that have not may be familiar with Obadiah's experience in carrying Dr. Slop's

* Delivered at the meeting of the Canadian Medical Association, at Ottawa, September 21, 1892.

on his high-trotting horse—will know that carrying the long forceps on horseback is not conducive to the equestrian's comfort, and I was easily induced to leave mine at home.

I had not been long in practice until I had a case which I thought required the forceps, but I was seven or eight miles from home, the night was dark and stormy, the roads bad, and I put off sending until it was perfectly plain that something must be done. I got the forceps, applied them, and in a minute or two my patient was over it. I was imbued with the idea that I had performed a serious operation, and remember saying to my patient, "I need not tell you to keep still, you'll do it without telling. She was a strong, German woman, and when I visited her next morning she was sitting up combing her hair. I could not help thinking that if I had had my forceps with me I could have saved the woman and her friends a great deal of anxiety, and herself five or six hours of intense agony.

Since then I have taken my forceps with me, and have not waited until the woman was in articulo mortis before using them. I have, in my forty years' practice, used the forceps a great many times. I have in that practice, unfortunately, been at the death of more than one patient, but I do not remember to have lost one after a forceps delivery. I have had, I suppose like a good many of my friends, rupture of the perineum a few times, but have had as large a proportion of such cases where no forceps were used as where I used them. I remember two cases where I had rupture while using the forceps. One of these was a case of eclampsia. I think my father was operating. The patient went into a convulsion and suddenly threw herself just when traction was being made, and the result was a rupture. Early this spring I had another case; it was not my own—it was a consultation case. The woman—a very small woman—had been in labour for a great many hours before I was called. I applied the forceps, the head came suddenly and unexpectedly on the perineum, and I had to put a stitch in it. I think these are the only cases I have had in connection with the use of the forceps, and I have come to the conclusion that the teaching of those days, and the aphorism "meddlesome midwifery is bad,"

are accountable for a great deal of suffering and a great many deaths. I do not mean to say the aphorism is untrue. Meddlesome midwifery is undoubtedly bad, but the indolent, irresolute, undecided man takes advantage of it and shields himself under it. He sends for you hours after the forceps should have been used, and he calmly tells you that he believes in leaving things to nature, and that "meddlesome midwifery is bad."

No sensible midwife would think of operating where nature, unaided, was equal to the task. But the question to be decided is, How are you to tell when it is certain that if you do not operate your patient will die undelivered? You can only tell by waiting until the last moment, and then it is often too late. There is no doubt but that the man who uses the forceps as I have done, has used them in some cases where the powers of nature, if left for some hours longer, would have been competent to deliver, but, on the other hand, by waiting I should have lost patients that I saved. As in most earthly things there is a doubt, but you will remember, Mr. President, that Hoyle, in his advice to whist players, says, "When in doubt, lead trumps." I do not think better advice could be given to the judicious, careful, skilful midwife. *When in doubt, operate.* With regard to the harum scarum, rattle-brained man, all I can say is that he has no business in the lying-in chamber, and that if he finds himself there, no advice that you or I, Mr. President, could give would help *him.*

Again, when I went to school we were told that when the disproportion between the fœtal head and the maternal parts was so great that there was no possibility of natural delivery, we were to use Smellie's scissors to reduce the size af the head, and to deliver in that manner. Now, since abdominal surgery is so much in fashion, and the abdominal section so common, that, if Darwin's theory is true, we may in the future expect to develop a race with the abdomen already opened, I see in the reports of the Obstetrical and Gynæcological Societies that in a number of them the question has been seriously discussed as to whether craniotomy should ever be allowed. I remember that at a meeting of one of these societies in the States the question was

brought up, and the proposition was advanced whether it should not be declared criminal, and that the man who reduced the fœtal head should be put in the same category as the criminal abortionist. It was stated that the fœtus had as great a claim to life as the mother, and when it was shown that under the best of circumstances, with the most skilful men, there was more danger to the mother delivered by the Cæsarian section than by craniotomy, it was held by some that the fœtus was worth as much to the world as the mother, and that it was her duty to submit to the operation, even with the greater chance of losing her life, in order to save that of her child. I remember reading with amusement the remarks of one of the medical men there, who stated that he considered the life of the fœtus of more value to the community than that of the mother; that the mother had reached the end of her tether—she had done her possible, and, I suppose, he concluded she was only an ordinary hum-drum wife and mother, whereas the possibilities of the fœtus were unlimited, it might amount to almost anything, and that when the midwife by delay allows the fœtus to perish, or plunges the scissors into the living brain, he may feel he has stilled forever

> "Hands that the rod of empire might have swayed,
> Or waked to ecstasy the living lyre."

We all know that too many children born in natural labour die before completing the first year of life, and a still greater proportion of them perish before they arrive at the age of usefulness, and then, I think you will bear me out in saying, that after safely passing through all this, the chances of the child being an idiot, insane, or a criminal, are greater than of its being anything extraordinary. You will remember that when that amusing crank, Mr. Walter Shandy, wished to prove the correctness of his hypothesis, that the pressure of the cerebellum on the cerebrum during natural labour was injurious to the intellect; to prove the superior intelligence of those not subjected to it, could only bring up the case of Julius Cæsar and the doubtful and equivocal one of Edward the Sixth, and I doubt whether, with all the births by the Cæsarian section which have taken place since, you can materially increase Mr. Shandy's catalogue.

Now, we should bear in mind that the mother has gone through all this, has passed the quicksands and shallows of childhood, youth and adolescence: she has arrived at the time when she is the corner-stone of a family, when her life is of the utmost importance to her friends and family, and when her death would perhaps be a loss irreparable, and I say that she has infinitely greater claims on life than her fœtus. I say that under the best circumstances. I am not speaking against the Cæsarian section. Her chances of recovery would be less after the Cæsarian section than if she had been delivered by craniotomy. Perhaps, if the operation were performed by the best men of the colleges, the mortality would be small; but imagine how such an operation would be performed by the ordinary country practitioner. I can sympathize with a country practitioner when he is brought face to face with questions of this kind—"Shall he open the abdomen or reduce the size of the fœtal head? Perhaps he has never seen the living abdomen opened, or ever seen the section of the cadaver since he left college, and it is by the country practitioner that most of this work would have to be done. In a case of this kind, say, for instance, there are men in the States, in Kingston, or even in Ottawa, here, who can operate successfully: they might as well be in the moon as far as we are concerned. The boy who started practice without going to college said he should probably kill his first half dozen patients, but he would gain experience by it. But a man practising amongst our country families would not see that many cases during a long practice. In all my practice the question has but twice been raised whether I should resort to the Cæsarian section or craniotomy on the viable fœtus. Soon after I commenced practising I had a case. The woman had been in labour for a long time, the forceps had been applied and it was not possible to deliver. The fœtus was evidently dead. I used the scissors and delivered per vias naturales. About four years ago I had a patient, a small woman, with a pelvis of the masculine type, and the child was enormously large. She had been in labour for several hours. The head would not engage. I tried to apply the forceps above the brim, and am not ashamed to confess I made a complete failure. I

turned and brought down the feet. I thought perhaps by pressure above, and the application of the forceps to the after-coming head, that I could deliver, but the disproportion was so great that I entirely failed to get the head engaged. The fœtus was dead, the cord pulseless, and I passed the scissors into the base of the brain,—and those of you who have tried that on the base of a strongly ossified skull will not envy me my labour. However, I delivered the woman. I had attended at the birth of both the father and the mother; their mothers were remarkable for having large children, and her chance of bearing a proportionate child was small. I said to the mother-in-law, "Now, if this woman gets in this way again, I would advise, if she is anything like as large as she was, not letting her go her full term. That was some four years ago. Some time in June last I happened to be attending another daughter-in-law of hers, and she told me this woman was over seven months gone, was very poorly, and very large. I said I would advise her to have premature labour brought on. They had moved at this time several miles away. There was a young doctor living near her. They told him my opinion, and he at once passed a catheter into the uterus. He told them labour would come on in twenty-four hours or so. They waited forty-eight hours and then thought something was wrong. I went and found the os pretty well dilated, and I could feel the feet. I told her labour would take place before long, and that I thought the young doctor would get along all right. I had not been home long until I was sent for again. I found the os fully dilated and the head presenting. I ruptured the membranes and down came the cord. My friend was recently from college, and he thought that by putting the patient in the knee-chest position he could reduce it. He kept her in that position as long as she would endure it, and thought he had succeeded, but when I came to examine her I found the cord there still; I brought down the feet and delivered. The child was living, and is still, and doing well.

I think that when a country practitioner, who has had no experience in opening the abdomen, is brought face to face with the question, "Which shall I save, the mother or the child?"

and saves the mother, he has nothing to reproach himself with, and that he should be sustained by the profession. It is not in the interest of our profession or our patients to encourage operations in which we are likely to make disastrous blunders.

You will remember that O'Meara tells us that during the accouchement of the Empress Maria Louisa the presentation was abnormal, and Dubois asked the Emperor if it should be necessary to sacrifice the life of one of them, which he should save, the mother or the child. Badly as he wanted an heir, he replied, and I think it was one of the most sensible things that ever the great Napoleon said—"The mother, it is her right."

Discussion.

Dr. Wright (Ottawa)—I listened with a great deal of pleasure and satisfaction to Dr. Harrison's very eloquent remarks on the progress made in obstetrics during the last forty years, and I am sure we all enjoyed his address and learned a great deal from it. There were one or two points that probably attracted the attention of all. The one was as to the use of the old aphorism about the meddlesome midwife, and I can recollect the impression made on my mind as a student. As he said very properly, everything depends on the definition of the word meddlesome. I must say I think, as a rule, instruments are used more frequently than they should be. I do not mean to say that they are so used by Dr. Harrison, but we are all more inclined to be too hasty in the use of the forceps than we are to be tardy. I was interested in his remarks on rupture of the perineum. That danger is present to everyone of us when we are about to use the forceps. I quite agree with him that as many ruptures of that kind occur without the use of the forceps as with them, provided the instrument is used properly. I should have been pleased if he had touched on a point in which we are all interested—that is, how far there is a necessity for using vaginal and uterine douches, and how far that necessity occurs in the general practice of medicine. The reports of cases by which we are more or less guided usually come from hospital practitioners, and the statistics come nearly altogether from hospitals. I should

like to hear from general practitioners and specialists as to what their experience has been in private practice. For some time I was inclined to believe that it was a desirable thing and a wise precaution to wash out the vagina. I am now inclined to believe that more harm is done by indiscriminate use of the vaginal douche. I am inclined to believe that the natural process of labour is aseptic, and if the bed-clothing and the hands of attendants and everything else are kept aseptic, a woman is in very little danger of trouble. It is a matter in which I should like to hear the opinions of members present. My own practice is, when no complication occurs, to leave the woman alone and instruct the nurse to be careful about asepsis of all the surroundings. I should like also to hear the opinions of some medical men present about the after treatment of an ordinary midwifery case. I have quite myself, and I daresay most others have, given up the old method of dieting, which I think was altogether fallacious, and the old method of insisting upon absolute rest. One of the most practical points in dealing with lying-in women is insisting on change of position as soon as possible. I look upon bed-pans and all those things as being absolutely injurious and among the most frequent sources of trouble. I think getting a woman into a rectangular position for the ordinary physiological functions is of great use in getting rid of clots and other matter more or less deleterious.

Dr. Dickson—The remarks of Dr. Wright lead me to feel it my duty to express my opinion with reference to the use of the douche. I think that if the practitioner is careful in the manipulation of the case up to the time when most men are in the habit of employing the douche, the douche will not be necessary —that is, if he is careful to keep his hands perfectly free of all septic matter, and careful to see that the vagina is relieved from all clot and other matter, there is no necessity to use the douche. With reference to dieting and the constrained position that it was formerly thought necessary to require the woman to maintain, I think the ideas he has expressed are entirely in harmony with advanced management of obstetrical cases. In my opinion, in most cases it is wholly unnecessary to compel a patient to

continue the position on her back for any time whatever. I think the patient is much better fitted for a satisfactory recovery if she is allowed to take any position that she finds most comfortable. The use of the bed-pan I find also is a great mistake. It is awkward to evacuate the bowels or bladder in that position, and if the patient is prevented from assuming an upright position she is also prevented from getting rid of the accumulation of blood in the form of clot, and which often, by its presence, leads to great trouble. If she is allowed to assume an upright position she is able to get rid of those foreign elements and the prospects of her recovery are very much greater.

