AN ADDRESS
ON
BACTERIOLOGICAL RESEARCH.

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When I was honoured by receiving an invitation to deliver one of the addresses at the International Congress I had to choose as the subject of my discourse that science with which I have now chiefly to occupy myself, namely, hygiene, or bacteriology, to which I had formerly almost exclusively devoted myself for many years.

I decided upon the latter, as I take it that bacteriology still possesses the most general interest. I will therefore endeavour to sketch out for you with rapid strokes the present position of bacteriological research, at least in some of its more important parts. To those who are familiar with bacteriology I do not pretend to offer anything new. Nevertheless, that I may not come before even them with empty hands I propose to weave into my discourse some facts discovered in the course of inquiries on tuberculosis which I am carrying out and which have not yet been made public.

Bacteriology is, at any rate so far as concerns us as medical practitioners, a very young science. Even as recently as about 15 years ago, scarcely more was known than that in anthrax and relapsing fever peculiar heterogeneous structures were found in the blood, and that in diseases arising from wound infection so-called vibrios were occasionally present. No proof had yet been adduced that these things were the causes of the disorders in question, and with the exception of a few investigators, who were regarded as eccentric, scientists rather looked upon these results as curiosities than as possible clues to the origin of disease. A different opinion could hardly have been held, as it had not in any one case been proved that these objects were independent entities peculiar to these diseases. Bacteria, indistinguishable from the anthrax bacilli, had been found in putrefying fluids, but especially in the blood of suffocated animals. Some investigators would not allow then to be living organisms at all, but maintained that they were crystalloid formations. Bacteria identical with the spirilla of relapsing fever were said to occur in marsh water, and in the pulp of carious teeth, and it was alleged that bacteria similar to the micrococci of diseases depending on wound infection were found in healthy blood and in healthy tissues.

With the experimental and optical resources then available no further progress could be made, and matters would long have remained in that stage if new methods of investigation had not just at that time been introduced, which at once entirely changed the whole state of affairs, and opened the way to further advance into the dark region. With the aid of improved systems of lenses, and their better adaptation to the required purpose, even the smallest bacteria were rendered clearly visible and distinguishable in their morphological relations from other micro-organisms. At the same time, by the use of nutrient media, which could be used either in the fluid or in the solid form, it became possible to separate the several germs, and to obtain pure cultures, in which the peculiar properties of each individual species could be ascertained with complete certainty. What these new aids to research could accomplish was very soon shown. A number of new well-defined species of pathogenic micro-organisms were discovered, and, what was of greater importance, the causal relation between them and the diseases with which they are associated was established. As the disease-causing agents that have been discovered all belong to the group of bacteria, this fact suggests that the true infectious diseases are exclusively caused by definite species of bacteria quite distinct from each other; and we may cherish the hope that at no very distant time the specific generating factors of all communicable diseases will be discovered.

In the meantime, this anticipation has not yet been fulfilled, but the further development of bacteriological research has made very unexpected progress in other directions. Confining myself in the first place to the positive results of bacteriological research, I wish to draw special attention to the following points.

It must now be regarded as completely proved that bacteria, like the higher vegetable organisms, form constant species, though the limits of these are sometimes difficult to define. The opinions which a few years ago were maintained with great pertinacity, and which is even now held by certain investigators, that bacteria are variable in a manner different from all other living organisms, and can at one time assume certain morphological or biological properties, and at another others entirely different therefrom, and that at most only a few species can be admitted; or that bacteria are not independent organisms at all, but rather belong to the developmental cycle of mould fungi, or, as some will have it, of the lower algæ; or that they are offshoots from animal cells, as, for instance, of blood corpuscles: all these views are untenable in the face of the overwhelming number of collected observations which, without exception, go to show that we have here to deal with well-defined species. If we consider the fact that some infectious diseases caused by bacteria—such as leprosy and phthisis—were described in their unmistakable features by the very oldest medical writers, we can certainly conclude therefrom that pathogenic bacteria have the tendency rather to preserve their properties for a long time than to change quickly, as has generally been assumed in view of the variable character of many epidemic diseases. It is true that, within certain limits, deviations from the usual type of the species may occur in bacteria, and especially in pathogenic bacteria; the bacteria, however, do not even in this respect differ in the least from the higher plants in which manifold changes, mostly the result of external influences, are met with, which at most may warrant us in speaking of varieties, but not in denying the species.

