Paper read before the Birmingham and Midland counties branch of the British Medical Association, the Midland Medical, the Oldham Medical, and the Devon and Exeter Medico-Chirurgical Societies / by M. Armand Ruffder.

The

British Institute of Preventive Medicine.

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BY

M. ARMAND RUFFER, M.A., M.D. (Oxon), Honorary Secretary to the Institute.

Gentlemen,—In the first place, I have to thank the President of the Society for giving me the opportunity of coming before you to-night as the representative of the Council of the British Institute of Preventive Medicine, in order to explain, to the best of my ability, the objects of this Institute. In the second place, I must thank all those who have come here to-night to meet me for their kindness in attending, and, in return, I can only say that I will try and be as short as possible, and not detain them a moment longer than I can help.

I should have wished that some more influential member of our Council could have addressed you to-night, but, in their absence, I hope you will put up with a less known member, and grant me your attention for a few minutes.

The Institute was born, so to speak, on July 1st, 1889, when the late Lord Mayor, Sir James Whitehead, convened a meeting at the Mansion House "in order to raise a sum of money to be presented to M. Pasteur;" and another sum for sending poor people bitten by rabid animals to be treated by that gentleman.

You may perhaps remember that both objects for which this fund was raised have been attained, for in November of the same year the Committee forwarded £2,000 to M. Pasteur, together with the autograph of our Chairman, thanking M. Pasteur for the kindness he had extended to over two hundred English patients bitten by rabid animals, whom he had inoculated gratuitously; and, ever since, all the patients who have applied to the Mansion House Fund, and who were unable to pay their journeys to Paris, have been helped and sent by us to that city.

In November, 1889, the Committee decided that it would be well to establish in England an institute for the special study of bacteriology and preventive medicine, similar, to some extent, to the institutes already existing in Paris, Berlin and St. Petersburg.

I suppose what I first have to explain is—What is the work which will be carried out in this establishment, and what is the good of such

an institute? In order to do this, I must first remind you of what bacteriology has already done for the human race and for animals.

There is one problem which from time immemorial has presented itself to the mind of every thinking person—namely, what is the actual cause of fermentation; what is the actual cause of infective disease? The idea that infectious disease is analogous to fermentation has been entertained by many since the infancy of chemistry and medicine; but it is only within this century that the plants which cause substances to ferment have been found to be of the same kind as those which cause disease, and which prove fatal, at some time or other, to the majority of the animal kingdom. It is strange to read, even now, the disputes which raged shortly after Leeuwenhoek first discovered bacteria in human excrement. It was even then supposed that these living germs might be the cause of fermentation and disease, and huge, ponderous volumes were written as to their supposed presence in various organs and in various fluids, including the fluids of the body after death from infectious disease.

It is hardly necessary to say that, with the poor optical instruments of that time, and when no one knew anything of the structure of the human body, all these suppositions should have been more or less erroneous. It is probable that the germs on which the whole doctrine of Pathology was based by one observer, and which are described as always being present in the blood, were nothing more nor less than deformed blood corpuscles.

The science of bacteriology lay forgotten for a long time, until early in this century, various observers began to investigate these lower forms of life purely from a botanical point of view, and in the early works published by Dujardin and others, it is astonishing to find pictures which might have been copied from some modern photograph plates. At that time, however, the methods for separating bacteria from each other were not known, and hence all these investigators were struck with the ever-varying characters of these germs; for the same infusion which one day showed nothing but micrococci, would be swarming next day with bacilli, which observers naturally assumed to have grown from micrococci. It was only when botanists had been helped by physicians, and had been taught to separate these various germs, that it was proved that microbes, just like the higher plants and higher animals, always breed true.