The CHAIRMAN—I have been very much pleased indeed with the address of Dr. Harrison, and more especially as it was unexpected and impromptu. I agree with a very great deal that he has said, and I particularly agree with the remarks that fell from Dr. H. P. Wright as to the use of the uterine injections and also change of position. I have been in the habit, for a great many years, of allowing a woman to change her position just as soon as she felt so inclined, and particularly when nature called on her to urinate I have had her raised to an upright position. But there is one precaution you have to take in this, it must be gently done. I think it is a very bad thing, indeed, for a woman to rise suddenly after she is delivered. Now as to the use of forceps, I think we have gone, perhaps, to the other extreme. Some years ago, as Dr. Harrison very properly pointed out, it was considered a very critical and dangerous operation. Now, particularly with the younger men, I think they are using the forceps a little too often. However, it is better to err on that side. With regard to rupture of the perineum, I believe, and have believed for a great many years, that you can do a great deal to prevent rupture by using the forceps. You have perfect control of the head. When the labour pains are severe you can hold the head back, and I think it is the greatest safeguard to the perineum to use forceps carefully. My late lamented friend Dr. Tye, whom you all know, and who was one of the best practitioners in the country, held with me that he had saved a great many perineums in that way.

I have done the same I think. If the forceps are used properly —if the perineum is allowed to dilate and warm applications are used, I think you prevent rupture in a great many cases.

ENTERECTOMY FOR THE CURE OF FÆCAL FISTULA.*

By H. H. Chown, B.A., M.D., Winnipeg.

The case which I bring before this Association is of interest both from its mode of origin and from the means used to cure it.

Madame V. entered the Winnipeg General Hospital on May 4th, 1892, with a large wound in the left groin, from which fæcal matter was escaping. The skin around the opening was intensely inflamed, hard, brawny and excoriated. The inflamed area included the upper third of the front of the thigh, the left vulva, and the lower third of the left side of the abdominal wall. This surface was very tender and painful.

The history of the case, as obtained from the patient, was that for many years a lump occasionally appeared and disappeared in the left groin. It was not painful and caused her no trouble. Although she never showed it to a physician, I have no doubt that it was a small inguinal hernia. During the spring of this year the patient fell in the yard and struck her groin against some hard substance. The blow was strong enough to cause discolouration of the parts, and the lump then became permanent and also painful. The injury converted the reducible hernia hitherto existing into an inflamed, irreducible hernia. No vomiting, constipation, or other symptoms of obstruction of the bowels supervened. Because of the presence of the tumour and of the pain which it produced, she decided to deal with it surgically on her own account. A poor widow, twelve miles from a doctor, she could not afford to secure professional assistance; so, with a razor, she made an incision over two inches long into the tumour, and succeeded admirably in performing a left inguinal enterotomy.

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* Read at the meeting of the Canadian Medical Association, at Ottawa, September 21st, 1892.

The patient was born in Belgium, came to Manitoba in 1889, and is 46 years old; she is the mother of four healthy children, no miscarriages. Before this trouble arose she had always been perfectly healthy, and never consulted a doctor for any form of sickness. Her husband died two months after she came to Canada, and since then she has had to work very hard to support her family. She is a strong, well-nourished woman. Her appetite and digestion are good. The bowels have always been regular before she made the artificial opening into them.

During the two months that she was in the hospital before the operation was performed, the fæces poured out of the opening in the groin. No form of dressing that we could devise would keep her even relatively clean. The inflammation of the skin was not only very painful, but also kept the temperature above the normal. The fact that she did not emaciate showed that the opening in the bowel was not high up, but must involve either the ileum or colon. A small amount of fæcal matter would pass into the bowel below the opening, so that every few days she had a small natural motion per anum. The opening was exactly in the fold of the groin, but no portion of the bowel could be seen externally, and no spur could be felt by the finger in the wound. Nothing appeared to check, in the least, the amount of discharge from the bowels, while the condition of the patient was pitiable indeed—a burden to herself and a source of disgust to those around her.

As the state of the patient was not improving in the slightest, as there was no tendency in the wound to grow smaller, and as the quantity of fæces passing through the lower bowel was constantly decreasing, I suggested the need of operative interference. After explaining to her the risks involved, she urged that one be undertaken, as she preferred death to her loathsome condition. Greig Smith, in his work on "Abdominal Surgery," gives three plans of dealing with these cases—

1. Closure by plastic operation.
2. Division or removal of the spur.
3. Resection of the bowel.

The first was not possible in my case because of the position and

size of the opening, and because the inflamed tissues would not yield good results in a plastic operation. The second was not possible, because there was no spur to be felt or seen. The third plan was therefore decided upon. Greig Smith says, "In cases of large loss of substance of one side of the bowel, without flexure and without the existence of a spur, resection may from the first afford the only prospect of cure." No words of mine could more accurately describe the condition of affairs in my patient, and the specimen shown will prove that nothing less than excision could have been successful.

Notwithstanding attention to feeding, to frequent washings of the bowel both above and below the opening, and to the free use of antiseptics on the surrounding cutaneous surface, it was impossible to get the skin into a satisfactory condition. It was made as clean as possible beforehand, but was a source of great anxiety throughout the after history of the case.

I operated on July 4th, 1892, before several members of the hospital staff, and was ably assisted by Drs. Porter, McInnis and Metcalf, members of the house-staff. My incision was made in the left linea semilunaris, and was about two inches long. After opening the peritoneum, I carefully separated the adherent bowel from its attachments, using the fingers only for this purpose. I then brought the involved intestine outside the abdominal wall, and the balance of the work was done extraperitoneally. On examining the bowel I found the opening was fully two inches long, and that just below the opening a ring of constriction had already formed to such an extent that I could not pass the tip of my little finger through the contracted portion. In the specimen shown, the glass tube would not pass through the lower portion until its end had been drawn out to a cone-shaped terminus. On account of the occurrence of this annular contraction, and the certainty that complete stricture would soon follow, excision of the intestine was the only course open to me.

As I had no proper forceps for occluding the bowel above and below the field of operation, I used tapes passed through the mesentery and tied firmly around the intestine. I then, with scissors, made two transverse incisions across the bowel, between

three and four inches apart, and removed the intervening portion with a triangle of the mesentery. For use in suturing, I had prepared fine cambric needles threaded with the finest Chinese twist silk. The silk should be cut in lengths of about two feet, and tied with a single knot at the eye of the needle, with one end cut to within two inches. Invert each cut end of the bowel and close it with a double row of continuous silk suture. Then draw the ends past one another to the extent of six inches, if possible, and apply two parallel rows of continuous suture to unite the bowels near the mesenteric line. Make the suture line one inch longer than the length of intended opening, and leave the needles threaded at the end of each row. Open the bowel on each side with scissors, placing the incision about one-quarter of an inch from the double row of sutures already inserted, and making it three or four inches long. To check hemorrhage from the incised wounds, a quick, continuous, over-hand suture is run along the cut edges, including all the coats on both sides, and continued around each free edge. The needles on the double line of suture previously placed along the mesenteric border are now picked up, and the sutures continued around the upper or free side of the incisions until they reach their point of origin. The lateral openings in the bowel, besides the sutures placed along the edges to check hemorrhage, are completely surrounded by a double row of continuous sutures, a pretty sure guarantee against fæcal extravasation. In order to prevent invagination of either blind, pouch-like end, it is wise to pass a stitch near each end to unite it with the adjacent bowel surface. For cleansing the parts nothing but pure boiled water is required. After thorough douching, the bowel is dropped back into the abdomen, the omentum drawn down over it, and the external incision closed by two lines of silk sutures. No drainage and no flushing of the peritoneal sac is necessary. The artificial opening through the abdominal wall was thoroughly curetted, swabbed out with chloride of zinc solution, packed with iodoform gauze, and then left to fill in with granulations. The operation performed and, indeed, almost the words used in describing it are copied from a pamphlet published by Dr. Robert Abbe, of New York, on "Intestinal Anastomosis and Suturing."

The operation, from the time of commencing chloroform to her return to bed, occupied nearly two hours. When the patient reached her ward about 4 p.m. she was in capital condition. The first twelve hours she had no food or drink, the next twelve hours she had cracked ice only, and then began to get half-ounce doses of milk or beef-tea every hour. There was slight vomiting and hiccoughing the first night, but after that the patient expressed herself as being free from pain and very comfortable. The bowels were kept locked up by opium for forty-eight hours, and then she received a seidlitz powder every three hours until the bowels moved at 2 a.m. on the third day, or about sixty hours after the operation. The first motion was soft and natural, with very little blood, mucus or pus. I continued the seidlitz powders, and the bowels moved again about 1 p.m., the same day. As long as she remained in the hospital she was given saline purgatives to keep the bowels open, and when she left she was furnished with a large bottle of black draught, with instructions to use it if required.

On account of the inflamed condition of the skin, and also because of the opening through the abdominal wall where the fistula had been, I was compelled to dress the wound every other day and use bismuth freely over the surface to keep it dry. I had two stitch abscesses, but fortunately they did not infect the deeper tissues. Indeed, in cases like this, where one has to operate through tissues which cannot be rendered aseptic, the condition of the wound must be a constant source of great anxiety. The temperature reached 101°F. the first, third and fifth days after the operation, then came down and remained practically normal during her stay in the hospital. She left on July 22nd, eighteen days after the enterectomy, and was able to walk over one and a half miles to reach the railway station. I received a letter from her early this month (September) stating that she was quite well, and had had no difficulty since her return home.

Two important questions are now *sub judice* in reference to operations similar to the one I have here described. The first moot point is in reference to the use of rings or plates. A number of American surgeons have suggested that the technique

could be improved by the use of decalcified bone plates, rubber plates, catgut rings, etc. I had the pleasure last year of seeing Dr. Dawbarn of New York demonstrate, on the subject, his plan of using raw potato for plates. The consensus of opinion, however, seems to be returning to the original plan of simple suture. In some cases the rings have twisted, and thus become the causes of obstruction after the operation. Senn draws attention to the possibility that plates may be tied so tightly as to cause gangrene. R. F. Weir, in a paper on the subject, draws attention to the tendency of the opposed intestinal incisions to slip out beyond the rings or plates so that they have to be tucked back, often more than once. Dr. Abbe says "that the attempt to simplify the technique of lateral anastomosis by bone plates or other devices has not improved it." Dr. Bell, in a graphic description of gastro-enterostomy, published in the MONTREAL MEDICAL JOURNAL, May, 1892, reports a case where the remains of the plates were found at the site of operation twenty days after, "the plate in the stomach still firm and scarcely altered in three-fourths of its periphery," and therefore a source of danger. The ease and safety with which suture without plates can be accomplished lead me to favour that plan.

The second question not yet decided is as to the preference between lateral anastomosis and end to end suture. The danger in each is cicatricial contraction of the opening, but, as in lateral anastomosis, the size of the opening is unlimited and perfect apposition is more easily obtained, it is, in my opinion, to be preferred.

AN ANOMALOUS RASH IN SCARLATINA*

By R. E. McKechnie, M.D., Wellington, B.C.

Mr. Chairman and Gentlemen:—I am about to relate the facts connected with a small outbreak of scarlet fever, in which four cases in sequence presented an anomalous rash in addition to the typical one. I am the more encouraged to write on such a subject as scarlet fever on account of the avoidance in medical journals and by medical meetings at present of any reference to such a well known disease. In fact, scarlatina, measles, mumps, chicken pox, and such like sicknesses are being treated by the profession as diseases, about which everything is known and all their anomalies noted. Bacteriology, the recent advances in surgery, experiments with new drugs, and new theories of chloroform syncope, appear now to engross the whole mind of the profession. The consequence is that in our journals the matters discussed are generally on a level a little higher than that attained by the general practitioner, and articles which should owe their chief value to them, to their use in keeping one's mind fresh on ordinary points which he is perhaps forgetting, are generally not to be found.

