AN ADDRESS
ON
CHOLERA AND ITS BACILLUS.

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(Continued from page 403.)

If one consider all the qualities of the comma-bacilli that I have hitherto described, one must be convinced that they belong to a special well characterised species of bacteria; and that, by means of their characteristic qualities, they can easily be recognised and distinguished from other bacteria.

After obtaining this conviction, it was, above all, important to establish the relations between the comma-bacilli and the real process of cholera; and, first, I had to investigate whether they are to be found in all cases of cholera, and whether they are absent in all other cases; that is, whether they belong exclusively to cholera. In this direction, as large a series of cases as possible was thoroughly investigated. In Egypt, ten *post mortem* examinations could be turned to account. It is true these were only microscopically examined; for I was not then sufficiently acquainted with the qualities of the comma-bacilli, which they show while growing in food-gelatine, to be able to make use of the gelatine-process for proving the presence of the bacilli. But I was convinced by careful microscopic investigation of the presence of the comma-bacilli in all these cases. In India, forty-two *post mortem* examinations were made both microscopically and by cultivations in food-gelatine, and in no case were the bacilli absent. In a series of cases, which had been very acute, an almost pure cultivation of comma-bacilli was met with in the intestinal canal. Further, in India, the dejecta of thirty-two cholera-patients were similarly examined, and each time comma-bacilli were present in them. The liquid vomit of cholera-patients was also often examined; but comma-bacilli were only found twice in the vomit, and in these cases the quality of the vomit enabled us to conclude that it was not properly the contents of the stomach, but contents of the intestine, which had been driven upwards by abdominal pressure, and evacuated. The liquid had an alkaline reaction, and looked exactly like the contents of the intestine. I have also found comma-bacilli in eight other *post mortem* preparations, some of which I had previously had sent me from India, and others I had received from Alexandria, from Dr. Kartulis and Dr. Schiess Bey. Finally, I recently made two *post mortem* examinations at Toulon, together with Dr. Strauss and Dr. Roux; and in these cases also, as well as in the dejecta of two patients, comma-bacilli were found. In these two *post mortem* examinations at Toulon, we had to do with exceptionally characteristic and acute cases. One of the men, a sailor, was to have been dismissed from the hospital on the same day, as convalescent from malaria; but this could not be done, for at about eleven o'clock in the morning he had an attack of cholera. He died in the afternoon at three, and the corpse could be dissected by half-past three. I will here observe that, in almost all the cases examined by me, the *post mortem* examinations have been made a very short time after death. We have often made the dissections immediately after death; in most cases, two or three hours, at latest, after death; so that *post mortem* putrefaction could not yet have had the effect of changing the condition of the intestine or of its contents. In the case mentioned, as in those of a number of earlier *post mortem* examinations, we could also convince ourselves of the presence in the intestine, in very acute cases, of almost a pure cultivation of comma-bacilli. I was able to demonstrate this fact to Dr. Strauss and Dr. Roux, who had not as yet succeeded in proving the existence of comma-bacilli either microscopically or on firm nutritive soil. These gentlemen had always been of opinion, Dr. Strauss told me, that a special trick in the preparation was necessary in order to colour and cultivate the comma-bacilli. Then, however, they were convinced that nothing is more simple than this, if only a pure and uncomplicated case be chosen for investigation.

In the second *post mortem* examination also in which I took part at Toulon, the comma-bacilli were found in the intestine in almost a pure cultivation. I then asked Dr. Strauss to take this opportunity of showing me the micro-organisms which, according to his view, are to be found in cholera-blood. But these appearances were not to be found in either case.

If we add all these cases together, nearly one hundred have been examined with regard to the presence of comma-bacilli, and the bacilli have been found in all of them. But the investigation has not only proved the existence of the comma-bacilli, but, as I have repeatedly hinted, they always stand in exact proportion to the cholera-process itself; for, where the real cholera-process proper caused the greatest modifications in the intestine, namely, in the lower section of the small intestine, they were to be found in greatest numbers; from these upwards they diminished more and more. In the most uncomplicated cases, they appeared almost like pure cultivations. The older the cases, and the more secondary modifications have taken place in the intestine, the more do they recede into the background.

In accordance with the cholera-material that I have so far examined, I think I can now assert that comma-bacilli are never found absent in cases of cholera; they are something that is specific to cholera.

As a test, a considerable number of other corpses, dejecta from patients and persons in good health, and other substances containing bacteria, were examined to see if these bacilli, which were never missing in cases of cholera, might, perhaps, occur elsewhere also. This is a point of the greatest importance in judging the causal connection between comma-bacilli and cholera.