Thus it happens that a species of bacteria under unfavourable conditions of nourishment may present stunted forms, and that some of the properties which are most obvious to the eye, or which are interesting to us from our medical point of view though perhaps of little importance to the general life of the plant (as, for example, the formation of colouring matter, the faculty of growing in a living animal body, or of producing certain poisonous materials), may for the time, or, as far as experiments on the point at present tend to show, permanently disappear. In these cases, however, we have always to do merely with fluctuations which take place within certain limits, and which never depart so far from the central point of the type of the species as to make it necessary to admit transformation into a new or other known species—for example, of the anthrax into the hay bacillus.

As, however, on account of the small size of the bacteria, we have not, as in the case of the higher plants, well-marked morphological signs available for purposes of classification, it is all the more necessary for us, in defining the species, not to confine our attention to particular signs, of which it cannot *a priori* be known whether they are among the constant or the variable characteristics of the species under consideration, but conscientiously to collect as many properties—morphological as well as biological—as possible, even if at first sight they appear to be unessential, and to determine the species only after a complete picture so obtained. In this respect it is impossible to go too far, and many misconceptions and contradictions which are to be met with in bacteriology are attributable to the unhappily inadequate manner in which this rule is generally adhered to.

A very characteristic example of the difficulty in the way of determining species is afforded by the typhoid bacillus. If it is found in the mesenteric glands, in the spleen, or the liver of the body of a person who has died of typhoid, then there is never any doubt that it is the genuine typhoid bacillus that one has to do with, since up to the present no other bacteria, which could be confounded with it, have ever been observed in those situations.

But the case is altogether different if it is a question of identifying typhoid bacilli in the contents of the intestine, in soil, in water, or in atmospheric dust. There are found numerous bacilli which closely resemble them, and which only a very skilful bacteriologist could distinguish from the typhoid bacilli; and even such a one could not do so with absolute certainty, as unmistakable and constant distinctive marks are always wanting. The statements often made in recent times to the effect that typhoid bacilli can be identified in soil, conduit water, and alimentary substances, can only be received with legitimate doubt. The same holds good with regard to diphtheria bacilli. On the other hand, a fortunate chance has determined that for some other important pathogenic bacteria, such as the tubercle bacilli and the cholera bacteria,

signs so certain *a priori* are available, that these organisms can be positively identified under all, even the most difficult, circumstances. The great advantages which have accrued from the certain diagnosis of the causes of disease in these cases should be a strong inducement to us, in spite of all earlier futile attempts, ever anew to seek for similar certain signs for the identification of typhoid and diphtheria bacilli, and other important pathogenic bacteria; then, not before, will it be also possible to follow these generators of disease on their hidden and extremely intricate path outside the body, and in that way to lay firm foundations for a rational prophylaxis.

How cautious, however, one should be in judging of the marks which serve to distinguish bacteria from each other, even in the case of well known species, I have discovered in dealing with tubercle bacilli. That species of bacteria is, as is well known, so distinctly characterised by its vegetation in pure cultures, by its pathogenic properties, and by each individual one of these marks, that any confusion of it with other bacteria seems to be entirely excluded. And yet even in this case one should not trust to one of the marks referred to for the determination of the species, but should follow the proven rule of taking into account all the available properties, and only regarding the bacteria under consideration is fully identified if these all agree. When I undertook my first researches on tubercle bacilli I was careful to proceed strictly in obedience to this rule, and in accordance therewith tubercle bacilli of the most diverse origin were tested, not only with regard to their reaction to colouring matters, but also as to the conditions of their growth in pure cultures and as to their pathogenic properties. It was only with reference to the tuberculosis of fowls that this plan could not be carried out, as at that time it was not possible for me to obtain fresh material from which I could breed pure cultures. As, however, all other kinds of tuberculosis had yielded identical bacilli, and the bacilli of fowl tuberculosis entirely agreed therewith in their appearance and their behaviour with regard to aniline colours, I believed myself justified in pronouncing for its identity with the others, in spite of the missing link in my research.