What I am about to mention cannot be classed as important, but merely as interesting. That it must be uncommon, I gather from hunting through a dozen or more authorities, one only, Fagge, even mentioning a papular eruption. If but one case had occurred I would have thought nothing of it, imagining it from some accidental coincident cause. But four cases in sequence put the matter in a different light, and made me believe them to be abnormal cases of scarlet fever. I will now briefly mention them as I found them.

Bella R., aged six years, had been sick five days when I first saw her. Her mother, who gave me the history, stated that the illness began with vomiting, fever, and a sore throat; and that a scarlet rash appeared the next day. There were no catarrhal symptoms. On the legs and arms a "pimply" eruption had

* Read at the meeting of the Canadian Medical Association at Ottawa, September 21st, 1892.

appeared, but left the day before my first visit. During the second and third days the child had been delirious, at which time also the urine had been scanty and high-coloured. I found the child free from fever; the throat still reddened and sore; the tongue of the typical strawberry type; and a slight persistence of the scarlatinal rash on the trunk and extremities. About the wrists the skin was rough from commencing desquamation, and the sides of the trunk were in a like condition. On the forearms and legs were a number of spots, varying in size from a pin's head to that of a split pea. These were of a faint purplish colour, which disappeared temporarily on pressure, and the skin over which was desquamating slightly. These spots corresponded to the sites of the "pimples" described by the mother which had disappeared the previous day. Three days later there was free peeling of the cuticle on the forearms, and in a week more the skin of the soles of the feet and the palms of the hands began to come off freely. The case evidently had been one of scarlatina, and at the time I looked on the mother's account of the "pimples," as she called them, as of little moment.

The second case was in Annie R., aged four years, a sister of the preceding. While Bella was convalescing I was called in to see Annie. I found her slightly feverish, with no catarrhal or anginoid symptoms, and no rash. I gave her some gray powder, and she was all right next day. But I was again called in two days later. She had then been sick about twenty hours. Her sickness began with vomiting and fever. I found her temperature $103\frac{4}{5}°$; pulse 160; the cheeks highly flushed; a slight scarlet rash on the thighs and arms, but a well-marked rash on the neck, chest and upper half of the abdomen. Her tongue was coated with a thick white fur, through which enlarged red papillæ projected. The uvula, fauces and tonsils were brightly injected. There were no catarrhal symptoms. On the next day the scarlet rash was more marked on the trunk, and slightly marked on the forehead. The cheeks were still highly flushed. The tonsils were swollen, nearly touching the uvula, with slight enlargement and tenderness of the glands at the angles of the

jaw. The pulse was still high, 160; and the temperature 103°. A few miliary vesicles appeared on the arms. On the forearms and legs a few small dark-red papules had also appeared. On the fourth day, the only change seen in the case was in the augmentation of the numbers of these papules on the arms and legs. They had increased to about 150, and were of various sizes, from that of a rape-seed to that of a hemp-seed. They were irregularly scattered over the surfaces; were discrete and of a dull-red colour, resembling that of a measley eruption, and were found in greatest numbers on the legs and forearms, with a few on the thighs and arms, and *none anywhere else*. Her mother said that these were exactly similar to the "pimples" she told me about which were on Bella, but that on the latter they had been much more numerous. And in her case, too, they had only appeared on the extremities. The next day the tongue had cleaned and was of the typical strawberry kind; the soreness of the throat had lessened; and the scarlatinal rash was rapidly fading. But what surprised me was that the papular eruption had completely disappeared. I had made arrangements to have it photographed, and was considerably disappointed at its sudden departure. In its place were the same spots of hyperæmia marking the site of each papule, which I had seen on Bella. The case convalesced without any fresh point of interest, desquamation being normal.

The other two cases I did not see myself, but on making inquiries readily found them; and as the parties who had seen them were very intelligent, and the circumstances attending were such as were needed, I unhesitatingly accepted them.

One case was in the nurse girl in charge of these two children. She had had the disease a couple of weeks previously, and while it was as typically scarlatinal as those just narrated, she also had the papular eruption on her extremities. Her mother, when consulted, verified the girl's story, and as the girl herself nursed these two children through the attack and so saw the eruption when she declared that her's had been similar, I was justified in believing her.

Her sister, a nurse girl in a neighbouring family, made the

fourth case, my information being as positive and similar. She, in her turn, communicated scarlatina to the child she was caring for, but it proved to be an ordinary case, no papular eruption appearing.

Before these few cases occurred, there had been in the neighbourhood, for two months or more, an odd case of scarlet fever appearing, and after them a few more, all of a mild type, La Grippe also was raging at the time.

In looking up authorities, the thought struck me that Rotheln might account for these papules. But the more I read the more I found that authorities contradicted each other. All agreed that it was a mild disease, with but slight anginoid symptoms, whereas one of my cases was severe enough to be delirious, and both had higher temperatures than are looked for in German measles.

The character of the eruption was also plainly scarlatinal, a finely diffused redness. This was the fundamental eruption in these cases, the papules being merely superadded.

Squire, in Quain's Dictionary, gives this definition of Rotheln. "a specific, eruptive fever, the rash appearing during the first day of the illness, beginning on the face in rose-red spots, extending next day to the body and limbs, subsiding with the fever on the third day, not preceded by catarrhs or followed by desquamation." But my cases, or at least the one which I saw during the whole course of her illness, did not develop the papular eruption until the third day, and not then on the face, as other authorities state, but on the extremities; and the cases were followed by desquamation. All the authorities I consulted were positive about there being no desquamation, but one, Bristowe, who says it is often followed by branny desquamation. But in these cases the cuticle peeled freely, coming off the wrists in strips, and off the palms and soles in large pieces. Bristowe also described the tint of the eruption as resembling that of measles, the spots being of a dusky or purplish hue. And he mentions the rash as sometimes assuming a papular character in places. It was after reading his description of Rotheln that I felt a need to establish my diagnosis of scarlet fever. But,

even he, says the rash develops first on the face and rapidly diffuses itself over the whole body, attaining its height of development on the second day—that is, twenty-four hours before the papules began to appear on my cases. So that the anginoid symptoms, the high temperature, the early appearance of the fundamental scarlet rash and the late appearance of the papular one, and the free desquamation following, completely exclude Rotheln and establish scarlet fever as the correct diagnosis. Hence I am justified in bringing before your notice my notes of these unfrequent cases.

CASE OF TUBAL PREGNANCY.

Rupture Between Second and Third Month—Laparotomy —Death.

By F. H. Mewburn, M.D., Lethbridge, Alberta.

Mrs. R., aged 29 years, was suddenly taken sick Aug. 29th last. On my arrival I found her in a state of collapse, evidently the result of internal hemorrhage. Great pain was complained of in the region of the uterus, but more so on the left side. Restoratives were administered and the woman rallied. She gave the following history: The attack came on suddenly while she was performing her usual housework, and was characterized by intense abdominal pain and fainting; she had missed two periods; had always been regular before; had had only one child, and that nine years ago; no miscarriages; for the last two months she had considered herself pregnant.

Vaginal examination was as follows: There was a slight discharge of blood from the uterus; os uteri dilated, admitting the tip of examining finger; on both sides of uterus and at the back, in the cul-de-sac, there was great fullness; examination very painful. Nothing could be gained by bimanual examination, as the patient was so stout.

The diagnosis was ruptured tubal pregnancy and an operation urged, but patient refused to consider it. During the next four or five days there was a mild attack of peritonitis, characterized by slight elevation of temperature, quick pulse, and pain; these

symptoms gradually subsided, except the pain, which was always present, though not so intense as at first. On September 9th, eleven days after the first hemorrhage, a second one occurred, and the collapse was even more severe than that caused by the first attack. The woman rallied. Vaginal and rectal examination gave the same information as before, but the condition was much intensified,—the posterior wall of the vagina bulged out between the labia and the course of the urethra was distorted. The woman was now willing for an operation. As soon as possible she was removed to the Galt Hospital and laparotomy performed. On opening the abdominal cavity blood-clots presented everywhere—between the intestines, attached to the intestines and to the omentum, while in the pelvis, surrounding the uterus on three sides, was a mass of blood-clot that was with difficulty removed, and when removed, filled a quart glass; there was also about half this quantity of fluid blood. Amongst this mass was the fœtus and placenta. A rupture was found in the left tube, near its fimbriated extremity. The tube was clamped, ligated and removed. The toilet of the cavity was tedious Hemorrhage at the time of operation was trifling. Hot saline solutions were used for flushing. A glass drainage-tube was placed in position, and the wound closed and dressed. The patient rallied for a time, then gradually sank, and died twelve hours after operation.

Remarks.—It is a matter of deep regret that the patient refused operative treatment at first, although strongly urged to submit to it, as in all probability, had she done so, her life would have been saved. After the second attack she was very anxious for an operation, and I considered that a laparotomy gave her the best chance for her life. Her removal to hospital was a matter of necessity owing to her surroundings and circumstances

Reviews and Notices of Books.

Naphey's Modern Therapeutics, Medical and Surgical. Including the Diseases of Women and Children. A Compendium of Recent Formulæ and Therapeutic Directions from the Practice of Eminent Contemporary Physicians, American and Foreign. Ninth edition; revised and enlarged. Vol. I—General Medicine and Diseases of Children. By ALLEN J. SMITH, M.D., Assistant Demonstrator of Morbid Anatomy, University of Pennsylvania; and J. AUBREY SMITH, M.D., Assistant Demonstrator of Obstetrics, University of Pennsylvania. Philadelphia: P. Blakiston, Son & Co., 1012 Walnut street. 1892.

The ninth edition of this well-known and popular work includes all the recent important additions to general and special therapeutics in internal medicine and diseases of children. The work, on the whole, is certainly one of undoubted value. It must be of great assistance to the busy practitioner to be able in a few minutes to refresh his memory with the leading indications in the treatment of diseases and how they are to be met.

A Text-Book of the Principles and Practice of Medicine. For the Use of Medical Students and Practitioners. By HENRY M. LYMAN, A.M., M.D., Professor of the Principles and Practice of Medicine in Rush Medical College, Chicago. With one hundred and seventy illustrations. Philadelphia: Lea Brother & Co. 1892.

Between entirely new works and new editions of older works on medicine, there is a great choice for the practitioner and student of the present day. The most recent of the entirely new works is the one under consideration. Dr. Lyman is well qualified from experience to undertake the herculean task of writing a work to include a description of the entire range of internal diseases. The task is very well performed in so far as it is possible for any one man to perform it. The day is about past when even ordinary text-books of medicine will be the work of only one man. The sooner it arrives the better. It is impossible for any one man to be thoroughly conversant with the entire range of internal diseases. To

make the nature and treatment of disease clear the writer should have exact and extensive knowledge.

Dr. Lyman's work will be popular with students and practitioners, as it is well arranged and represents fully the most recent knowledge. The sections devoted to treatment are clearly and well written, and are not padded with a mass of useless detail and accounts of methods better forgotten. The illustrations are numerous and are unusually well executed.

A Manual of Medical Jurisprudence and Toxicology. By HENRY C. CHAPMAN, M.D., Professor of Institutes of Medicine and Medical Jurisprudence in the Jefferson Medical College of Philadelphia; member of the College of Physicians of Philadelphia, of the Academy of Natural Sciences of Philadelphia, of the American Philosophical Society, and of the Zoological Society of Philadelphia. With thirty-six Illustrations, some of which are in colours. Philadelphia: W. B. Saunders, 913 Walnut street. 1892.

This is a treatise of Jurisprudence and Toxicology, being a course of lectures delivered to students. The author offers no excuse for the publication, save the request from the students and the hope that others may be benefited, which, to say the least, is hardly a good aim for a book. What this small work contains is fairly good. Its chief fault lies in what it does not contain. It is to be regretted that so many books are made in these days whose value exists solely in the author's mind.

Histology, Pathology, and Bacteriology. A manual for Students and Practioners. By BENNETT S. BEACH, M.D., Lecturer on Histology, Pathology, and Bacteriology, New York Polyclinic. Students' Quiz Series, Edited by BERN B. GALLAUDET, M.D., Demonstrator of Anatomy, College of Physicians and Surgeons, New York; Visiting Surgeon Bellevue Hospital, New York. Philadelphia: Lea Bros. & Co.

