itself over the length and breadth of the land, and may produce a huge mortality. It may, of course, be said that scarlet fever is always epidemic somewhere, and that, do what you will, you cannot stamp it out; that children must therefore run their chance. Such an argument is, I hold, altogether fallacious; if it were once admitted, it would prevent us from taking any steps whatever to check the spread of diseases of this kind. I want every individual here present to realise that much more can be done, and ought to be done, in the way of arresting and stamping out these infectious diseases, than we are doing; and that it is the duty of every right-minded man and woman to do what he or she can to help in this matter.

[To be continued.]

FURTHER RESEARCHES ON CHOLERA.

By R. KOCH, M.D.,

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Speech at the commencement of the Second Conference on Cholera, held at Berlin on May 4th, 5th, 6th, 7th, and 8th, 1885.

GENTLEMEN,—As an introduction to our discussions, I propose to give you a short sketch of the investigations of cholera made since our last conference, and to add thereto some facts with regard to investigations which I have carried on, in conjunction with Drs. Weisser and Frank, on the pathogenic properties of the cholera-bacteria, and some questions connected therewith.

As you will, no doubt, remember, the investigations made by the Commission sent to India to investigate cholera, showed that a micro-organism, belonging to the group of bacteria, is present in cholera, chiefly in the intestines of persons who have died of that disease, and in the dejecta of those suffering from it; and that this organism can be distinguished from all other bacteria by its peculiar characteristics, and thus must be regarded as a distinct species. As these bacteria were never found elsewhere than in cholera, we had to do with a micro-organism which was characteristic of cholera, and which stood in the closest relation to it. You remember, further, the conclusions which I have drawn, partly on account of this connection, between the cholera-bacteria and cholera, and partly on account of the correspondence between the behaviour of the cholera-bacteria and epidemiological experience—the conclusion being that the cause of cholera was to be looked for in these bacteria.

It was to be expected that my statements would not at once be accepted. I had never expected that myself; and it seemed to me very desirable that my investigations should be tested, in all directions, with the utmost possible completeness, and with thorough criticism. This has also happened to the most complete extent. A number of investigators, skilled and unskilled, have busied themselves with this question, and have arrived at results, of which I shall give you a short sketch. It would lead me too far to inform you of all which has been written with regard to the cholera-bacteria; and I will, therefore, confine myself to the most important works.

The first who published anything with regard to the relation of the cholera-bacteria to cholera were Finkler and Prior, who, as is undoubtedly known to you all, were of opinion that they had found a micro-organism in cholera nostras which could not be distinguished from the cholera-bacilli. If this statement had been correct, then naturally the importance of the cholera-bacteria would have been lost. I obtained, through the courtesy of Messrs. Finkler and Prior, the opportunity of thoroughly investigating for myself the bacteria found by them, and was able to compare them with the cholera-bacteria found by me. It was very soon evident that very important differences existed between these two forms of bacteria. Instead of long descriptions, I will show you some preparations, by which you will be able to convince yourselves of their different behaviour. I hand you two vessels. In one are pure cultivations of cholera-bacteria, in test-tubes which contain nutritive gelatine, and are closed by cotton-wool plugs. You will easily recognise the characteristic growth of the cholera-bacteria, which form in the nutritive gelatine a thin whitish thread of the length of the puncture made by the platinum wire. The liquefaction of the gelatine which is caused by the growth begins at the upper end of this thread, and spreads very slowly. At the same time, a shrinking or drying occurs at the upper end of the thread; and thus a funnel-shaped depression is formed, which, when