Later I obtained from various quarters pure cultures which were said to cause tubercle bacilli, but which in many respects differed from them; in particular they had, when used by skilful and thoroughly trustworthy investigators in infecting animals experimentally, yielded discordant results, which were looked upon as unexplained contradictions. At first I believed that I had to do with changes such as are not seldom observed in pathogenic bacteria if cultivated in pure cultures outside the body, or for a long time under more or less unfavourable conditions. To solve the riddle, however, an attempt was made to breed ordinary tubercle bacilli in the above-mentioned presumed variety under the most different influences. They were for a period of many months cultivated in a temperature so high that only a scanty growth took place; in another series of experiments the cultures were again exposed to still higher temperatures for so long a time that they were brought as near as possible to death. In the same way I exposed the cultures to the action of chemicals, light, and deprivation of moisture; they were through many generations cultivated together with other bacteria; they were inoculated in continuous series into animals only slightly susceptible; but in spite of all these influences the properties of the micro-organisms showed only slight changes, which fell far short of what occur under similar circumstances in other bacilli. This appears to show that the tubercle bacilli preserve their properties with great constancy—a view which is in accordance with the fact that pure cultures of them, which have now been cultivated by me in a test tube for more than nine years, and have never in that time been inside a living body, are still entirely unchanged but for a slight diminution of virulence. When all attempts to establish the connection had failed, an accident at last furnished the solution. A year ago it happened that I got hold of some living fowls which suffered from tuberculosis, and I embraced the opportunity of doing what it had been impossible for me to do before, and of making cultures directly from the diseased organs of the animals. When the cultures came to maturity, I saw to my astonishment that they presented exactly the appearance, and also the other characteristics, of the enigmatical cultures resembling the genuine tubercle bacilli. It subsequently came to light that these mysterious cultures had come from fowl tuberculosis, but on the hypothesis that all forms of tuberculosis are identical they had been considered genuine tubercle bacilli. I find a confirmation of my observation in the researches which Professor Maffucci has made on fowl tuberculosis, and which have recently been published. I do not hesitate to maintain that the bacilli of fowl tuberculosis are a species by themselves, but closely related to the tubercle bacilli; and closely connected with this is the question, which is of direct importance, practically whether the bacilli of fowl tuberculosis are also pathogenic for human beings. This question cannot, however, be answered till this species of bacillus has been met with in man in the course of experiment, or until its absence has been established in a sufficiently numerous series of cases. For that purpose workers must not confine themselves to research with reagents to colouring matters, but in each particular case must employ the cultivation method.

All recent investigations clearly indicate that in separating the species of bacteria we must proceed with the greatest circumspection, and make the limits of the several species too narrow rather than too wide.

In another important fundamental question, also, the conditions are much clearer and simpler than before—that is to say, with regard to the proof of the causal relation between pathogenic bacteria and the infectious diseases associated therewith.

The idea that micro-organisms must be the cause of infectious diseases was early expressed by several leading spirits, but the general opinion could not bring itself to accept the notion, and showed itself very sceptical with regard to the first discoveries in this domain. All the more was it desirable in the first cases to prove on irrefutable grounds that the micro-organisms found in an infectious disease are actually the cause of that disease. At one time the objection was always brought forward that there was nothing more than an accidental coincidence between the disease and the micro-organisms, that the latter did not play the part of dangerous parasites but of harmless guests, which found in the diseased organs conditions of life which were wanting in healthy bodies. Many, while acknowledging the pathogenic properties of the bacteria, believed it possible that, under the influence of the morbid process, micro-organisms, accidentally or constantly present, which were otherwise harmless, became transformed into pathogenic bacteria. If, however, it can be proved: First, that the parasite is met with in each individual case of the particular disease and under conditions which correspond to the pathological changes and the clinical course of the disease; secondly, that in no other disease is it found as an accidental non-pathogenic guest; and, thirdly, that if completely isolated from the body and cultivated in pure cultures with sufficient frequency, it can reproduce the disease—then it can no longer be considered an accidental accompaniment of the disease, but in that case no other relation between the parasite and the disease can be admitted than that the parasite is the cause of the disease.