To the already too numerous list of Quiz compends is added one on Histology, Pathology and Bacteriology, all comprised

within the compass of 150 pages, including a most voluminous index. It is to be deplored that such injurious *tracts* as this, and the others of the same series, are allowed to flood the market. They take the same place in science that the dime novel does in literature, destroying a taste for what is true and useful. As such books are not scientific, so no true scientist would write one; in fact, it is only a mind cramped by mediocrity and disloyalty to science that could beget such lifeless images.

Hand-Book of Physiology. By W. MORRANT BAKER, F.R.C.S., late Surgeon to and Lecturer on Physiology at St. Bartholomew's Hospital, etc.; and VINCENT DORMER HARRIS, M.D., London, F.R.C.P., Examiner in Physiology at the Conjoint Board of the Royal Colleges of Physicians and Surgeons, and in the University of Durham, etc. Thirteenth edition. Philadelphia: P. Blakiston, Son & Co. 1892.

This standard publication is better known under the familiar name of Kirke's Hand-Book of Physiology, and that it has come to the thirteenth edition shows that it is a popular and useful work. The new edition is thoroughly revised and somewhat enlarged, and is fully illustrated, some of the plates being coloured. While not an exhaustive treatise on the subject, it is a most readable book and one on which a knowledge of physiology can be safely based.

The Essentials of Histology. Descriptive and Practical. For the use of Students. By E. A. SCHAEFER, F.R.S., Jodrell Professor of Physiology in University College, London. Third edition. Revised and enlarged. Illustrated by more than 300 figures, many of which are new. Philadelphia: Lea, Brothers & Co. 1892.

Schäfer's work has always been popular, and is considered by many competent histologists as the leading work on this subject. The third edition is in every respect, as far as we are able to judge, thoroughly up to the most advanced knowledge

of the finer anatomy of the tissues. We have much pleasure in recommending the work as one of great value.

A Manual of the Practice of Medicine. Prepared especially for Students. By A. A. STEVENS, A.M., M.D., Instructor of Physical Diagnosis in the University of Pennsylvania, and Demonstrator of Pathology in the Woman's Medical College, Philadelphia. Illustrated. Philadelphia: W. B. Saunders. 1893.

This work was written at the request of the students and with the hope that it may serve as an outline of practice of medicine, which shall be enlarged upon by diligent attention upon lectures and critical observation at the bedside. In form it resembles a note-book, all superfluous words being omitted, and its chief use will be to save students taking notes of the lectures. It is too short and sketchy to be a good text-book, but it will be useful as a review, and to look up points when the practitioner has not time to consult larger works.

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Night and Day, a Record of Christian Philanthropy. Edited by T. J. Bernardo, F.R.C.S.E. Containing a resumé of the Gossage case before the House of Lords.

An Inquiry into the relative merits of Vaginal Hysterectomy and High Amputation or Partial Extirpation by Galvano-Cautery in Cancer of the Cervix Uteri, by John Byrne, M.D., M.R.C.S.E., Surgeon-in-Chief St. Mary's Maternity, President of the American Gynæcological Society (1891-92), etc. President's Address, from Brooklyn Medical Journal, October, 1892.

Society Proceedings.

MEDICO-CHIRURGICAL SOCIETY OF MONTREAL.

Stated Meeting, November 11th, 1892.

JAMES STEWART, M.D., PRESIDENT, IN THE CHAIR.

New Members.—Dr. J. R. Spier and Dr. A. S. Wade were elected ordinary members.

Rhinolith.—DR. BIRKETT exhibited a rhinolith or nasal calculus removed from a woman aged 35, who for the past six or eight years had suffered from a chronic discharge from the left nostril. On examination, the entrance of the nostril was seen to be blocked by granulation tissue, and a probe, on being passed in one-quarter of an inch, impinged on a hard body. After reducing the temporary hyperæmia with cocaine, a body could be observed under the inferior turbinated bone, and was readily removed after being broken into two or three pieces, followed by complete relief to the catarrh.

DR. PROUDFOOT had removed a somewhat similar concretion from the nose of a child who suffered from a very fetid nasal discharge. The concretion was about the size of a marble, and he experienced some difficulty in fracturing it. The nucleus was found to be a small roll of paper. He thought that these concretions were generally phosphatic.

Carcinoma of the Ovary.—DR. LAPTHORN SMITH exhibited the specimens and reported the case, as follows:—The patient from whom I removed these two tumours was a married woman, 42 years of age, the mother of five children, the youngest of whom is 14 years of age. She has never suffered with her periods, and the menopause was passed three years ago. She had always enjoyed good health until June last, when she began to vomit constantly. In July her abdomen began to swell, and in September she first began to notice a swelling of the feet and legs. She then presented the following appearance. She was sitting bolstered up in a chair, her face dark-red in colour and almost cyanosed, her abdomen enormously distended, and her

feet and legs swollen and pitting deeply on pressure. Her heart was extremely weak, rapid and intermittent, and at first I thought her a case of heart disease, gradually filling up with water, but I failed to detect any organic murmur or evidence of dilatation. She had been vomiting incessantly for several weeks, her bowels were very constipated, and she was passing only two or three ounces of dark-red urine daily. The abdomen was so much distended that a careful examination only revealed the fact that it was full of liquid and under great tension. Neither did an examination per vaginam show any indications of the presence of a solid tumour, the vaginal vault being merely bulged downwards by the weight of the superincumbent fluid. The urine contained one-fourth albumen. Under treatment with small doses of calomel and bicarbonate of soda the vomiting was stopped, and after a few weeks further treatment with digitalis the quantity of urine secreted in the twenty-four hours rose to sixteen ounces and the albumen disappeared. The legs became less swollen, but there was no improvement in the distension of the abdomen. As her condition was serious I determined to tap her, and, if necessary, perform abdominal section. I therefore took her into my private hospital on the 1st of October, and on the following day I introduced a very fine trocar into the abdomen, half-way between the umbilicus and pubis, and thus removed two gallons and a half of straw-coloured fluid in half an hour, without the patient experiencing any faintness or other unpleasant symptoms. As soon as a gallon of water had escaped, the abdominal walls became sufficiently relaxed to permit me to feel two large solid tumours floating freely in the abdomen; when all the water had escaped, the liver could be found very much enlarged. Although I feared that these tumours might be malignant I urged their immediate removal. It is interesting to note that during the days following the tapping the quantity of urine passed in twenty-four hours rose to thirty-five ounces, and became quite clear in colour. I presume that this may be explained by the removal of the pressure of the ascitic fluid which pressed heavily enough upon the kidneys to flatten them out or collapse them and so diminish the circulation through them. On

the 5th of October, two days after tapping, I removed these tumours—one weighing, after the operation, seven and the other five pounds. I was assisted by Drs. Springle and Ritchie. The peritoneum was very much thickened, and the omentum was shrivelled up like a piece of scorched leather close up to the liver, which latter organ was very much enlarged and covered with metastatic deposits, but there were no adhesions. The abdomen was flushed with plain hot water, and the wound closed with silkworm gut sutures and buried in dry boracic acid powder. A drainage tube was left in for three days, and the silkworm gut sutures were left in twenty-four days, causing no discomfort whatever. She walked out of my hospital on the 30th October, looking and feeling very much better than when she entered.

The tumours present a fairly regular and very smooth appearance there being only here and there smooth raised patches on their surface about an inch in length. On section, the cut surface appears like pure fibrous tissue, but on microscopic examination by Dr. Bruère, the characteristic cells of carcinoma could be detected. As there were no papillary or other cysts anywhere in the structure, this is without doubt primary cancer of the ovaries, although the disease is somewhat rare. The most interesting point in the case was the entire absence of symptoms pointing to disease of the ovaries. The patient maintains that she never had the slightest pain in the region of the ovaries. Unless I had removed the fluid it would have been impossible to have recognized the presence of the tumours, and the patient would have been dead ere now.

With regard to the advisability of operating, Winkel claims that isolated primary cancer of the ovary may be completely cured by early extirpation, although it fails, of course, to produce a radical cure when adjacent organs, especially the peritoneum, have already become affected. When the carcinomatous tumour can be readily extirpated the operation will remove the source of the ascites and tension, and at least temporarily contribute to the patient's comfort. He mentions three cases which not only bore the operation well, but were improved for months afterwards. I was somewhat surprised to find the patient make such

an easy recovery from the operation, although I have noticed in other cases that a diseased peritoneum tolerates interference much better than a healthy one. She had none of the usual discomforts which generally follow an abdominal section, and would have been able, and was willing, to get up, if I had allowed her to, two or three days afterwards.

DR. GARDNER considered the case interesting and instructive as illustrating abdominal dropsy. He thought it was the rule, when the dropsy does not yield to constitutional treatment, that tapping should be performed so that a diagnosis can be made. He had not met with cases in which he could not detect the growths by vaginal examination. Extirpation of these masses often lead to latency of symptoms or symptomatic cure even though their structure may be declared to be malignant.

DR. BELL asked what were the evidences of cancer of the liver, and if there were any lymphatic infiltrations. The tumours were encapsulated, and he would not expect them, from their gross appearance, to be carcinomatous, nor would he expect a patient who was suffering from such extensive cancer to make so good a recovery.

DR. SMITH, in reply, said that no one could see the peritoneum roughened and thickened, or see the condition of the omentum, without deciding that the condition was cancer. He thought that the enlargement of the liver and the nodular character of the organ pointed to its involvement.

Uterine Myoma.—DR. WM. GARDNER exhibited an enormous tumour removed from a woman aged 47. The symptoms had been rapid enlargement of the abdomen since last July, with some hemorrhage. The diagnosis had been difficult, but he had inclined to myoma; the rapid growth was remarkable. The tumour was removed by total abdominal extirpation.

A Ready Method of Cultivating the Bacilli of Diphtheria.—DR. WYATT JOHNSTON said that about a year ago he had given some results of the cultivation of the bacillus of diphtheria in blood serum, as (if the case is seen early) it afforded a very valuable method of diagnosis. It is known that the bacilli are distinguished by their rapid growth on albuminous substances,

within eighteen to thirty-four hours after being sown they become quite distinct, while putrefactive bacteria do not attain any material dimensions in that time. The difficulty of obtaining blood serum is so great that this method of diagnosis has not come into general use. The egg has been known for a long time to be an excellent culture medium, and recently it has been recommended by Sakarhof to cultivate the diphtheria bacillus upon it; the method he used consists in maculating slices of hard-boiled eggs, sterilized and placed in sterilized tubes. This method being somewhat complicated for general use he (Dr. Johnston) had made a modification, which consisted in obtaining a hard-boiled egg and simply cracking it at one end and removing the shell membrane, which leaves a perfectly sterile surface, and which may be inoculated with a sterile wire. Place the egg upside down in an egg-cup and leave it in a warm place. He had not as yet worked with the method sufficiently to say whether it has precisely the same diagnostic value as the growth on blood serum.

DR. MILLS asked Dr. Johnston to state briefly the symptoms in animals following inoculation. Some conditions of the throat in animals look like diphtheria, but are really not so.

DR. JOHNSTON said that there were two conditions known as diphtheria in animals, one in pigeons and the other in calves; they resemble diphtheria anatomically, but the organisms causing them are quite distinct. If a guinea-pig is inoculated with 0.5 c.c. to 1 c.c. of the broth-culture of the diphtheria bacillus the animal dies in from 24 to 48 hours. At the site of the inoculation there is extensive œdema and hemorrhage with, sometimes, necrosis at the point of inoculation; there is a bloody serous effusion into the pleural and peritoneal cavities, and the organs show fatty degeneration. The diphtheritic affections commonly supposed to be communicated by the milk are usually anginas due to pathogenic streptococci.

Case of Pediculi Pubis on the Scalp.—DR. G. G. CAMPBELL exhibited a specimen of pediculus pubis which he had found on the scalp of a child who was brought to him for a peculiar appearance of the eyebrows, which proved to be due to the ova of the

pediculus; on examining the head the pediculi pubis were found. Dr. Campbell said that most authorities deny that such a condition is ever found, and he had only been able to find one recorded case.

Empyœma of the Antrum of Highmore.—Dr. Birkett read a paper on this subject, which will appear in our next issue.