Towards the middle of the century we find various great men in all countries discussing the origin of life and the origin of the innumerable micro-organisms which appear in fermenting and putrefying fluids and solids. This was the great time when the battle raged between the partisans of spontaneous generation and those who maintained that spontaneous generation was a myth. When one reads nowadays the disputes which raged between Pasteur and Bastian, between Schwann and his opponents, between Tyndall and others, one can imagine how futile these must have appeared to people of that time. What did it matter, as some one said, whether cotton wool did or did not arrest all the germs that passed through it? What did it matter whether liquids in a hermetically sealed glass fermented or not? and whether this fermentation was

produced by the fluid itself or from particles coming in through the air? It was to many a pure question of fighting over trifles. Many who prided themselves on being *practical* men, sneered at the scientific enthusiasm which could be found to fight about trifles of this description; and yet, when one looks at it impartially now, one feels sure that if that battle had not been fought, if Pasteur and others (the greatest among whom is certainly Tyndall) had not *proved* once for all that putrefying pus will no more produce bacteria than the blood of a dead bull will produce bees, as the ancients thought, the doctrine of the spontaneous origin of disease would not even now be excluded.

By new methods it has been shown that the germs of disease breed true, just as the germs of putrefaction or the bacillus of anthrax can never be converted into the bacillus of typhoid, or the latter into the micro-bacillus of pneumonia. It is true that, by domestication, by altering their habits, by change of food, some of the properties o bacteria may be made to vary to a certain degree; nevertheless, the fact remains, that none of them can any more change their nature than the leopard its spots.

It is not easy to fight a hidden enemy, and, therefore, our power to combat disease has increased much since the discovery that the source of contagion is in most cases something tangible—a living organism which we can see, the habits of which we can study, the weaknesses of which we can discover, and which, in many cases, we can render harmless or even turn to our own benefit.

The great chemist, Pasteur, who was the first to prevent and cure the diseases of wine and beer, shortly afterwards discovered a means of preventing the diseases of animals, and even of man, by an efficient system of inoculation. I need hardly mention to a medical audience its results against anthrax, quarter evil, rouget and rabies, but I would remind you that these results have not been obtained in France only, but that there is now hardly a country in Europe in which the application of such methods has not been successfully carried out.

At the last meeting of the Congress of Hygiene in London, I wrote to all the directors of the various anti-rabic institutes in Europe and in other continents, and asked them to send me the official returns of the cases treated at their institute. It is a remarkable fact, and one which I hope will not grieve my illustrious friend, M. Pasteur, that his own statistics in Paris are, except for one institute, the worst of the whole lot, for I find that the per centage of cures obtained at other institutes, managed according to M. Pasteur's method, are even better than his own. The difference is very slight, but still exists. I do not think M. Pasteur is likely to complain of his pupils doing better than he does, and to my mind it shows, conclusively, that the method which he investigated has proved true, not only in his hands, but in those of practitioners in all countries.

The objections which have been raised against M. Pasteur's treatment of Rabies were, at one time, almost universal. One hears but little of them now, except in papers published by certain societies chiefly devoted to slandering Medical Science. It is curious that it is almost impossible to trace the source of these statements.

It has been stated, for instance, that the so-called Paralytic Rabies did not exist before M. Pasteur's time, and if ever an unfortunate patient treated by M. Pasteur dies after inoculation, with symptoms of paralysis, the treatment is blamed with that result.

Now, if Paralytic Rabies is due to the treatment, it is quite clear that it should never have occurred before M. Pasteur's time; and yet Morgani, in the 17th century, described typical cases of Paralytic Rabies occurring after the bites of mad dogs. I have collected more than thirty cases of Paralytic Rabies occurring in Europe during the last three years in people who never underwent M. Pasteur's treatment. I have in my possession cases of several persons bitten by one rabid wolf, amongst whom those bitten and then inoculated by M. Pasteur recovered, and those not inoculated died of Paralytic Rabies. I would specially refer in this connection to cases published by Gamalcia.

Inoculation for rabies ought to be, in my opinion, looked upon as a surgical operation. I have now collected over 600 cases, in which some persons bitten have been inoculated by M. Pasteur, whilst others bitten by the same rabid animal have refused to be treated. I find that the mortality among those not inoculated amounts to 14 per cent. at least; the mortality among those inoculated does not reach 1 per cent.