This proof has now been furnished in the fullest measure with regard to a number of infectious diseases, such as anthrax, tuberculosis, erysipelas, tetanus, and many diseases of animals—generally all those diseases which are communicable to animals. At the same time it has further been shown that in all the cases in which the constant and exclusive occurrence of bacteria in an infectious disease has been established, the latter never behave as accidental guests, but like the bacteria already certainly known to be pathogenic. We are therefore fully warranted in affirming that if even only the first two requirements of the proof are fulfilled —that is to say, if the constant and exclusive occurrence of the parasite is established—the causal connection between parasite and disease is validly proved. Starting from this basis, we must admit that a series of diseases in which the experimental infection of animals has hitherto failed or been only partially successful are, in spite of this, to be regarded as parasitic. Among these diseases are typhoid fever, diphtheria, leprosy, relapsing fever, and Asiatic cholera. I wish to call special attention to cholera in this connection, inasmuch as the inclusion of it among parasitic diseases was opposed with extraordinary pertinacity. Every imaginable effort was made to rob the cholera bacteria of their specific character, but they have victoriously resisted all attacks, and it can now be regarded as a universally admitted and firmly established fact that they are the cause of cholera.

Apart from these general, but owing to their fundamental significance highly important, questions, bacteriological investigation has gained firm footing in many directions, and has made clear the relations of pathogenic bacteria to infectious diseases. It would, however, lead us too far to go into these points in detail, and it may be sufficient to point out that we are now for the first time

in a position to frame for ourselves correct notions as to how morbid substances behave outside the body in water, soil, and air; notions which differ materially from earlier views founded on uncertain hypotheses; for the first time we can obtain positive information as to how far the generators of disease are to be looked upon as true parasites—that is, as things which exclusively belong to the human or animal organism—or whether we have to deal with parasites which find their conditions of existence also outside the body, and only occasionally act as generators of disease. This point is of great importance for the prophylaxis of some diseases, especially tuberculosis. Further, the manner in which the generators of disease find their way into the body has, in the case of some pathogenic bacteria, been ascertained with sufficient accuracy to allow of more correct ideas being formed as to these processes. Our knowledge as to the behaviour of pathogenic bacteria in the interior of the body also grows ever more complete, and many pathological processes which hitherto seemed enigmatical are being brought nearer to our understanding. This is the case with regard to the combination which occurs so frequently of more than one infectious disease, one of which, therefore, is to be looked upon as primary, and the other as secondary. The latter gives to the original disease an abnormal, particularly severe, character, or follows it as an after-disease. These circumstances have been observed especially in small-pox, scarlet fever, diphtheria, cholera, and even in typhoid fever and tuberculosis.

Moreover, there must here be mentioned the results which the investigation of bacteria has yielded in relation to their metabolic products, as among these are found some which have peculiar poisonous effects which may possibly have some influence on the symptoms of infectious diseases—perhaps, indeed, cause the most important of them. Of quite special interest in this connection are the recently-discovered poisonous albuminous substances, the so-called "toxalbumins," which can be obtained from cultures of the bacteria of anthrax, diphtheria, and tetanus.

Much ardour has also been shown in working out the question which is closely connected with these points, as to the nature of immunity—a question which can only be decided with the help of bacteriology. It is true that this question has not yet been brought to a true conclusion, but it is becoming more and more evident that the view which for some time held the foreground, that we had to deal with purely cellular processes, with a kind of struggle between the invading parasites on the one hand and devouring phagocytes on the other, is steadily losing ground, and that here also it is most probable that chemical processes play the chief part.

Bacteriological research has, in this relatively short time, supplied an abundance of material respecting the biological condition of bacteria, and much of it is also of importance for the medical side of bacteriology. Thus there is the occurrence of persistent conditions, which in many bacteria, for example, in the bacilli of anthrax and tetanus appear in the form of spores, and are distinguished by a resistance to high temperature and to the influence of chemical agents, which, as compared with other living things, is without parallel. Again, the numerous researches as to the effect which heat, cold, drying, chemical substances, light, etc., have on the pathogenic bacteria which have no spores have yielded many results, which may be used for prophylactic purposes.