Discussion.—Dr. Major said that the paper covered the ground so fully that there was but little to add. He had, several years ago, tabulated 189 cases of myxomatous polypi, and among them antrum disease occurred thirteen times; more recently he has had ten cases, seven having myxomatous polypi and the remaining three hppertrophy of the turbinated bone. The origin of the disease is no doubt from dental causes even though the teeth may appear good, and whether it may be secondary to nasal disease, or that nasal disease follows antrum disease, is not settled. As to symptoms, the old classical appearance of deformity of the face is not now looked upon as a necessary feature; the odour of the pus is perfectly characteristic, and is not at all like that due to syphilis; another symptom is the redness of the gums, and is important, at least as corroborative. He fully agreed with everything Dr. Birkett had said about surgical treatment. He uses a steel-worker's drill, which is reduced to fit a dental engine; the operation takes two or three seconds, and is entirely painless. He looks upon the drainage-tube more as a means for washing out the cavity than simply for drainage. He then proceeded to explain the kind of tube he is in the habit of using; after the opening has been made, a wooden plug is introduced into it and a plaster cast is taken of the mouth and teeth, and upon this model a tube, which exactly fits the opening, is made.

Dr. Shepherd had seen three cases lately, two having had sinuses above the pre-molar tooth; he removed the tooth and scraped through into the antrum with a Volkman's spoon. He did not see the use of so many instruments, and thought too much stress was laid upon the washing out of the antrum; in empyæma of the thoracic cavity a general washing is done once only.

Dr. Buller quoted a case of iritis following the operation by a general surgeon, and thought that the success of the special surgeon was entirely due to his attention to detail and so the instruments he uses.

Dr. Proudfoot fully agreed to the necessity of frequent washing of the cavity, for the cleaner the parts are kept the better for the patient.

The Late Dr. Geo. Ross.—The following resolutions of regret at the death of Dr. George Ross were proposed by Dr. F. J. Shepherd, seconded by Dr. A. Proudfoot, and carried by a silent standing vote :—

"*Resolved*,—That this Society has learned with the profoundest sorrow of the death of Dr. George Ross, a past-President and one of its foundation members. Dr. Ross's wide clinical experience and intimate knowledge of disease, combined with his remarkable powers of observation and judicial criticism, made him a most valuable member. The various papers and reports contributed by him from time to time to this Society were always received with the greatest interest and listened to with the closest attention.

"*Resolved*,—That Dr. George Ross' death, at the early age of 47, is a grievous loss to the medical profession of Canada, of which he was so great an ornament, and in which he exercised so great an influence, not only as a clinical teacher and writer, but as an active member of the various medical societies and corporations in whose work he took such a prominent part.

"*Resolved*,—That a copy of these resolutions be sent to the relatives of the late Dr. Ross and to the daily press."

Selections.

Thrombosis and Embolism in Fatty Heart; Use of a Valuable Stimulant.—(By W. Langford Symes, Kiltegan.)—Of equal rarity, interest and danger, the following notes may help to illustrate an untoward event which will sometimes be found to happen in the course of cases of heart disease, though happily more frequently heard of than seen. In vegetative valvulitis, especially of the aortic orifice, one is naturally mindful of the possible occurrence of embolism at any moment, but when no valvular disease exists, or is clinically inappreciable, such occurrences are very infrequent, their origin in fibrination depending upon vital conditions which are very rapidly fatal or only recovered from under the most prompt and heroic remedies. Aid is, however, so seldom at hand that such cases usually reach us an instances of "sudden death," or are exhibited as pathological specimens of "*ante-mortem* clot." Of such nature was the following case:—

A spare, feeble man, of about 45, consulted me for great "weakness," fainting on exertion, inability to walk up a hill, or even much on the level; coldness of the extremities, headaches, and difficulty in breathing. On examination there was evidence of fatty metamorphosis of the heart, with sounds very weak and "clicky," and the impulse very faint, but no *bruit* over any of the valvular areas. He suffered from advanced anæmia. For some time he was able to walk to see me, and his condition was much improved by a mixture of—Ferri et ammon. citrat.; ammonii iodidi; liquoris arsenicalis; liquoris strychninæ; with a liberal nutritious dietary and gentle exercise in the open air. His occupation was that of a night watchman, but this he was frequently obliged to relinquish owing to his health. He now unfortunately contracted an acute bronchitis, which was with great difficulty prescribed for, owing to the weak condition of his heart-muscle not admitting of antiphlogistic remedies. This much aggravated his condition and produced great dyspnœa. Eventually it was removed and he again recovered, having, however, some hypostatic congestion in the bases of his lungs.

Examining his chest one day as he lay in bed—having found for the first time a murmur over the pulmonary area—I gently sat him up to examine his back, propping him carefully with pillows, while his wife held him at the opposite side. I did not hear him breathe, and on again telling him to take a full breath, no air entered the base of his lungs. On looking at his face I was horrified to find that he was perfectly white, and to all appearances dead. We at once laid him back and held him up by the heels, applied mustard to his heart, epigastrium, arms and legs, and gave him several hypodermics of ether; but although his wife poured some brandy into his mouth, and his lips were freely sprinkled with liquor ammon. fortior (which he always had at hand), he did not breathe. There was no pulse, no sound over his heart, no respiration; his eyes were glassy and fixed; he could not swallow, and severe slapping of the epigastrium had no effect. Having exhausted all available stimulants, and recollecting that I had read somewhere of the value of a hot spoon applied to the epigastrium in states of asphyxia or syncope, I took a hot lump of coal from the fire with the tongs, and applying it over the xiphoid cartilage once or twice, was gratified to find it produce powerful contractions of the inspiratory muscles. This was kept up severely around the insertion of the diaphragm until, by degrees, respiration returned, his eyes opened, his heart beat, and the colour came back to his face. He then took some brandy, and finally rallied. Next day he complained of great pain in the left thigh, the inner side of which was acutely tender, most marked in the middle third. The limb was cold, and there was considerable loss of sensation, with inability to move the leg freely. A hard, knotty, cord like condition was felt over the vessels, in the neighbourhood of the profunda femoris artery, very closely resembling that found in phlebitis. There was no œdema, but, on the contrary, the leg was small and blanched. With poppy fomentations, flannel bandages, and hot jars the pain was relieved, and collateral circulation encouraged; and after a considerable lapse of time he regained its use completely, finally recovering sufficiently to resume occupation, but of a much milder nature.

Since healthy blood, freely propelled through healthy vessels by a healthy heart, will not coagulate, any disturbance of this equilibrium, whether *cardiac*, *vascular*, or *hæmic*,, may readily produce thrombosis, and this is even more serious when affecting the vessels of the venous and pulmonary than those of the systemic circulation.

Although the intimate relations between coagulation and its immediate cause is still an unsettled point, the most modern pathologists agree that some minute endothelial change of structure is a necessary antecedent in all cases, even those where a hæmic influence is most at work. Thus, where the blood is primarily affected (cf. pyæmia, the puerperal state, etc.); there must be some peculiarity of structure or function in that part of the vessel at which fibrination takes place—some physiological affinity between the endothelium and fibrin factors which induces this deposition. Feebleness of the circulation, from a weak propelling power, is, however, amongst its powerful predisposing causes, cardiac thrombosis commonly occurring shortly before death. Here some blood remains almost stagnant in the half-emptied auricular appendix or apex of the ventricle, where, should the structure of the intima present any abnormality, gradual deposition of fibrin takes place, a so-called "cardiac polypus" forms, and this, unless detached to plug some distant vessel, may suddenly occlude an orifice and produce instantaneous asystole. This was evidently the case in the instance cited, and the difficulty in dislodging the thrombus was only exceeded by the anxiety of the moment, and the remorse that I had caused it myself.

Of these globular *ante-mortem* clots, or cardiac polypi, an interesting specimen, hanging from the left auricle, is beautifully depicted by Dr. Norman Moore in his "Pathological Anatomy of Disease."

That fatty metamorphosis of the heart, without appreciable valvular disease, thus induces thrombus in its cavities by a *structural* instead of an *adynamic* process, is a matter easily accepted. The degeneration is essentially structural when once set a-going by the dire influence of malnutrition, and although

the only conclusive proof of its pathological existence is microscopic evidence that the muscular discs have been replaced by fat granules, with chemical change of the cell protoplasm, the heart may be so altered in advanced cases as to present to the naked eye the appearances of paleness, friability, or the peculiar transverse "zig-zag" markings with light bands, frequently found on the carneæ columnæ and musculi papillares of the mitral curtains—the so called "tabby-cat striation."

Thus, from want of evidence to the contrary, we believe that slowing of the blood-current from weakness of the heart is a subsidiary factor, and the constant antecedent of coagulation while the blood is flowing is some change in the intima of the vessel. On this point most modern pathologists are now agreed in the case of thrombus in a vessel, and similarly we may take cardiac thrombi in a fatty heart as the direct outcome of structural changes in its lining membrane, frequently apparent even to the naked eye.

We know that certain conditions of the blood favour the formation of thrombi—*e g*, anæmia, fevers and wasting diseases, septicæmia, cachectic or "marantic" (Virchow) states, etc., but here, again, we have no evidence that the *modus operandi* of these conditions is other than a capillary interference with the nutrition and vitality of the vessel at some particular point. Even extreme passive congestion, or stagnation, in veins, is now believed to favour coagulation by interfering in the first instance with the nutrition of the vessel wall.

Thus, as soon as perverted endothelial function is succeeded by alteration in its structure, as soon does the mysterious vital equilibrium between the blood and its containing "tunica intima" become disturbed at a given point, and thus arises the main factor in the production of all coagula. The initial steps in the next stage of the process are, according to one noted physiologist (Bizzozero), now taken by the "blood plaques," or elementary blood corpuscles, but thus far only can we safely speculate in the present state of our pathology.

The murmur heard immediately before the accident over the pulmonary area was, I believe, due to coagulation in the left auricular appendix, since it was never previously detected.

In a lecture delivered at St. Bartholomew's Hospital in June, 1890, Sir Dyce Duckworth cites an almost parallel case of embolism of the profunda femoris, followed by aneurysm and fatal rupture of the sac into the tissues of the thigh. This patient had, however, advanced aortic patency, the embolism occurring on the tenth day after admission, and the vessel bursting twenty one days later, proving fatal in eleven hours. Here also it closely simulated thrombosis of the saphena vein, but there was no œdema of the leg.

Should the wonderful value of such a simple, powerful, and ever-available stimulant be recognised from these notes, the chief object in their publication will have been achieved, and some future lives perchance saved.—*Dublin Journal of Medical Science*, September, 1892.

A Case of Plica.—(By Henry W. Stelwagon, M.D.)—This somewhat unique case, the notes of which I herewith briefly present, came under my notice some months ago. The patient, an extremely ignorant Irishwoman, aged 40, had been in this country but some weeks when she applied at the dispensary for treatment for an ordinary acne eruption At her second visit she was accompanied by her sister and brother-in-law, who desired to call my attention to a peculiar hair growth upon the patient's scalp. The hair, upon disarranging for examination, was found, with the exception of the part referred to, to be normal, and reached to the shoulders. From a dollar-sized area in the middle of the occipital region, however, well down toward the neck, the hair had grown into a felted lock, about the thickness of a thumb, and four feet one inch in length—extending, in fact, to the patient's ankles. The skin from which it sprang was normal, and the hair arising therefrom for about three-fourths of an inch from the scalp exhibited no tendency to felting or irregularity; beginning here it rapidly passed into a soft, closely-felted mass with an irregularly smooth surface, with here and there hair ends projecting. Towards the extremity the lock for several inches began to taper, and terminated in a brush-like end of about the thickness of a small finger. The mass was dry,

without stickiness or offensive character, and free from vermin. The growth had begun twelve years back, and it had been gradual. When the sister who had accompanied the patient to the dispensary left Ireland ten years ago, the lock, as she remembered it, was then six or seven inches longer than the other hair and presented the same felted condition. The growth in all its aspects was carefully examined, and gave no evidence of being in any way due to lack of care or want of cleanliness; moreover, there was no sign or indication that the patient herself was at all responsible for its existence. So far as could be determined by careful investigation, this curious formation had been of spontaneous origin, slowly becoming longer and longer, and that, too, without any corresponding growth or tendency to felting of the hair of the rest of the scalp. In this instance, as, indeed, in all cases of plica, the patient and her relatives and friends looked upon it with more or less awe and superstition. I regret to say that my failure to recognise the full depth and import of this reverential feeling lost me an opportunity for the further study, macroscopical and microscopical, of this unusual formation, inasmuch as an unfortunate suggestion made as to the advisability of cutting the lock off close to the scalp (with the idea in view of ascertaining whether the new-growing hair would exhibit the same tendency) was met on all sides with fear and doubt, and led, I believe, to the patient withdrawing herself from observation.—*Amer. Jour. of Med. Sciences*, December, 1892.