looked at by transmitted light, looks as if it were an air-bubble. The cultivations of Finkler's bacilli, which are present in the second glass, and were prepared in exactly the same way, behave very differently. They liquefy the gelatine much more quickly, and in the whole extent of the needle-track. Hence that drying up or absorption of a part of the thread does not occur, and thus you miss the apparent air-bubble in the upper part of the cultivation. I will, however, remark here that this air-bubble, which is here seen in the cultivations of the cholera-bacilli, and is absent in those of Finkler's bacteria, is not the only point of distinction. It has been said that, under certain circumstances, a similar bubble may be formed in the cultivations of Finkler's organisms. This is correct. If the organisms be grown at the lowest possible temperature—that is, very slowly—a bubble-formation is produced, similar to that of the slower growing cholera-bacilli. These cultivations which you see here, however, have grown under exactly similar conditions—namely, in gelatine of the same concentration, and at the same temperature; and hence, if these bacteria be the same, they must behave similarly in the gelatine-cultivation. But this is not the case. When you compare the cholera-cultivations with Finkler's, you observe that, when the first show scarcely perceptible evidence of liquefaction, and in the lower part are thin and thread-like, Finkler's organisms have already liquefied a great part of the gelatine, and formed a sac-like fluid part with muddy contents. This is, however, as I have said, not the only difference. The products of the decomposition of these two forms of bacteria are also different, as is evident from the fact that the cultivations of Finkler's bacteria have a stinking smell, while those of the cholera-bacilli have a peculiar aromatic odour. Further, they behave differently when they are grown on boiled potatoes. The cholera-bacteria do not develop at all on potatoes at the ordinary temperature of the room, or only so slightly as not to be evident to the naked eye. You see a potato, which was inoculated with cholera-bacteria six days ago, and there is not the slightest growth. On the other hand, on this potato, inoculated at the same time with Finkler's bacteria, there has been formed, as you see, a greyish yellow slimy mass, which consists of an enormous vegetation of Finkler's organisms.

The same results as regards the difference between these two kinds of bacteria have also been obtained by others quite independently of my investigations; for example, by Dr. Van Ermengem, who had likewise received specimens of their bacteria from Finkler and Prior.

I should like to mention only, in addition, that it seems to me questionable whether the bacteria found by Finkler are related to cholera nostras, as Finkler has supposed. They were found in the evacuations of persons who had suffered from diarrhoea, and that not in the fresh evacuations, but after these had stood in a putrefying state for fourteen days. I have seen microscopical specimens of the fresh evacuations, and these did not contain the peculiar forms of Finkler's bacteria, but other forms, which, however, according to Finkler's idea, belong to their cycle of development. In addition, I may point out here that, up till now, a number of undoubted cases of cholera nostras have been investigated by different observers for the presence of comma-shaped bacteria. At our first conference I was able to inform you of some cases in which nothing could be found which was at all like the cholera-bacteria. Since then, I have investigated several cases, in part fatal ones, with a like negative result. But other investigators, also, have examined numerous cases in like manner, for example, Van Ermengem, Watson Cheyne, Biedert, etc.; but no one has found in cholera nostras bacteria which were the same as the cholera-bacteria or Finkler's organisms.

The second noteworthy research on cholera-bacteria which I must mention is that made by Dr. Klein. As is well known, Klein was sent to India by the English Government to carry out investigations into the etiology of cholera. From the reports by Klein which have as yet been published, we must conclude that he has exclusively busied himself in upsetting my statements. At least, I have as yet found nothing in Klein's publications but what stands in direct contradiction to my results. Klein has brought back nothing positive or new from his expedition. Any other result could scarcely be expected; for already, before he went to India, his judgment of my statements was decided. He attempted, at that time, to show that I had contradicted myself; that I had, in Egypt, compared the bacteria found in the wall of the small intestine with the bacilli of glanders, but that the latter were not curved, but straight bacilli; then all at once, in India, the straight bacilli had become curved ones. This objection has later, also, been brought forward by others; but—in order at once to settle this point—he who asserts this has evidently never seen side by side sections containing cholera-bacilli and glanders-bacilli—I have taken the liberty of placing here such preparations, and you will be able to convince yourselves that it is very difficult to distinguish these