Among these factors, light appears to me to be one of the most important. As to direct sunlight, it has been well known for some years that it kills bacteria with tolerable quickness. I can affirm this as regards tubercle bacilli, which were killed in from a few minutes to some hours, according to the thickness of the layer in which they were exposed to the sunlight. What seems to me, however, to be particularly noteworthy is that even ordinary daylight, if it last long enough, produces the same effect; cultures of tubercle bacilli die in five to seven days if exposed at the window in compact masses.

With reference to the etiology of infectious diseases, the fact is also of importance that all bacteria can grow only under moist conditions, that is, in presence of water or other suitable fluids, and that they cannot, of their own accord, pass from wet surfaces into the air. In consequence of this, pathogenic bacteria can be taken up by the air only in the form of dust and dust particles, and only those which can retain their vitality for a long time in the dry state can be dispersed by atmospheric currents. They are never, however, able to multiply in the air, as was formerly held to be the case with regard to morbid substances.

In all the provinces which have hitherto been mentioned, bacteriological research has completely fulfilled, indeed, partly exceeded, what it seemed to promise at the time of its first development. In other respects, however, it has not answered the expectations which were formed of it. Thus, in spite of constantly more and more improved methods of staining, and in spite of the employment of better systems of lenses we have not yet succeeded in discovering more as to the inner structure of bacteria than was accomplished by the original methods. Recently, for the first time, the new method of staining appears to give further information as to the structure of bacteria, in so far that it has been possible to distinguish an inner part, which is probably to be regarded as the nucleus, from the outer covering of protoplasm, and to make organs of motion, the flagella, apparently proceeding from the layer of plasma, visible with a distinctness which it has not hitherto been possible to attain.

In several respects, however, and precisely in some in which such a result was least to be expected, bacteriological research has left us completely in the lurch, namely, in the investigation of a number of infectious diseases, which, by reason of their pronounced infectiousness, seemed to present particularly easy points of attack to investigators. This is the case most conspicuously with the entire group of infectious exanthemata, such as measles, scarlet fever, small-pox, and exanthematic typhus. For not one of these have we succeeded in discovering even the least clue as to the nature of the agents which produce it. Even vaccinia, which is always at our disposal, and can so easily be made the object of experiments on animals, has obstinately resisted all endeavours to discover the real agent which produces it. The same thing holds good with regard to rabies.

Again, we know nothing as to the generating factors of influenza, whooping-cough, trachoma, yellow fever, cattle plague, pleuro-pneumonia, and many other undoubtedly infectious diseases. In the case of most of these disorders neither skill nor perseverance in the use of all resources at present at our command has been wanting, and we can only regard the negative results of the efforts of many investigators as indicating that the methods of research which have proved effectual in so many cases are inadequate for the solution of these problems. I am inclined to think that in the case of the diseases referred to we have to deal, not with bacteria, but with organised generators of disease, which belong to quite different groups of micro-organisms. This opinion is all the more warranted by the fact that peculiar parasites, which belong to the lowest order of the animal kingdom—the protozoa—have recently, as is known, been found in the blood of many animals, as well as in the blood of human beings suffering from malaria. At present nothing beyond the simple recognition of these remarkable and highly important parasites has been accomplished, and no further progress will presumably be made until we have succeeded in cultivating these protozoa outside the body, in the same way as we do bacteria—in artificial nutritive media, or under different, as far as possible, natural conditions—and in studying them in their conditions of life, their course of development, etc. Should this problem be solved, as to which there is no room for doubt, then most probably, in the investigation of pathogenic protozoa and related micro-organisms, an impulse will be given to bacteriological research which it is to be hoped will lead to the full clearing up of the mystery which still surrounds the etiology of the diseases in question.