Barber's Itch.—Gottheil (*Medical News*, Sept 24th, 1892) writes on this subject. He says that the term barber's itch" is used by the laity to designate any disease affecting primarily the skin of the bearded portion of the face. In many cases the barber has nothing to do with them. Three separate and distinct diseases are common upon the bearded face. There are others, but they are of rare occurrence; and others that occur primarily on hairless portions, and may extend to the beard; and still others to which the integument covering the jaws and neck shares a liability with the rest of the dermal covering. These we may leave unconsidered; their rarity and

their other symptoms remove them from the category of barber's itch. The three diseases commonly seen upon the bearded face are—

1. Eczema barbæ, or ordinary eczema.

2. Perifolliculitis barbæ, or sycosis non-parasitica.

3. Trichophytosis barbæ, or sycosis parasitica, or sycosis, or ringworm.

Let us briefly consider their distinguishing features.

1. *Eczema barbæ* does not differ essentially from eczema as it occurs upon other portions of the body. It is a superficial, catarrhal inflammation of the skin, slightly modified by location. It is not necessarily confined to the hairy parts, since the capillary structures are not primarily involved. Hence it may spread from behind the ears, or from the alæ nasi to the bearded face or lip; or, beginning on the hairy skin, it may involve the lips, nose and ears—favourite seats of the eczematous process. The integument affected is reddened and swollen, and secretes abundantly the peculiar gummy serum characteristic of dermal catarrh. The hairs are matted together, and, with the serum, form dry, brownish crusts. In every case, however, the skin itself is intact; a catarrhal inflammation never causes ulceration or destruction of tissue. Removal of the gummy crusts discloses a reddened, inflamed, weeping skin, but no cicatricial tissue or destruction of the hairs. The patient complains much of the itching, as well as of the disfigurement and discomfort caused by the scabs.

2. *Perifolliculitis barbæ* rejoices in as many names as does the heir to a European throne. One of the commonest is sycosis, either alone, or with the adjunct "non-parasitic." This name is to be rejected. It does not describe the disease, and it leads to endless confusion with parasitic sycosis. Both maladies, in fact, are called "sycosis," simply, by different authorities. Perifolliculitis, on the other hand, is descriptive and unmistakable, and it places the malady in its natural relationship with perifollicular inflammation of other portions of the skin.

Perifolliculitis of the beard consists of a pustular inflammation affecting the hair-follicles and the tissues in their immediate

vicinity. It therefore commences in the beard, and is strictly limited to it. It begins with the appearance of a few red, conical papules, each of which has a hair implanted in its centre. The skin between the papules is unaffected. The cell-exudation soon leads to pus-formation, and we have the characteristic lesion of the disease, an inflammatory pustule pierced by a hair. These hairs, though firmly implanted during the early papular stage, are, of course, loose during the later pustular period, extraction being then painless, the entire hair-sac lying loose in the minute abscess. Not every hair of the area affected is involved, so that there are always healthy tissue and normal hair between the diseased glands. But each hair-follicle affected is destroyed; a minute scar takes its place; other follicles in the neighbourhood are subsequently affected; and so, finally, extensive areas may be denuded of hair, and the place of the skin be taken by a thin expanse of cicatricial tissue.

3. *Trichophytosis barbæ* is known as parasitic sycosis (a name to be rejected for reasons already given), and as tinea barbæ, and is simply a ringworm of the beard. It may or may not coexist with ringworm of neighbouring or distant parts of the body. When fully developed, its appearance is quite characteristic. The hair of the beard being very deeply implanted, reaching far down into the subcutaneous connective tissue, the growth of the parasitic fungus is not interfered with by the connective tissue of the cutis; it develops freely, and soon leads to the formation of large, deep-seated nodules and tubercles. These masses are characteristic. The surface of the skin may be normal in some places, whilst in others it is fissured, inflamed, or desquamating. The hairs issuing from the nodular masses are broken off close to the skin, and are frayed at their ends, as in ringworm of the scalp. Suppuration around the follicles and permanent destruction of the capillary structures result.

These three maladies, then, constitute the unscientific entity known as barber's itch. The differences between them are usually so marked that the diagnosis is easy. How are they to be avoided? If the hygiene of the barber-shop were all that is necessary, our task would be an easy one.

Eczema of the beard is in no sense a contagious disease. It has never been caught by one patient from another suffering from it; it is not inoculable. Yet more than half of the cases of barber's itch are cases of simple eczema. Neither the barber nor any of his customers has anything to do with it. Eczema of the beard, like eczema elsewhere, is due to certain external and internal conditions. External irritants, chemical or mechanical, will cause it; and an acrid nasal discharge is responsible for a considerable number of eczemas of the upper lip. The internal causes are not very definite; but it is agreed that dyspepsia and inefficient renal action predispose to dermal catarrhs. The avoidance of this form of barber's itch is therefore dependent upon the avoidance of certain external irritants and the care of the general health. Improper—*i.e.*, too strongly alkaline—soaps are especially to be avoided, and the digestive and renal functions should be kept in order.

Nor is perifolliculitis of the beard a contagious disease, save in the limited sense that all pus may be contagious under certain conditions. Irritating powders and cosmetics, exposure to dust or to intense heat, are the causes of the malady, which is especially likely to occur in those in whom the hair of the beard is stiff and wiry. A localized or mild eczematous condition often causes so much irritation that the finger-nails are freely used, and by these foci of purulent infection pus-organisms are implanted in the hair-follicle. Thus eczema and perifolliculitis frequently coexist, but the predominance of one or the other morbid process is generally marked. The prophylaxis of perifolliculitis consists, of course, in the avoidance of the agencies that cause it. The face-powders used after shaving are undoubtedly responsible for some cases. Thus, again, in this second variety of barber's itch the barber is entirely innocent.

Ringworm of the beard, the third and last variety, is a frankly contagious disease. Whether or not there is more than one variety of the trichophyton parasite is a question still under discussion; but ringworm of the beard is always derived from another case of ringworm, and this malady is sometimes carried from one person to another by the barber. I say sometimes; I

might say rarely, for the chances of contagion in this manner are small. The razor itself touches the skin only with its edge, which is frequently wiped and stropped; the fungus could hardly adhere to its blade. The lather-brush, the towels—but especially the barber's hands and finger-nails—are the mediate carriers of the contagion. The prophylaxis is cleanliness; boiling of the towels; thorough cleansing of brushes, or, better still, individual brushes and cups; the use of the nail-brush on hands and finger-tips after each shave; with these, ringworm of the beard may surely be avoided. There has been improvement in the hygiene of the barber-shop of late years, but enough cheap and careless ones remain to maintain in our dispensaries and clinics a steady, though small, stream of cases of trichophytosis barbæ.

And now as regards the cure of these diseases of the bearded face, they are frequently obstinate, prone to relapse, and annoying to the physician. An accurate diagnosis is necessary, for in certain essential particulars the treatment required differs in these three maladies. Certain general rules may be laid down. The general health must be cared for; the gastro-intestinal and renal functions must be examined and regulated; sources of irritation—bad soaps, dusting-powders, exposure to dust and to the inclemencies of the weather—must, if possible, be eliminated. Nasal and aural discharges must be cured.

Fracture of the Vertebral Column caused by Muscular Action.—M. Verneuil (*Rev. de Ther. Med.-Chir.*, Oct. 15, 1892) mentions the case of a man aged 49, in whom a fracture of the vertebræ was caused by a severe effort, due to slipping on the ice. There was intense pain, which yielded to rest. There were two attacks later due to the same cause. Five months later a prominence was made out, which led Verneuil to believe that a fracture had been caused by the muscular action. He treated it with a jacket of plaster and moulded leather. The conclusions drawn were that such fractures are difficult of recognition, that it is well to treat injuries to the spine with more care than is ordinarily shown, and that plaster jackets are required much more

frequently than is usually thought. M. Polaillon reported a somewhat similar case, where the sixth cervical vertebra had been fractured by muscular action. He immobilized the neck by means of a plaster collar, but the patient, believing himself cured, went to work too soon and suddenly died. The autopsy showed a hemorrhage compressing the cord.

Etiology of Whooping-Cough. — Galtier, (*Lyon Médical*, Dec. 11, 1892), sums up the conclusions arrived at from some experiments regarding the causation, as follows:

1. Whooping-cough is due to a microbe.

2. It is caused by an aerobic germ which is easily cultivated, which exists in the more solid portion of the expectoration, and is of a rounded form.

3. That inhalations and gargles of turpentine are most useful in the treatment.

4. That it can be transmitted to certain animals (rabbits, dogs, chickens), but the dog and the hen are most susceptible.

Treatment of Lupus.—Thayer (*New Orleans Med. and Surg. Journal*, Nov., '92) gives the treatment as follows:— The very first indication in commencing the treatment of lupus is to endeavour to modify the general condition by appropriate treatment. The patient should at once be placed under the best hygienic influences, with proper food and regimen. The system of the patient should be newly built up as it were. The disease itself is to be combated at the same time by such external and internal remedies as appear to exert a salutary influence on the development and progress of tubercles and the ulcerative processes. Local applications to the ulcerative surfaces, such as caustics and powerful astringents, have been generally relied upon, Nitrate of silver, potassa fusa, butter of antimony, supernitrate of mercury, arsenical powders and pastes, scraping out the diseased tissue with curette or spoon, and last, but not least, "the solar cautery," using the concentrated rays of the sun for this purpose. When the disease is extensive the cauterization should be done with great caution. It should be confined to a single part at one time, and from

time to time extended successively to the whole of the affected surfaces. When the ulcers are covered with scabs they must previously be gotten rid of by the use of poultices. During the treatment the patient should avoid exposure to heat or rigorous cold, and dampness. For want of attention to these precautions cicatrices that appeared sound have frequently been seen to open again afresh. When the disease is accompanied with any violent functional disturbance this must be remedied by appropriate treatment; preparations of iron, iron with bitter compounds, iodine preparations, sulphur baths, etc. The unmedicated baths of hot water, used daily, have, under my directions, proved of great value in lupus. Food of good quality well cooked, a residence in a dry and bracing atmosphere, are powerful modifiers of constitutions; cheerful surroundings, proper moral influences and encouragement act as tonics to the depressed mind and render life more valuable to the individual.

I now come to what I consider the most important and instructive point in this paper; to wit: The successful treatment of lupus with concentrated rays of the sun, which I have named "the Solar Cautery." Some twenty-five years ago, while experimenting with a common lens (small sun-glass), I focused the rays of the sun upon a small mole upon my arm. Instantaneously it began to smoke, and within a few seconds it was destroyed: No pain or inflammation followed the operation; the burned surface healed readily, leaving no trace of its former self, not even a cicatrix. This simple experiment suggested the idea that the lens could be used with success in destroying morbid growths. I at once put this idea to practical tests. From that time to the present I have continued to make use of the solar cautery in a variety of surgical diseases, and have rarely indeed experienced any disappointments. It has advantages over and above all caustics. Some of these are the following: It is, at all times, under the control of the operator. Its action stops the moment the lens is removed. No pain follows, as the concentrated sun rays act as an anæsthetic. It will destroy morbid tissue, while the adjoining normal tissue remains uninjured owing to its great vitality. Very slight inflammatory action follows its use.