Now, suppose I, or one of my friends, were bitten, the argument which I would use is this: "If nothing be done, you have eighty-five chances in a hundred that you will recover; but, if a trifling operation be performed, your chances will increase to ninety-nine per cent. I can vouch that the operation is harmless, for all M. Pasteur's assistants, both at home and abroad, have been inoculated." Who, in the light of these facts, will hesitate for a moment to be inoculated? You have also heard, no doubt, that the deaths from rabies have increased in France since M. Pasteur's treatment has been introduced. The people who make these assertions generally base their remarks on the statistics supposed to be published by the Bureau d'Hygiène.

Now, when I first began to study rabies, I confess I could not understand how, even should M. Pasteur's treatment be absolutely useless, it could possibly produce rabies in people not inoculated; further, when I saw the extremely favourable results obtained, I determined to look a little into these statistics, and I found, to my great astonishment, that no statistics worthy of the name existed in France as to the number of cases dying of rabies before the year 1880. I found, for instance, that, in one year, the official statistics gave a mortality for the whole of France of thirty patients, but, strangely enough, in that year no cases are reported to have died from rabies in the department of the Seine. Now, knowing what a hotbed of rabies this department of the Seine is, I looked up the reports of the Prefect of Police and the Medical Officer of Health for the same year, and I found that although the official statistics showed no deaths from rabies, yet seven persons had died in that one department alone from this disease. What value can be attached to statistics such as these? Even now, although, thanks to M. Pasteur, attention has been drawn to this subject, the statistics, as I happen to know, as to the number of deaths

in France are utterly unreliable. I would sum up what appears to me to be the true conclusion we can come to with regard to this subject, as follows :—

"I would not be inoculated against an uncertain bite, but were I, or one of my friends, bitten by a certainly rabid animal, I would not hesitate for a moment, but take the first train to Paris and be inoculated."

If we now turn to the results which have been obtained against diseases of animals, we have what we may consider a better standard for judging of the value of Pasteur's works. I say a better one, although it may appear to many to be really lower ; but it was Spencer who first said that the mercantile test was perhaps the most searching test to which any doctrine could be put. Now, in France, the inoculations against anthrax have been put through this mercantile test, and over a million sheep have been inoculated in Europe during the last year. The results have been most striking, especially in Russia, where, thanks to the inoculations, the mortality amongst sheep from all causes fell last year from 20 per cent. to 1 per cent.

You will remember that in Germany, chiefly owing to the influence of Professor Koch, M. Pasteur's statements were at first to some extent discredited. Since that time, however, preventive inoculations against various diseases have been carried out on a small scale in Munich and also in the Grand Duchy of Baden, and in a letter from Professor Kitt, of Munich, Director of the Veterinary School, I was informed that the results obtained have been very favourable. In Austria the treatment is now carried out on a large scale, and the Austrian Government has recently decided to establish in Vienna an institute similar to those in Paris and in Berlin. Even in England inoculations against anthrax are now performed, especially by my friend, Professor Penberthy, of the Royal Veterinary College, although, owing to there being no institute for the purpose at home, he is obliged to send to Paris for the necessary material. This fact is not one of which we can be proud.

I have said that we have now discovered our enemy, but it is only within the last few years that the organisms which cause suppuration, typhoid, diphtheria, tetanus, pneumonia, phthisis, glanders, and other infectious diseases have been discovered and isolated. However, it was not enough to have discovered our foes without knowing their habits, and it is only recently that in the laboratory, pathologists, like Sidney Martin, Woodhead, and others have shown how little bodies of that swarming life—"Where the creature that measures the thousandth part of an inch is a king"—produce the symptoms which so often end in the death of the patient. In the last year Martin isolated the chemical substance to which the paralysis which follows diphtheria is due, and still more recently Behring and Kitasato have proved that by appropriate treatment with chemical re-agents and with the serum of immune animals, this substance can be so neutralized as to become harmless, even when injected in enormous doses.

The same experimenters also demonstrated that it was possible to cure animals of tetanus, even after they had shown the first symptoms of the disease.

These new cures discovered in the laboratory are not yet applicable to man, but is it not something to be able to cure animals of such fatal diseases as tetanus and diphtheria with almost absolute certainty?