Amongst these objects for investigation was the corpse of a man who had had cholera six weeks before, and had afterwards died of anæmia. There was no farther trace of comma-bacilli to be found in his intestines. The dejecta of a man, who had had an attack of cholera for eight days previously, were also examined; his stools were already beginning to be consistent; in this case also comma-bacilli were absent.

I have also thoroughly examined more than thirty corpses, in order to convince myself more and more that these bacilli are really only found in cases of cholera. Corpses of those who had died of affections of the intestines, *e.g.*, of dysentery or of those catarrhs of the intestine frequently mortal in the tropics, were chiefly selected for this purpose; also cases with ulceration in the intestine, a case of enteric fever, and several cases of bilious typhoid.

In the last named disease, the modifications in the intestines are at first sight very similar to those which take place in severe cases of cholera, in which hæmorrhage of the intestine takes place. The small intestine is in the lower section infiltrated by hæmorrhage; but, strange to say, this change affects rather the Peyer's patches in bilious typhoid, whilst in cholera they are very little changed.

In all these cases where we had to deal chiefly with diseases of the intestine, no trace of comma-bacilli was to be found. Experience teaches that such affections of the intestine make people especially liable to cholera. So one might have presupposed that comma-bacilli, if they were to be found anywhere else, must be found in these cases. Besides these, dejecta of a large number of dysenteric patients were examined without the comma-bacilli ever being met with. I continued these investigations afterwards in Berlin, together with Dr. Stahl, my untiring fellow-labourer, a man who promised much for the investigation of bacteria, had not an early death unhappily put an end to his work. We examined a considerable number of various dejecta, especially of children's diarrhœa, as well as that of grown-up persons; saliva also, and the mucus that adheres to the teeth and tongue, and which abounds in bacteria, for the purpose of finding comma-bacilli, but always without success. Various animals were also examined with this view. Because a complication of symptoms very similar to those of cholera can be obtained by arsenical poisoning, animals were poisoned with arsenic, and afterwards examined. A great number of bacteria were found in the intestines, but no comma-bacilli. Nor were they found in the sewage from the drains of the town of Calcutta, in the extremely polluted water of the river Hooghl in a number of tanks which lie in the villages and between the huts of the natives, and contain very dirty water. Everywhere, where I was able to come across a liquid containing bacteria, I examined it in search of comma-bacilli, but never found them in it. Only once did I come across a kind of bacterium which, at first sight, bore a strong resemblance to comma-bacilli, and that was in the water which, at high-water, floods the land of the salt water lake that lies to the east of Calcutta; but, on a closer inspection, they appeared larger and thicker than comma-bacilli, and their cultivations did not liquefy gelatine.

Besides these observations, I have had a considerable experience in bacteria, but I cannot remember ever having seen bacteria resembling the comma-bacilli. I have spoken to several people who have made a great number of cultivations of bacteria, and have also had experience,

but all have told me that they have not yet seen such a kind of bacteria, I, therefore, think I may say positively that the comma-bacilli are constant concomitants of the cholera-process, and that they are never found elsewhere.

The question to be answered now is, how we are to represent the relation between the comma-bacillus and the cholera-process. In answering this question, three different assumptions may be made. It can be said, first, that the cholera-process favours the growth of comma-bacilli by preparing the nutritive soil for them, and that, consequently, so striking an increase of precisely this kind of bacteria takes place. If this assertion be made, then one must start with the presupposition that everybody already has comma-bacilli in his body at the time when he is attacked by cholera; for they were found in the most various places in India, in Egypt, in France, and in people of most various origin and nationality. On this assumption, this kind of bacteria must be one of the most widely spread and most usual kind. But the contrary is the fact; for they are not found, as we have seen, either in those persons who are suffering from other diseases, or in persons in good health, or, except in man, in places favourable for the development of bacteria; they are always found only where cholera is prevailing. This assumption cannot, therefore, be regarded as admissible, and must, therefore, be dropped.

Secondly, one might try to explain the regular concurrence of comma-bacilli and the cholera-process in this way: that conditions are created by the disease, by means of which, amongst the many bacteria that are to be found in the intestine, one kind or another is changed, and assumes the qualities that we have observed in the comma-bacillus. But, in regard to this explanation, I must confess that it has no actual foundation whatever, and is a pure hypothesis. So far, we know of no transformation of one kind of bacteria into another. The sole instances of transformation in the qualities of bacteria rest on their physiological and pathogenic effects, not on their form. Anthrax-bacilli, for instance, when treated in a particular manner, lose their pathogenic effect, but they remain quite unchanged in form. In this instance, we have an example of loss of pathogenic qualities. But this is precisely the opposite of what would take place by the transformation of harmless intestinal bacteria into dangerous cholera-bacilli. Of this latter kind of change from harmless into harmful bacteria, there is no instance that has been ever shown. Some years ago, when the investigation of bacteria was only in its first stage, such an hypothesis might with some degree of justification have been made. But the further the knowledge of bacteria has developed, the more certain has it been shown that, in regard to shape, bacteria are extraordinarily constant. With special reference to comma-bacilli, I will further remark that they retain the qualities above described when raised outside the human body. For instance, they were repeatedly cultivated, from one to another cultivation, in gelatine, to the number of twenty cultivations, and must have returned to the known forms of the common intestinal bacteria, if they were not as constant in their qualities as other bacteria; but this was by no means the case.