In a case of extensive disease of the upper lip the solar

cautery was used as follows: After a most thorough and critical examination of the diseased tissues, and learning from the patient (a very intelligent, common-sense lady) the history and treatment of her case during the last two years, I decided to apply the solar cautery to the diseased surfaces. My confidence in this remedy gave me great hope that the treatment would prove successful. With a powerful lens with a focal diameter of three lines, with a clear sky and unobstructed sunlight (essential in the success of the use of the solar-cautery), I most thoroughly cauterized the diseased surfaces, destroying the morbid tissues. This was accomplished in the space of two minutes of time. The cauterization was not very painful at the burning, all pain ceasing after the removal of the lens. I cannot recall one single instance where severe pain continued for any length of time from the use of the cautery. Having treated by this method more than one thousand cases, I certainly would remember some of those complaining of severe pain after the operation. I dressed the burned surfaces with zinc ointment, over which was placed a layer of absorbent cotton, wet in a 5 per cent. solution of carbolic acid, followed by a thicker layer of dry cotton. The next day there was more or less swelling of the parts, some redness of the adjoining skin, with more or less tenderness. Thirty-six hours after the operation an improved condition was visible. The same dressing was continued, being changed daily. The improvement went rapidly forward, the discolouration of the lip and nose disappearing from day to day. Two weeks after the operation this lady presented herself at my office with the ulcerated surfaces most thoroughly healed, with a slight but smooth cicatrix. The discolouration of the skin in the immediate neighbourhood had disappeared and the diseased surfaces assumed quite a natural appearance. This patient left me, as you will naturally anticipate, a very grateful and happy woman.—*Pacific Medical Journal.*

Cardiac Syphilis. (By Prof. Mariano Semmola.)—Syphilis of the heart forms a recent chapter of internal pathology and clinical medicine; nevertheless, I believe it deserves the fullest attention, not only with respect to the ætiolology of heart affections in general, but also from the

standpoint of treatment, since we possess a fairly marvellous therapy in syphilis. All cases of cardiopathia in which the lues form an etiological factor may be substantially modified by radical treatment; and in these instances the good offices of the physician should not be confined to the transient and symptomatic results which are to be achieved with so-called heart-remedies. In the following I shall not dwell on the cardiac affections which, of secondary character, follow more or less localized syphilitic arterites, nor upon those cardiopathias which are developed under the influence of either diffusing arterio-sclerosis or arterio-sclerosis of the coronary arteries. Nor shall I treat of the primary valvular cardiopathias in acute arthritis rheumatica which are accidentally accompanied with syphilitic lesions of the heart.

I shall here limit myself to directing your attention to an altogether different species of primary heart affections, which gain their development in veteran syphilitics and which may frequently reach the degree of a severe, non-compensated heart disease, to say nothing of the influence which an ancient and latent syphilis may have on the development of these heart diseases. These latter heart affections, which, as it seems to me, have received scant consideration from the clinic, constitute the most treacherous and fearful cardiopathias, since they often begin with insignificant functional disturbances, which are regarded as nervous phenomena; and, in truth, neither auscultation nor percussion reveals the signs upon which a diagnosis of organic heart-defect may be based, although an organic lesion already exists. The latter, however, is still so slight, and develops so slowly, that the manifest signs of a morbid process of the myocardium, of the heart ventricle, etc., are not yet presented, simply because *they cannot exist*, the initial morbid process not having as yet produced those subsequent pathological phenomena (dilatation, hypertrophy) which form the ominous clinical tableau of a heart disease. The patient neglects these as yet unimportant functional disturbances, and is content with the employment of some calmatives. Meanwhile the morbid process progresses insidiously, aggravating from day to day, and finally assumes formidable proportions, so that then—and frequently too late—the attention of the patient and the physician is seriously directed to the malady.

Among these rudimentary functional disturbances there is also one apparently insignificant symptom which the physician as a rule regards slightingly. I have seen patients and physicians lull themselves into confidence, not suspecting that they had before them a serious *arythmia*, uncomplicated or (as is frequently the case) accompanied with tachycardia. Every day clinical lectures and treatises are published on the therapy of cardiac arythmia; its nervous or arterial origin is discussed; we decide upon the use of antispasmodics, or we recommend atropine, preparations of bromine, and, in cases of arterio-sclerosis, iodine combinations as well; but in none of these papers have I seen allusion to a continuous persisting arythmia which resists all such medication. And yet there is such an arythmia; and when it fails to yield after essaying various methods of treatment, I believe the physician is justified in assuming the existence of a latent process of the myocardium, syphilitic in origin, and in at once beginning the specific treatment, as we know full well that the mercury treatment when tardily commenced is of no use; for the final anatomical sequelæ fatty degeneration, scleroses are no longer amenable to the curative influence of mercury.

The relations between syphilis and the heart were announced as early as 1859 by Virchow, on the basis of simple anatomico-pathological observations. Nevertheless, for a long time the clinic gave scant attention to this subject. In recent years, for the first time, isolated studies of "syphilis of the heart" have been published by Bogosslowsky, Jakarine, Nekan, Lang, Mauriac, Jullien, and some others. In general, all these authors based their reports either upon clinical cases of cardiopathia which were cured by the mercurial treatment, or upon anatomico-pathological observations of cases which *intra vitam* had exhibited very severe cardiac symptoms, discovering an autopsy simple or gummatous myocardites which revealed their syphilitic character.

Such is the present condition of this question. Inasmuch as I have always cherished the conviction that those heart diseases are the gravest which originate in defective nutrition of the heart muscle, with or without points of localization, although always in connection with dystrophic influences proceeding from the general nutrition, I have devoted my atten-

tion to this subject since the year 1883. At this time I had opportunity to observe a cardiopathic physician with the severest symptoms of a non-compensated heart affection. All known tonic heart remedies were employed, but without avail, and the patient's doom seemed inevitable. From the account of his life I learned that he had once contracted syphilis; that the gravest syphilitic phenomena had repeatedly made their appearance; that they had always, however, been caused to vanish by appropriate mercury-iodine treatment. In coming under my observation the patient exhibited absolutely no syphilitic symptom. In such instances, however, I have long adhered to an invariable principle, imparted to me thirty-five years ago by my master, Ricord, who was wont to declare: "If syphilis has once indubitably been contracted, then everything is possible—even the impossible." I therefore began to treat my patient with subcutaneous injections of sublimate, and prescribed large doses of sodium iodide (5 grammes within 24 hours). After four weeks of this treatment I presented the patient to my pupils; he was almost free from œdema, without palpitations, without arythmia, etc. The improvement continued, and our patient was enabled subsequently to resume his medical practice, and this without the administration of digitalis or any other cardiac remedy.

Since that time I have never, in my examinations of cardiopathic patients, failed to take into account their possible syphilitic antecedents. Thus I have been enabled to classify all my cardiopathic patients in two well-defined groups. In the following I mention another case, the last observed by me, to which I would direct your special attention.

Baroness B., æt. 45, has led a "joyous" life for twenty-five years, and has repeatedly disclosed the most classic symptoms of syphilis. During September of last year she came to me for treatment. Patient stated that in the main she felt very well and suffered from no pronounced complaint; in mounting stairs, however, she experienced great irregularity of the pulse and dyspnœa. She had already consulted various physicians, who in turn prescribed atropine, sparteine, and other medicines, without producing any improvement in her state. A nervous arythmia had been diagnosed, because, in truth, the most rigid physical examination failed to reveal either a noise

or any focus of alteration of the heart. I could not, however, convince myself that a purely nervous arythmia would be so persistent as the patient declared. I recommended absolute rest for three or four days, and explained to her that a daily examination of her condition would be indispensable, in order to obtain an assurance with respect to the persistence of her arythmia. After I had satisfied myself that this arythmia did not cease, even with absolute rest of the body and mind, I concluded, in view of the lady's antecedents, that her arythmia was not of nervous origin, that the case was rather one involving the sequelæ of a syphilitic infection (probably an initial myocarditis or a gummatous infiltration)—although a close external examination of the patient failed to disclose the presence of any characteristic signs of syphilis.

On the basis of this diagnosis I was, of course, obliged to prescribe a specific and rigorous treatment, as in the previous case (sodium iodide per os, and sublimate injections, during the first month; subsequently, for the period of five months, inunctions of mercury—1 gramme daily). The arythmia, as well as the disturbance of breathing, vanished completely within two months, and the patient at present enjoys excellent health.

As before stated, I have never omitted, since the first case of syphilis of the heart came under my notice in 1883, to enter upon a specific treatment with cardiopathic patients who have previously suffered from syphilis—of course, observing all the precautions indicated by the general condition of the patient, and keeping in sight, above all, the cachexia, that I might not later have occasion to deplore the unpleasant after-effects of the treatment. To this treatment I resorted especially in the cases of cardiopathia not attended with noteworthy alterations of the valvular apparatus, and, above all, in those cases in which treatment with cardiac tonics had proved futile.

In a large number of cases I have been able to establish some improvement in the general condition and in the heart function without achieving ultimate success, and the fate of these patients was about the same as if they had not undergone the specific treatment. Nevertheless, even in the latter cases I had the conviction that the specific treatment was very well borne—a fact which I was enabled to corroborate through

hæmochromometric examinations, which I have been making in my clinic for the past three years for the purpose of estimating the action of the so-called alterative medicaments. It is certain that if under the influence of the mercurial treatment the quantity of hæmoglobin increases, even though in slight measure, there must exist in this organism a special susceptibility to the biochemical action of the mercury, without which this agent would undoubtedly tend to destroy the red blood-corpuscles.

The slight general improvement, however, naturally could not, as I have before stated, exert any favorable action on the last alterations of the syphilitic process of the heart. The improvement of the general condition in a large number of cardiopathic patients in whom the anamnesis disclosed syphilitic antecedents, nevertheless, shows peremptorily that if the specific treatment had been instituted earlier these patients might have been enabled to escape their doom and live much longer. Hence, if the chapter of cardiac syphilis from the pathologico-anatomical point of view may be regarded as sufficiently illumined, the same does not apply to the diagnosis, to which in this study I desire to make a contribution.

Besides this series of cases which, as stated, seem dubious owing to the very incomplete and transient curative results, in the course of nine years I have collected a second series of cases (twenty-seven in all) in which I was in position to enter upon specific treatment immediately at the beginning of the cardiopathia, as in the instance of the Baroness B. cited. The classic symptom of such a commencing cardiopathia is the *persistent arythmia*, at times uncomplicated, at times accompanied with tachycardia, with the occasional collateral symptom of *impeded respiration*. In all these cases there is not the slightest valvular defect involved. The objective examination revealed only persistent cardiac ataxia. After meals or during states of emotion the arythmia at times increased into a veritable *delirium cordis*. The heart, however, never returns to its normal rhythm—not even after prolonged rest or during sleep. Almost all these patients were enabled to follow their occupations and suffered no difficulty in breathing, provided they did not over-exert themselves in their labours.

All the twenty-seven patients who have come under my

treatment in the course of eight years enjoy to-day a perfect cure from their arythmia. With each patient the specific treatment was renewed every year for two or three months, while the iodine treatment was continued throughout the year.

I believe the following conclusion may be drawn from my clinical observation: "When a patient who has passed through an unquestionable syphilis presents himself to the physician with symptoms of persistent arythmia which yields neither to hygiene nor to pharmaceutical remedies, the physician must straightway assume that a syphilitic process is at the bottom of the trouble, and he must prescribe to the patient a specific treatment, even if there be no symptom exhibited at the time to supply a palpable proof of constitutional syphilis."

In more fortunate cases, where the mercurial treatment produces cure, do we encounter a beginning myocarditis or a gummatous infiltration? In this respect I must avow my ignorance, and I confine myself to the establishment of the therapeutic connection between an obstinate arythmia, recovery through specific treatment, and the assumption that this functional disturbance is related to a syphilitic process prevailing in the heart-muscle. In the present state of our knowledge of this subject, more cannot be said. Further, I am not able to assert that the arythmia repeatedly mentioned in the foregoing is a characteristic sign of a syphilitic myocarditis, for such a conclusion would indeed be premature. All the symptoms of a pronounced process of the myocardium, such as we observe in practice, possess in reality, so far as their syphilitic nature is concerned, nothing characteristic; they are simply the symptoms of serious disturbances of circulation which necessarily ensue in alterations of the heart, in consequence of the incapacity of the myocardium. When the patient dies the autopsy reveals syphilitic lesions in the myocardium, but it is impossible to render clear and distinct the ominous clinical tableau in order to trace back the initial functional disturbances which would permit the diagnosis of a beginning myocarditis. On the other hand, if the patient recovers as a result of the specific treatment, we still forego the anatomical demonstration, and content ourselves with the simple conjecture of the presence of a syphilitic myocarditis.