Up till now I have purposely left one question untouched, although it is precisely the one which is most frequently, and not without a certain amount of reproach, addressed to bacteriologists. I mean the question of what profit all the weary labour which has up to the present been expended on the investigation of bacteria has been? Properly speaking, no such question should be asked, as true research follows its own way without being misled by the consideration whether its labours will yield immediate advantage or not. I cannot, however, deem this question utterly unwarranted in the present instance, as very few of those who occupy themselves with bacteriological research leave practical objects entirely out of sight.

The practically available results obtained up to the present by means of bacteriological research are by no means so despicable as some askers of the above question think.

I need only refer to that which has been accomplished in the domain of disinfection. Just at this point there was previously no foothold; men worked completely in the dark, and often enough large sums were spent in useless disinfection, leaving altogether out of account the indirect harm done in other ways by the neglect of

hygienic precautions. Now, on the other hand, we possess certain knowledge by means of which we are able to test the efficacy of disinfectants, and if much still remains to be done in that province, we can at least affirm that the disinfectants now in use, so far as they have been tested, efficiently fulfil their object.

Among the practical results might also be reckoned the application of bacteriological methods to the control of water filtration, as for that purpose nothing else can take the place of these methods. Connected herewith are the discoveries arrived at by means of bacteriological research as to the filtering properties of the soil, and the important consequences resulting therefrom as to the employment of ground water for water supply, and for the proper construction of wells. In the same way as in the case of water bacteriological methods can be applied to the control of milk, especially as far as it is destined for the nourishment of children, as well as to the examination of other alimentary substances and other things in common use which are open to the suspicion of infection. The examination of the air in sewers, and the confirmation which the generally accepted opinion as to the injuriousness of sewer gas have thereby received; the examination of the air in schoolrooms, the recognition of pathogenic bacteria in alimentary substances, in the soil, etc., stand, as it is impossible to deny, in intimate relation with practice. Among the practical results I might further count the possibility which bacteriology has supplied of diagnosing sporadic cases of Asiatic cholera, and the first stages of pulmonary tuberculosis, the former being of importance for the prophylaxis of cholera, the latter for the timely treatment of tuberculosis.

All these things, however, are advantages which can only indirectly be employed in the struggle against bacteria. We have hardly any directly acting, that is, therapeutical, agents to place beside the indirect ones. The only things which can be adduced in this connection are the results which Pasteur and others have obtained by means of protective inoculation in rabies, anthrax quarter-evil, and swine erysipelas; with regard to antirabic inoculation, the only one which is applicable to man, it may be objected that the cause of rabies is not yet known, and probably may not be of a bacterial kind, and that this preventive inoculation should not be placed to the credit of bacteriology. Notwithstanding this, that discovery grew on bacteriological soil, and could not have been made without the antecedent discoveries of protective inoculation against pathogenic bacteria.

Although in these directions, in spite of endless toil, bacteriological research has only such insignificant results to show, I am nevertheless not of opinion that this state of things will always continue. On the contrary, I am convinced that bacteriology will one day be of the greatest importance from the therapeutical point of view also. It is true, I look for relatively smaller therapeutical results in the case of diseases with a short incubation period and a rapid course. In these diseases, as for example in cholera, the chief reliance will always have to be placed on prophylaxis. I am thinking more of diseases of less rapid course, as these offer more points of attack to therapeutic enterprise. And there is scarcely a disease which, partly on this ground, partly on account of its far surpassing all other infectious diseases in importance, so challenges bacteriological investigation as tuberculosis.

Moved by these considerations, very soon after the discovery of the tubercle bacilli, I set about seeking for substances which could be used therapeutically against tuberculosis, and I have pursued this search, which has, of course, been often interrupted by my other occupations, perseveringly up to the present. In the belief that there must be a remedy for tuberculosis, I do not by any means stand alone.

Billroth has, in one of his last writings, expressed himself with all possible distinctness to the same effect, and it is well known that the same object is aimed at by many investigators. It seems to me, however, that the latter have not as a rule followed the right way in their investigations, inasmuch as they have begun their experiment on man. To that I ascribe the fact that everything which people have believed themselves to have discovered in that way—from benzoate of soda down to the hot-air treatment—has proved to be a delusion. Experiments must in the first place be made not on man, but on the parasites themselves in their pure cultures; even if substances have been found which have the power to check the development of tubercle bacilli in the cultures, man should not forthwith be chosen as the subject of experiment. But the question whether observations which have been made in a test tube hold good also in living animal bodies should first be settled in animals. Only if the experiments on animals have proved successful, should the method be tried on man.