two kinds of bacteria from one another in these sections. The glanders-bacilli are, no doubt, generally straight, but they are by no means rigid bodies, but, on the contrary, are soft and yielding, and very often assume more or less curved forms in the tissue when lying between cells closely packed together, to which they must accommodate themselves. On the other hand, the curved form of the cholera-bacilli is not so well marked in sections. One can, therefore, very well compare the two forms of bacteria with one another, as their size is very similar; and even now I would not hesitate a moment, if I wished to convey quickly a general idea of these bacteria to any one who had not yet seen the cholera-bacilli, but who knew the bacilli of glanders, to select, as a comparative object, sections containing glanders-bacilli; and it was only of the appearance in sections which I spoke when I made my returns from Egypt. With what astounding ignorance of bacteriology this matter has been treated, will be seen from this drawing, which was published by Lankester, in *Nature*, of December 25th, 1884. In this question, Lankester takes up the same standpoint as Klein, and says that it is a horrible error on my part to compare glanders-bacilli with comma-bacilli. In order to illustrate this, he figures here a glanders-bacillus, and side by side a hay-bacillus and a tubercle-bacillus. As you see, these different kinds of bacilli are shown as of about the same size. In fact, the glanders-bacillus seems to be longer and thicker than the hay-bacillus. The differences in size are, however, in reality very considerable, the glanders-bacilli and the bacilli of tubercle being both very much smaller than the hay-bacillus. Lankester has evidently never in his life seen a glanders-bacillus; nevertheless, he considers himself justified in giving his judgment as to the appearance of the glanders-bacilli and their resemblance to cholera-bacilli, which he had certainly at that time never seen in sections.

A further objection of Klein's, which, however, is of very small consequence, but which characterises the style of his controversy, is that the cholera-bacteria are not bacilli, but spirilla. I had stated in my communications that, on account of their curved form, they might take an intermediate place between bacilli and spirilla; as I am of opinion that the differences between bacilli and spirilla are not sufficiently made out, and I therefore consider it precipitate to make at this time a strict separation between them. However, it is all the same to me whether the cholera-bacteria are called bacilli or spirilla, so long as one pays attention to and lays stress on their other characteristics; the name is, in this case, of the least importance. I can, however, here show you that a capable botanist, namely, De Bary, still calls curved rod-shaped bacteria bacilli. You will find here, in his latest work on the morphology of fungi, a picture of a form of bacteria called by him bacillus megatherium. These bacilli are distinctly curved, and look like large comma-bacilli. Thus I do not think that I have made any mistake in designating the cholera-bacteria for the present as bacilli.

Further, Klein states that he has found the cholera-bacteria, not only in the deposits on the teeth of healthy men, but also in other diseases, more especially in persons who had suffered from diarrhoea; for example, in phthisical and dysenteric cases. He further asserts that they are present in cholera in only quite small numbers, and that the earlier the *post mortem* examination is made, in so much the smaller quantity are they found, large numbers being only present in bodies where the necropsy has been delayed. In this point, Klein stands in opposition, not only to me, but to all other investigators, who have found the comma-bacilli in numbers a very short time after death.

He further ascribes to me the assertion that the comma-bacilli are killed by weak acids—a statement which is quite erroneous. I have not, in my former communications, spoken of the death of the comma-bacilli, but, as you will remember, only of the hindrance to their development caused by different substances, and, among others, of the absence of growth in gelatine of an acid reaction.

In India, Klein states that he found comma-bacilli in the same tank in which we found them, but at a time when those who lived in the neighbourhood of the tank were free from cholera. No one knows what Klein found; whether they were the true cholera-bacteria, or, more probably, the same bacteria which he found in the evacuations of phthisical patients, or in the saliva of healthy persons, and which he held to be cholera-bacteria. Klein's report has also in England been subjected to a very thorough and able criticism by Dr. Watson Cheyne. Klein was compelled, in consequence of the unanswerable objections made by Dr. Watson Cheyne, to withdraw most of his assertions, or almost all which are of importance, and thus to record, in a drastic manner, the untrustworthiness of his former statements; more especially, he had to admit that the cholera-bacilli differed from those occurring in phthisis, in dysentery, and in the mouth; and he has

further admitted that he has found true cholera-bacilli in all cases of cholera. Thus he finally comes, under compulsion however, exactly to the same result as I did—namely, that the cholera-bacteria are a specific variety, and seen exclusively in cholera. Klein will not be able to escape from all the conclusions which follow from these facts, unless he again involve himself in contradictions.