The treatment of tetanus is now applied, in France at least, to horses; Behring, in Berlin, was able in that way to cure horses which had shown marked and severe symptoms of the disease, and I myself, in my last visit to Paris, saw this treatment applied to horses at Alfort. I had at that time an opportunity of seeing the results which have been produced in animals suffering from glanders, by using the chemical substance extracted from cultures of the glanders bacillus. The substance seems to have a special physiological effect on animals which are suffering from that disease, and on those animals only, and by using it glanders can be recognised in horses at a time when the general appearance of the animal indicates perfect health, and the symptoms, both local and general, are practically non-existent.

Veterinary surgeons, as I have said before, have a better opportunity than medical men of judging of the value of a new substance, for the life of an animal is of less value than that of a human being, and agriculturists, and especially horse owners, would give anything in order to get rid of this disease. In a stable near Paris, which for years has been devastated by glanders, the company owning the stables caused over 200 horses to be inoculated with mannein; after the inoculation 130 horses showed the typical symptoms produced in animals suffering from glanders. The owners, acting on the advice of the veterinary surgeons, caused these horses to be slaughtered at once. A careful post-mortem was made on each of them, and all, without exception, were proved to be suffering from glanders, although in the majority of them the disease had not been even suspected. Here, again, the commercial instinct of the owner will very soon point out the results which can be obtained by such a system. Since that time the test has been applied successfully to 15,000 horses.

Medicine, however, and preventive medicine especially, is but one of the sciences which have been benefited by bacteriology. I have already referred to what chemists and brewers have learnt from it, and I cannot give a better example of the practical results which have been obtained by this science, than by telling you that some years ago a brewer at Copenhagen presented £40,000 to the distinguished chemist who had learnt to treat the diseases of beer scientifically, as a token of gratitude for the services which he had rendered to the trade.

Agriculture has also learnt much from this science, for the process of nitrification and nitration of the soil (a process which influences the very source of life) has been shown by Percy and Grace Frankland to be due to the action of bacteria. Strange as it may appear, even such a science as mineralogy has been partly revolutionised by the works of Winogradsky and others on sulphur and iron bacteria. I have myself seen large tracts of country in the French Alps in which, through the action of these germs, iron was deposited on the surface of the ground from the iron springs in the neighbourhood.

I think it is needless for me to speak to a medical audience of the

way in which our scientific knowledge of disinfecting agents has been gained, but you will agree with me that to limit the spread of infectious disease it is necessary that we should have full and accurate knowledge of the disinfecting power of every disinfecting agent on every micro-organism. How, for instance, are we to stop the spread of phthisis, if we do not know what strength of disinfecting fluid will kill the specific bacillus? How are we to arrest the spread of typhoid, if we do not know how much sublimate or other disinfecting agent will kill the bacillus of typhoid? It is a far more complicated business than most people think, or have any idea of. Like most people, I have been unfortunate enough to see many of my friends die of phthisis, which, in some cases, was contracted from their nearest and dearest relations, and I have often been struck with the astonishing ignorance, not only of the public but of the medical profession, as to what constitutes a disinfectant. I have seen cases of phthisis in which the patient was never taught once from beginning to end that it was his duty to expectorate into antiseptics only. I have seen other cases in which the antiseptics, such as Sanitas or weak Condy's Fluid, were worse than useless, for they give the friends and the patient an idea of security where none exists. Similarly, in typhoid, I have seen physicians recommend the mixing of the typhoid dejecta, with such powerless agents as Condy's Fluid or utterly inefficient doses of sublimate. In other cases I have seen them mix them with 5 per cent. carbolic acid, forgetting (or not knowing rather) that carbolic acid has been found to be almost powerless on the typhoid bacillus, so that it is used in the laboratory for the isolation of that bacillus from other bacteria which may be present in the fluid. We must remember that every disinfectant will not act in the same dose on every organism—that arsenic is a poision, for instance, and yet some moulds will live in a solution of arsenic; and although this fact at first appears to make the task a more difficult and complicated one, yet it opens up the hope that a substance may be found for each micro-organism, which may prove fatal in small doses to that microbe, without producing any symptoms in the patient.