Proceeding according to these rules I have in the course of time tested a very large number of substances to see what influence they would exert on the tubercle bacilli cultivated in pure cultures, with the result that not a few substances have the power, even in very small doses, of hindering the growth of tubercle bacilli. More than this, of course, a remedy cannot do. It is not necessary, as has often been erroneously assumed, that the bacteria should be killed in the body; in order to make them harmless to the body it is sufficient to prevent their growth, their multiplication.

I have proved the following substances to be remedies which hinder such growth even in very small doses (to mention only the most important):—A number of ethereal oils; among the aromatic compounds, β naphthylamin, paratoluidin, xylidin; some of the so-called tar dyes, namely, fuchsin, gentian violet, methyl blue, chinolin yellow, aniline yellow, auramin; among the metals, mercury in the form of vapour, silver and gold compounds. The compounds of cyanogen and gold were especially conspicuous, their effect surpassing that of all other substances; even in a dilution of 1 to 2 millions they checked the growth of tubercle bacilli. All these substances, however, remained absolutely without effect if tried on tuberculous animals.

In spite of this failure I have not allowed myself to be discouraged from prosecuting the search for growth-hindering remedies, and I have at last hit upon a substance which has the power of preventing the growth of tubercle bacilli, not only in a test tube, but in the body of an animal. All experiments in tuberculosis are, as everyone who has had experience of them has sufficiently discovered, of very long duration; my researches on this substance, therefore, although they have already occupied me for nearly a year, are not yet completed, and I can only say this much about them, that guinea-pigs, which, as is well-known, are extraordinarily susceptible to tuberculosis, if exposed to the influence of this substance, cease to react to the inoculation of tuberculous virus, and that in guinea-pigs suffering from general tuberculosis even to a high degree, the morbid process can be brought completely to a standstill, without the body being in any way injuriously affected.

From these researches I, in the meantime, do not draw any further conclusions than that the possibility of rendering pathogenic bacteria in the living body harmless without injury to the latter, which has hitherto been justly doubted has been thereby established.

Should, however, the hopes based on these researches be fulfilled in the future, and should we succeed, in the case of one bacterial infectious disease, in making ourselves masters of the microscopic, but hitherto victorious, enemy in the human body, then it will soon also be possible, I have no doubt, to obtain the same result in the case of other diseases. This opens up an oft-promised field of work, with problems which are worthy to be the subject of an international competition of the noblest kind. To give even now some encouragement to further researches in this direction was the sole and only reason why I, departing from my usual custom, have made a communication on a research which is not yet completed.

Allow me, therefore, to conclude this address with the expression of a wish that the nations may measure their strength on this field of labour and in war against the smallest, but the most deadly, foes of the human race, and that in this struggle for the weal of all mankind one nation may always strive to surpass the other in the successes which it achieves.

THE Stephen Cottage Hospital at Dufftown, towards the erection and endowment of which Sir George Stephen, Montreal, and a native of Dufftown, gave £5,000, was formally opened by the Duke of Fife on August 5th.

PRESENTATIONS.—Dr. Hugh R. Ker has been presented by his friends at Halesowen, where he has practised for upwards of 20 years, with a handsome illuminated address and a silver Elgin bowl, "in recognition of important services rendered, especially to the poorer part of the community." Dr. Ker was also the recipient of a silver medal, a badge and ribbon from the "Court Loyal Shenstone Lodge" of the Ancient Order of Foresters.—Dr. Pierce, F.R.C.S., M.R.C.S. Edin., one of the coroners for the County of Derby, and Justice of the Peace, has received a public testimonial in the shape of a handsome illuminated address from the members of the Oddfellows' Club at Llanrhaiadr, enclosed in a heavily gilt frame, at a banquet over which he presided.