I come, now, to the investigations of Emmerich. Emmerich has found in Naples a special form of bacteria in the blood, and partly also in the internal organs, of nine cholera-cadavers, and of one patient suffering from cholera. He has also seen the true cholera-bacilli, but, as he says, not in all cases; and he, therefore, looks on the bacteria found by him as the true cholera-bacteria. Emmerich's investigations, and more especially the methods which he employed, have been subjected by Flügge to a criticism which I completely agree with; I consider it able, and altogether just. I should like to point out quite shortly that Emmerich's assertion, as to the constant presence of bacteria in the blood of cholera-patients, is directly opposed to the results of all other investigators, who have investigated cholera-blood and cholera-organs. The most important objection which must be made to his results is, that he has employed faulty methods. He introduced a certain quantity of blood, and of material from the organs, into test-tubes containing nutritive gelatine; and then took these tubes to Munich, and there, for the first time, made his pure cultivations. This reminds me of the method employed formerly by Hallier in his investigations on cholera. In the year 1866, if I am not mistaken, a flask containing cholera-dejecta was sent to him; he corked it, and let it stand till next spring, when he investigated it with all possible precautions. Emmerich's error is not so great, but still it is essentially the same. I have lately, by chance, had the opportunity of becoming acquainted with the investigations made by Professor Ceci during the cholera-epidemic in Genoa. He had followed exactly the same method as Emmerich; he had taken blood from cholera-cadavers, and also small portions from the spleen, kidneys, and other organs, and introduced them into the gelatine with a previously heated platinum wire. Ceci had, however, only employed perfectly fresh bodies, and had worked with the greatest care. When he took portions from the liver, for example, he made a cut through the organ at right angles to the surface; then with a fresh knife (likewise previously heated) he made another section at right angles to the surface of the former, and then from this second surface took portions for introduction into the gelatine. In this way, as he assured me, he had made more than 100 experiments. When he was here, he was able to show me about fifty such test-tubes, which had been covered with paraffin to prevent evaporation. In not one of them had growth occurred from the blood or from the pieces of the organs. This result thus is also directly opposed to that obtained by Emmerich. Emmerich has further stated that he has been able to produce, with his bacteria, effects on animals which completely corresponded to Asiatic cholera; he injected his bacteria subcutaneously in large quantities; his animals died after some time, and the intestine showed exactly the appearances of a cholera-intestine. If this is the case, as I do not doubt that it is, this would not in the least show (apart altogether from all other objections, and in spite of the faulty methods by which his bacteria were obtained) that these bacteria had anything to do with cholera. As has been long known, one can, with various bacteria, produce choleraic symptoms in animals. I have already, some years ago, experimented with bacteria, by means of which a form of septicæmia was produced in rabbits. I had to inject a considerable quantity subcutaneously, in order to kill the animals, and then I found peculiar appearances in the intestine which completely corresponded to those described by Emmerich.

The symptoms described by Emmerich can be produced in guinea-pigs, in a very characteristic manner, by a pathogenic form of bacteria isolated by Professor Brieger from human fæces. I will to-morrow inject some animals subcutaneously with these bacteria, in order that you may convince yourselves of their peculiar effect.

These are the most important objections which have been made against the comma-bacilli. I believe that I have shown you that they are unfounded. On the other hand, my statements have received much confirmation.

Very many have busied themselves in investigating saliva and tartar, the intestinal evacuations of the healthy and sick, putrid fluids and other mixtures of bacteria; but all trustworthy observers are unanimous in stating, that they have not been able to find any organism which was identical with the cholera bacteria. In the Sanitary Institute, more than 150 medical men have been instructed in the methods of demonstrating cholera-bacteria during the cholera-courses, and many hundred examinations of saliva, fæces, etc., were made

but, with the exception of a case to be mentioned later, we have never met with the characteristic cholera-bacteria. At the same time, all who were sufficiently acquainted with bacterial work, have convinced themselves that the cholera-bacteria can be easily distinguished from all other bacteria, and that their characteristic properties justify us in regarding them as a specific independent variety.