We must remember that organisms are in some respects extremely delicate creatures, and when we see such strong, hardy, active moulds as the aspergillus unable to live in a silver vessel, although the quantity of silver dissolved in the water is so small that it cannot be detected by any chemical agent, does it not open to us a field for research in looking for the specific drug for each organism? Moreover, micro-organisms act chiefly through their poisons, and such poisons we can already neutralize and render harmless in the solutions in which they are contained. I believe that in the majority of cases the human and animal body can take care of itself as far as the living micro-organism is concerned, and that the great object of future medical therapeutics will be the neutralization and destruction of these poisons in the animal body. That such a neutralization of poisons does take place even in the healthy animal body has been proved by the experiments of Schiff and others; and there can be no doubt that, like all the other functions of the body, this function may perhaps be stimulated

to greater action by appropriate drugs and appropriate general treatment.

To some extent, clinical medicine has already attained this with one disease, in which it has found a means of arresting the growth of the virus, and, at the same time, has discovered a drug which will so alter the nutrition of the patient that the virus is unable to produce its after-effects.

Science does not proceed by leaps and bounds, and in this short sketch of this new branch of knowledge, we see that bacteriology has proved no exception to the rule.

The science of bacteriology has been built up from materials derived from the various branches of science; it was created by physicists, by chemists, and more especially by physicians, biologists and others. But if it first borrowed from these sciences, it has repaid each a hundred-fold; for a knowledge of the science of bacteriology, and of those causative agents which give rise to the most important process of organic chemistry, namely, fermentation, is absolutely necessary now to every scientific chemist; a knowledge of those minute organisms which throughout the world nitrify and nitrate the soil, of those which reduce iron and sulphur, and thus cause the disposition of beds of these minerals in various parts of the world, a knowledge of all these organisms is as necessary to the biologist, and to the mineralogist, as it is to the physician; and, lastly, for medical men a knowledge of the attenuation of pathogenic forms of micro-organisms is daily becoming more and more necessary.

The time has now come when in many cases diagnosis can be made by searching for the cause of evil when the symptoms of the disease do not warrant us in making that diagnosis. I was able in this way only the other day to diagnose a case of typhoid in a child in which it was only suspected. The work of Weichselbaum and others has shown that in pneumonia a correct diagnosis of the disease can be made with absolute certainty by means of bacteriological investigation; and with regard to phthisis, when pathologist at the Victoria Park Hospital, I was often struck by the accuracy with which we could foretell the future course of the disease during the next few weeks, by searching for the number of bacilli contained in the sputum. A knowledge of bacteriology is, or will soon be, essential to the Medical Officer of Health, and a time will come, and that I think soon, when towns and villages will see that it is as necessary to have a bacteriological examination of their water supply as it is to have the present ordinary chemical analysis. Now, in the British Institute of Preventive Medicine we intend, in the first place, to teach. Teaching is of two kinds, namely, 1, teaching what is known; and 2, teaching to investigate. We intend, if possible, in the Institute of Preventive Medicine, to place at the disposal of the practitioners of England, and more especially, of course, at the disposal of the subscribers, what may be called the analytical bacteriological laboratory, that is a place where every subscriber will be able to send the specimens that puzzle him, so that they may be bacteriologically and pathologically examined. I believe that a good deal more could be done by bacteriological examination if there was

a central place as there is now in Paris, where specimens could be sent to be examined. It is a strange thing to me that physicians will not see that in diphtheria, for instance, a sure diagnosis can be made by twenty minutes' microscopical examination, or by making a culture in serum, when the physical examination of the patient as yet reveals nothing with certainty; nevertheless, by simply making a culture, and by examining carefully the membrane or the typical bacilli, we can say with certainty whether the malady is diphtheria or not, instead of hesitating for a long time as to its exact nature. No doubt when the diagnosis is certain we may say that it may simply be a mild case, but, at the same time, the certainty of the diagnosis gives us a clue as to the measures which are to be adopted for prophylaxis. Similarly with typhoid and other diseases. Moreover, we must remember that what has been done is as nothing compared with what yet remains to be done. The action of drugs on disease, the action of other chemical and physical agents remains to be investigated, but, above all, we must continue to search for the causes of those other diseases which may, perhaps, be due not to bacteria but to similar agents. I would refer more especially to affections such as scarlet fever, measles, vaccinia, small-pox and other infectious diseases of man and animals of which we know little or nothing. I have a strong belief that at present we do not find their causes because we use wrong methods, and that if we can only see the cause of one, we should find the causes of all the others. I will not dwell here on the cause of cancer, as it might be supposed that I am bringing personal questions into this matter, but whether I am right or not in believing that I have seen real parasites in cancerous tumours, you will agree with me that even the demonstration of such parasites would be a great advance on the present hopeless chaos in this question.