The purpose of the foregoing report is simply to establish

the connection prevailing between an ancient syphilis and recent cardiac disturbances, and the cure of the latter by specific treatment; it forms a clinico-therapeutic contribution to the history of cardiac spphilis. Supported by similar reports, diagnostic data should be acquired in the future which will be of great service to the physician in presenting an appropriate and seasonable treatment.—*Medical Age*, Nov. 25, 1892.

The Treatment of Myxœdema. — The *Lancet* reports a case of myxœdema now under treatment by the use of fresh thyroid glands as food. The patient is a woman aged forty, who for nearly three years has been under treatment for myxœdema. In every respect it is an extremely typical case: persistently subnormal temperature, general swelling of the integuments, dryness of skin, partial baldness, clumsiness of movement, slowness and thickness of speech, etc. The patient was admitted to the Royal Free Hospital, for the third term, in order to be treated by hypodermic injections of thyroid juice. In consequence of the difficulty experienced in obtaining the latter, Dr. Mackenzie was induced to try the effect of feeding the patient on fresh thyroid glands. The result has been a very striking improvement. The myxœdematous swelling has entirely disappeared, the temperature has become steadily normal, the skin moist and the speech natural. The patient says she feels as well as ever she did in her life. The change in her appearance is such that the existence of the disease would probably not suggest itself to any seeing her now for the first time. At first the thyroids of the two sheep were given every day finely minced. This was probably more than was advisable, because a remarkable acceleration of the pulse ensued, which lasted until the thyroids were discontinued for a time. In view of the recent observations on the advantage of the thyroid juice when given in the form of injections this method, if responding favorably to further observations, would seem to constitute a distinct advance. It can easily be carried out and is free from risk, which cannot be said of the subcutaneous mode of treatment.

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THE EXPECTANT TREATMENT OF TYPHOID FEVER.

In a recent number of the *Medical Record* Dr. Arnot Spence, of New York, has an interesting paper on the expectant treatment of typhoid fever. This paper is founded on the results of treatment in 323 cases of typhoid treated in the St. Francis Hospital, of New York city, under the care of Dr. J. H. Ripley. Of the 323 cases, 47 died—a mortality of 14.23 per cent. From the 47 deaths 12 occurred within 48 hours after admission. If these are omitted, the death rate is reduced to 11.25. A specially interesting part of the report is that relating to the cause of death. Leaving out the twelve moribund, we have eleven cases either from hemorrhage or intestinal perforation, the remainder being due to the direct exhausting effects of the poison. Dr. Spence appears to be well satisfied with the results of treatment. In face of the results obtained by the Brand treatment, it is strange to think that any physician should be satisfied with having a no greater mortality than eleven per cent. Brand and his followers would claim that twenty, at least, of the deaths in the cases reported might have been prevented under the cold water treatment. It is claimed, and we believe with justice, that the cold water treatment practically eliminates deaths from broncho-pneumonia and those due to cardiac failure from direct action of the poison. Fully one-half of the deaths in Dr. Spence's tables were caused by the above conditions. It is more than probable that had a strict hydriatic treatment been carried out the rate of mortality

would have been nearly a half less. In private practice the difficulties in carrying out the cold bath treatment are often very great, but in an hospital the only excuse for not following this method of treatment is a belief in the efficacy of other modes of dealing with the diseases. From the results obtained by the hydriatic treatment in Europe, and also on this side of the Atlantic, we believe that there is overwhelming proof that the death rate is much lower (probably from 4-6 per cent.) than from any other mode of treatment.

Further, convalescence is undoubtedly shortened—that long period of physical and mental debility, so common and so marked after severe cases of typhoid, is seldom or ever met in cases treated with cold water.

THE LETTER OR THE SPIRIT.

Article 1, section 3 of the "Code of Medical Ethics of the Canadian Medical Association, adopted by the Montreal Medico-Chirurgical Society," says: "It is derogatory to the dignity of the profession to resort to public advertisements, or private cards, or hand-bills, inviting the attention of individuals affected with particular diseases." The section goes on to particularize regarding the advertisements and to state the only possible exception to the rule that members of the medical profession must not appear in the public prints.

In a recent number of a Canadian medical publication we noticed, while looking over the advertisements, that, in addition to the usual notices of proprietary medicines, babies' foods, etc., no less than three Canadian medical men had announcements to the medical profession regarding their hospitals for the treatment of certain cases. The advertisements all read very much the same: "Dr. Specialist begs to announce to the medical profession that he has opened a private hospital for the treatment of such-and-such cases." By addressing the medical profession they steer clear of the letter of the law, which says that they must not address individuals affected with particular diseases. But do they observe the spirit of the law

by thus addressing the individuals through their attending physicians? We think not, and, in order not to be misunderstood, we will state exactly wherein the offence lies. This part of the code is based on the fact that when a tradesman advertises he merely recommends the wares in which he deals, but a doctor of medicine has no wares to advertise; his stock-in-trade is his education and abilities; he has merely himself to offer, and to recommend himself openly savors strongly of conceit. The advertising of a sanitarium or inebriate asylum is not open to this objection, as they have something quite apart from medical treatment to offer. But the advertisements to which we refer are direct bids for patients, the treatment of whom does not require anything which may not be found in any well-equipped general or private hospital, of which there is an ample supply in all our large cities, without medical men going into the boarding house business and parading the fact in the medical journals as if they were proud of it. If the advertisement stated honestly what it meant it would read something after this fashion: "Dr. Specialist begs to announce to the medical profession that, as they are not capable of treating certain cases, they will do well to send them to him, and, for mutual accommodation, he will take them into his own house, or next door, and treat them in the most approved fashion." But the modesty of these gentlemen is too great to state things in this rude, straightforward way, and it also prevents them from mentioning the fact that they are all professors in various Canadian Universities. It is a great spectacle to the students and graduates of these Universities to see their professors, men to whom they look for both precept and example, thus openly violating the spirit of the law, while steering clear of the letter. It is plainly to be seen that we live in a commercial age, when the pen is mightier than the sword. We were formerly taught that good wine needs no bush, and we still believe it to be as true as it was in the days of Shakspeare. In a city a specialist who has the confidence of his fellows will soon be widely enough known without calling in the aid of that potent agent, "the little ad."

POPULAR IDEAS OF HYPNOTISM.

The public has very crude ideas on many subjects, but on none more so than hypnotism, or its predecessor, mesmerism. The novelist, in particular, is a firm believer in it, and well he may, for it is a good friend to him and helps him out of many otherwise impossible situations. We have come across a good many specimens of the romancer's art in which hypnotism plays its part, but in none is the rôle quite as absurd as in a story which appeared in a late issue of a daily paper. A young lady, wealthy, beautiful, and all the rest of it, suddenly falls while drinking a glass of wine at a party, and is picked up apparently lifeless. The account is very circumstantial. "The most scientific tests of the existence of life were made. A mirror was held before her nostrils, no sign of life. A string was wound tightly around her fingers and unwound, but the flesh did not change colour. Lastly, her flesh was punctured with a needle, and the hole remained open after the point was withdrawn. Yes, Emily was dead." The body is borne to the grave-yard, followed by a vast concourse of mourners, and is placed in the vaults. That night the *dead* lady moves in her coffin and, thanks to an electric alarm, is immediately rescued from her perilous position. A doctor is called, and being unable to restore her to consciousness, suspects hypnotism. An interested friend plays the part of detective and extorts a confession from a waiter in a restaurant to the effect that, tempted by a large bribe from the residuary legatee, he had hypnotised the lady by means of a glass of magnetised wine. She was to be left to die in the hypnotic state, but by a chapter of accidents the plot was frustrated. Of course the story ends by the marriage of the amateur detective and the heiress.

When daily papers, while setting up to be popular educators, publish stories like these, can we wonder that people resort to quack medicines, faith cures, etc., for all imaginable ailments, wasting both health and money.

—The "International Medical Annual" for 1893 is now in press and will be shortly issued. This is the eleventh year of its publication, and each year it has improved. It contains a

report of all the new remedies and modes of treatment introduced during the year, selected by thirty.six department editors from the most recent American, British and Continental literature. It is a book of reference which every one should have to enable him to keep up with the times. It is published by E. B. Treat, New York.

Obituary.

ROBERT HUGH BERWICK, M.D., L.D.S.

With deep regret we chronicle the death of Dr. R. H. Berwick, which occurred on January 20th, 1893. He has left a gap in the ranks of the dental profession in Montreal which it will be difficult to fill, for though a young man he had already a large practice, and was greatly looked up to and respected. He was born at Farnham, Que., June 18th, 1866, and in 1888 received his Licentiate for the practice of Dental Surgery. Not content with this, he studied Medicine at McGill University, obtaining his degree in 1891. He then entered upon the practice of dentistry in this city, and by his ability and close attention soon obtained a large clientèle. This was the real cause of his death, for his intense application undermined his health and he fell a prey to that dread disease—tuberculosis. Dr. Berwick was one of the moving spirits in the new College of Dentistry, and was Professor of Dental Surgery. He also held the position of Dentist to the Montreal General Hospital. We desire to express our sympathy with his family, and to record our sense of loss in the cutting short of so bright and promising a life.

Resolutions of Condolence.

At a meeting of the Medico-Chirurgical Society of Montreal held Jan. 20, 1893, it was moved by Dr. Mills, seconded by Dr. Proudfoot, and

"Resolved,—That this Society has heard with deep regret of the death of one of its members, Dr. Robt. Hugh Berwick, who, though a young member of the profession, was one of the most promising, and one who had gained the respect of all with whom he had come in contact during his brief but successful career.

"Resolved,—That a copy of this resolution be sent to the friends of the deceased, to the Dental Association of the Province of Quebec, and to the Press."

At a meeting of the dental students the following resolution was unanimously passed:

"That the faculty and students of the Dental College of the Province of Quebec sincerely deplore the great loss sustained by the dental profession, and especially by the College, in the death, after a long and painful illness, of their confrère and teacher, Dr. R. Hugh Berwick, Professor of Dental Surgery, and one of the most promising members of the profession."

At a meeting of the Montreal Clinical Society, held Jan. 28, 1893, it was unanimously

"Resolved,—That the members of this Society, realizing their loss in the death of Dr. Hugh Berwick, one of the members, desire to express their sympathy with his relatives and their sorrow for his untimely death. His honourable dealing and his devotion to his profession had gained the respect and admiration of all who knew him, and though but such a short time in practice, he had won a reputation for himself second to none in the city."

ALPHONSE BARNALIE LAROCQUE DE ROCH-BRUNE, M.D.

Dr. LaRocque passed away on the 14th of January, in his sixty-sixth year. He was the son of the late Charles LaRocque de Roch-Brune, member of Parliament for Soulanges. After graduating from St. Mary's College, at Baltimore, he entered upon the study of Medicine at McGill University, and received his degree in 1847. He was the first Medical Health Officer of the city of Montreal, which position he resigned for private reasons after holding it for some years. He published several pamphlets on hygienic questions, and had always a keen interest in sanitary science. He did much to promote good feeling and sociability between the French and English-speaking inhabitants of Montreal, and, latterly, devoted much of his time to this worthy object.

Medical Items.

—Péan delivered his last lecture at the St. Louis Hospital on December 24, 1892, having reached the age at which the surgeons are compelled to retire.

—The spring term of the London Post-Graduate Course (4th year) began January 16th and continues to March 11th; the summer term begins May 1st and continues to June 24th; and the winter term, October 9th to December 2nd.

—Pasteur celebrated his seventieth birthday on the 27th of December last by a reception held in the amphitheatre of the Sorbonne. The President of the Republic, all the high government officials, the members of the Academy and the leading scientists were present to congratulate him. Addresses were presented from England, Russia, Germany, and many other countries.

A GYNÆCOLOGICAL ERROR.—Dr. George Ross, of Richmond, Va., reports the case of a woman who had suffered from unremitting, agonizing tenesmus, the result of a mass which she had carried in her bladder for seven years, and which proved on inspection to be a pledget of absorbent cotton once saturated with iodine, a truncated cone in shape, and thinly incrusted with phosphate of calcium. The patient believed that it had been introduced by her first physician, who, when attempting to apply some intra-uterine dressing, had mistaken the urethra for the cervical canal.