Further, the spread of cholera during the last year in Europe, has afforded opportunities to a number of physicians to test my statements by the investigation of cholera-patients and cholera-cadavers, and to confirm them almost without exception.

The first thorough investigations were made by Nicati and Rietsch, to whom I showed the methods of investigation during my stay in Marseilles. They have examined a large number of cases (if I am not mistaken, more than thirty) for the presence of comma-bacilli, and have found them in all without exception. Almost at the same time, Van Ermengem studied the epidemic in Marseilles, and likewise found the comma-bacilli in all cases. Baber, Watson Cheyne, and Pfeiffer give the same report from their observations in Paris.

In Italy, the comma-bacilli have been found by Ceci, Escherich, Armanni, and Fede. Lately, Schottelius has published some facts with regard to positive results in Turin. Ceci's communications as to his investigations, made in the first instance in conjunction with Klebs, are in so far of interest, as both these investigators at first reported, that they had not found the comma-bacilli in all cases, and that they had also been found in other diseases, for example, by Klebs in a case of pneumonia. Later, however, Ceci, who had worked with insufficient objectives, procured a Zeiss's microscope with an oil lens, and with this subsequently demonstrated the presence of the comma-bacilli in his preparations and cultivations, so that he is now, as he himself told me, convinced of their constant presence in cholera. I received from Klebs a preparation from the above-mentioned case of pneumonia, as well as a portion of the evacuations in question, but I have not succeeded in finding any cholera-bacteria in them. I do not believe that Klebs still insists on identifying the bacteria in question with cholera-bacteria, on account of their very doubtful morphological resemblance alone, as all cultivation-experiments yielded negative results.

I can also add here some facts with regard to further investigations which I have myself made. When I left Calcutta, I gave the remainder of my cover-glasses to Dr. Dissent, physician at the Sealdah Hospital, with the request that he would make preparations of the intestinal contents from cases of cholera. Dr. Dissent, whom I have to thank for much help in procuring material for investigation during my stay in Calcutta, and for very valuable information with regard to cholera in Bengal, has also carried out this request with the greatest willingness, and has, in the course of half a year, sent me cover-glasses from more than eighty cholera-cases, from every case about five cover-glasses. Of these, only seventy-nine could be used, as some cover-glasses were spoilt during the transport. Of these seventy-nine cases, which were examined here by Drs. Weisser and Frank, seventy-six came from *post mortem* examinations, and three from dejecta of patients. Comma-bacilli were absent in only five of these cases, and in these five the preparations consisted of numerous red blood-corpuscles, whence it may be concluded that they came from cases at a late stage of the disease. In thirty-seven cases, the bacilli were present in moderate numbers, but quite sufficient to enable one to demonstrate them with certainty. Of these, thirty contained blood; in three it was doubtful whether blood was present or not, and in four there was no blood. Twenty-seven of the preparations contained very many bacilli, and in ten cases there was almost a pure cultivation. The latter again confirmed my former statements, that the more rapid the case the more numerous and the purer were the comma-bacilli.

I have, further, something to show you which has a certain amount of interest: it is a number of cholera-cultivations from different sources. I received, from almost all those who have found the cholera-bacteria during the recent epidemic, specimens of their cultivations, and it seemed to me of value to test these to see if they corresponded in all their properties. I have for this purpose collected them, and you see here cultivations from Marseilles, three different ones from Paris, and two from Italy. Along with them, you see one brought by me from Toulon. No difference can be observed in these cultivations, and they correspond exactly when investigated with high powers, and their pathogenic properties are identical.

All these facts, then, confirm fully the statement made by me at our former conference, that the cholera bacteria occur only in cholera. Nothing of importance has been added by these recent investigations to what I have formerly communicated to you.