As for rabies, although I am persuaded of the efficacy of M. Pasteur's treatment, yet I hope I shall never see the day when an institute for that purpose will exist in England. I hope that before long those who have taken an interest in that subject will persuade the public that in a country like England, surrounded as it is by water, the occurrence of an infectious disease, propagated simply by contagion, is a disgrace to civilization; people will see that practical applications of preventive measures applied to dogs will stamp that affection out, and stamp it out for ever. The public mind has, in London at least, been educated to that pitch, and I was glad to see that when the muzzling order was issued the opposition to it was exceedingly small, and those whose vocation it is to protect animals at the expense of man had, at any rate, the grace to remain silent.

But for other diseases, such as quarter-evil, anthrax, and so on, there is a large demand for preventive inoculations, and I was much astonished when, two years ago, I published a small article on that subject in the "Nineteenth Century," at the number of letters which I received from agriculturists asking me how they could produce and obtain these vaccinating substances; alas! the only answer that I could give them was that they must go to Paris for the materials. I will not waste your time by telling what the Institute would be to chemists and others; but

the fact that our treasurer himself is one from whose book most of us learned chemistry, namely, Sir Henry Roscoe, shows the necessity for such an establishment. It is only right that an institute containing members of all branches of science should be founded in order to study biological questions; for the problems involved in the study of this life are so difficult, so complicated, that it is impossible for one man alone to follow out these problems in all their branches, though what one man cannot do several working together can manage. There must be co-operation in science as in everything else, and such co-operation can only be obtained when men are centred together, and have not to run, as at present, from one end of London to another in order to find help. When the Institute Pasteur was opened, M. Pasteur said to me, "I should like my institute to be a kind of convent composed of men whose only idea, whose only aim in life would be the advancement of science. That would be a real nineteenth century monastic order." I believe that such an institute would not clash in any way with the present laboratories in London, and the best proof of this that I can bring forward is that Dr. Woodhead, the Director of the Joint Laboratories on the Embankment, and Dr. Sherrington, of the Brown Institution, have both become subscribers to our Institute.

As I have stated in the beginning of this paper, Sir Joseph Lister has now asked the medical profession to help him, and I will give you one example of what he means by help. When we wanted to register this Institute as a charitable institution, you will remember, perhaps, that the President of the Board of Trade, listening to the unreasonable cries of our opponents, declined at first to grant us our request. He gave way ultimately, simply owing to the fact that the medical profession showed in the most unmistakable manner that their sympathy was with us. The petition which was signed at that time contained the names of all the leaders of science and medicine in Great Britain, and the opposition which had been raised collapsed entirely.

Now, what Sir Joseph Lister and the Council have been asked by rich men when applied to for subscriptions is this—What does the medical profession think of your plan? In answer to this, we can point to several facts, namely, that the Royal College of Physicians and Royal College of Surgeons are represented on our Council; secondly, that resolutions of sympathy have been passed by a large number of the Medical Societies of London and the provinces; thirdly, that the medical profession in London alone have subscribed nearly £2,000.

Now, gentlemen, do not mistake my meaning. I have not come here only to ask you to subscribe to the Institute. But I hope you will let me tell our Council and our friends that we have your sympathy, that you approve of this undertaking, and that, should we be attacked again, we may count on your support.

ALFRED BOOT AND SON, PRINTERS, 24, OLD BAILEY, E.C.—1719.