[To be continued.]

ON A NEW PROCEDURE FOR THE REMOVAL OF SMALL CALCULI FROM THE BLADDER, IN MALE CHILDREN.

By THOMAS ANNANDALE, F.R.S.E.,

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ALTHOUGH lateral lithotomy is a most successful operation in male children, it must, I think, be acknowledged that in the case of small calculi, this operation is a severe one, considering the small size of the irritating body to be removed. Erichsen (*Science and Art of Surgery*, vol. i, page 51, 8th edition) remarks, "Very many boys are cut for stone every year, and recover; but I scarcely recollect to have met with a middle-aged adult who had been operated on in childhood."

It is also well known to surgeons that lateral lithotomy in children has some special risks connected with the operation itself, and depending upon the tender nature of the urethral structures and position of the bladder. It is quite possible to seize and crush a stone in the young male bladder by means of a small lithotrite, but it is not so certain to insure the complete removal of the fragments after the proceeding as evacuating catheters are still made too large to pass along the young male urethra. I believe that this difficulty may be overcome, and that in certain cases lithotripsy may be thoroughly and successfully carried out even in children.

Sir H. Thompson (*Practical Lithotomy and Lithotripsy*, page 229), in advocating lateral lithotomy as the rule in young males, remarks, "The exceptional cases are those in which the stone is only too large to pass by the urethra, and therefore small. For these, there is no occasion to perform lithotomy. Opposed as I am to lithotripsy in children as a rule, for reasons already named, I nevertheless believe that, when the stone is so small as to be easily pulverised at a single crushing by a slender lithotrite, it is the simplest and best method of proceeding, and that when the stone can be well and easily crushed in two sittings it may be admissible." Unless the fragments of the crushed stone can be completely evacuated at the time of the operation, I am of opinion that lithotomy is still preferable to lithotripsy in children, even in cases where the stone is small.

In the meantime it has been my wish to discover some method which would be more simple, and cause less injury to the urethral and vesical structures in the case of male children, than the ordinary operation of lateral lithotomy, more particularly when the stone to be removed is limited in size.

In the following case, I practised what I believe to be a new procedure, and have hopes that it may prove to be an useful addition to our means of treatment in connection with this department of surgery.

CASE.—A boy, aged 4½, was sent to me by my friend Dr. Hunter, of Linlithgow, on account of symptoms of stone in the bladder which had existed for about a year. The usual symptoms were present and well marked, and, upon sounding him, I detected a small and light stone.

On December 10th, I put him under the influence of chloroform, and dilated his urethra by passing Nos. 6, 7, 8 and 9 silver catheters in succession. The first three passed readily, but No. 9 was slightly grasped in its passage along the urethra. Before removing this last catheter, four ounces of antiseptic fluid (corrosive sublimate 1 to 4000) were injected through it into the bladder. This catheter being withdrawn, a small lithotrite, having a diameter about equal to a No. 8 bougie, was introduced along the urethra into the bladder. After a little careful manipulation, the stone was seized, and fixed between the blades of the instrument. It was then found that, by depressing the handle of the lithotrite, its vesical extremity, together with the stone, could be readily felt through the abdominal wall immediately above the pubes. The lithotrite being held in this position, a small incision, an inch in length, was made in the middle line of the abdominal wall over the pubes, and for a short distance above it. The various tissues were divided, until the wall of the bladder was exposed at the point against which the blades of the lithotrite and the enclosed stone were pressing. A little further depression of the handle of the lithotrite caused the extremity of its blades covered by the stretched wall of the bladder to protrude through the wound in the abdominal wall; and a small incision having been made through the wall of the bladder by cutting upon the extremity of the lithotrite, the blades of the lithotrite, together with the stone, were pushed through the wound. The stone was here extracted from between the blades of the lithotrite; and the open extremity of a No. 7 India-rubber catheter was seized, and drawn into the bladder and along the urethra as the lithotrite was removed, thus leaving a drain for the urine to escape from the bladder.