There only remains the third assumption—namely, that the cholera-process and the comma-bacilli stand in immediate connection with one another; and, in this respect, I know of no other supposition than that the comma-bacilli are the cause of the cholera-process; that they precede the disease, and that they produce it. The opposite would be, what I have just explained, that the cholera-process produces the comma-bacilli; and this is, as was shown, not possible. As far as I am myself concerned, the matter is clear, that the comma-bacilli are the cause of cholera.

Now it can certainly be demanded that, if this be the case, further proofs should be brought in support of it, and above all, that the cholera-process should be produced experimentally by means of the comma-bacilli. Every imaginable effort has therefore been made to meet this demand. The only possible way of giving such a direct proof of the cholera-producing effect of comma-bacilli is to make experiments on animals, which, if we are to believe the statements of writers on the subject, can be done without any difficulty. It has been said that cases of cholera occurred amongst cows, dogs, poultry, elephants, cats, and several other animals; but on closer examination these statements are found to be quite unreliable. As yet we have no certain instance of animals falling spontaneously ill of cholera in periods of cholera. All experiments, also, which have hitherto been made on animals with cholera-substances, have either given a negative result, or, if they were said to give a positive result, they were not sufficiently supported by evidence, or were disputed by other experimenters. We occupied ourselves, nevertheless, in the most careful and detailed manner, with experiments on animals. Because great value must be laid on the results on white mice obtained by Thiersch, I took fifty mice with me from Berlin, and made all kinds of experiments on them, at first feeding them on the dejecta of cholera-patients and the contents of the intestine of cholera-corpses. We followed Thiersch's rules as accurately as possible, not only feeding them with fresh material, but also with the same food after the fluids had began to decompose. Although these experiments were constantly repeated with material from fresh cholera-cases, our mice remained healthy. We then made experiments on monkeys, cats, poultry, dogs, and various other animals that we were able to get hold of; but we were never able to arrive at anything in animals similar to the cholera-process. In precisely the same manner we made experiments with the cultivations of comma-bacilli; these were also given as food in all stages of development. When experiments were made by feeding the animals with large quantities of comma-bacilli, on killing them, and examining the contents of their stomachs and intestines with a view to find comma-bacilli, it was seen that the comma-bacilli had already perished in the stomach, and had usually not reached the intestinal canal. Other bacteria are different in this respect, for a beautifully red-coloured micrococcus was found accidentally at Calcutta, which was easily recognised by its striking colour, and was therefore especially suitable for such an experiment. This micrococcus was, at my request, given by Dr. Barclay, of Calcutta, to mice as food, and the contents of the intestines of these animals were placed upon potatoes. The red colonies of the micrococcus again formed, which had thus passed the stomach of the mouse uninjured. On the other hand, comma-bacilli are destroyed in the stomachs of animals. We were forced to conclude from this that the failure of these experiments by feeding the animals was due to this property of the comma-bacilli. The experiment was therefore modified by introducing the substances direct into the intestines of the animals. The belly was opened and the liquid was injected immediately into the small intestine with a Pravaz's syringe. The animals bore this very well, but it did not make them ill. We also tried to bring the cholera-dejecta as high as possible into the intestines of monkeys by means of a long catheter. This succeeded very well, but the animals did not suffer from it. I must also mention that purgatives were previously administered to the animals in order to put the intestine into a state of irritation, and then the infecting substance was given, without obtaining any different result. The only experiment in which the comma-bacilli exhibited a pathogenic effect, which therefore gave me hope at first that we should arrive at some result, was that in which pure cultivations were injected directly into the blood-vessels of rabbits or into the abdominal cavity of mice. Rabbits seemed very ill after the injection, but recovered after a few days. Mice, on the contrary, died from twenty-four to forty-eight hours after the injection, and comma-bacilli were found in their blood.

Of course, they must be administered to animals in large quantities; and it is not the same as in other experiments connected with infection, where the smallest quantities of infectious matter are used, and yet an effect is produced. In order to arrive at certainty whether animals can be infected with cholera, I made inquiries everywhere in India whether similar diseases had ever been remarked in India amongst animals. In Bengal, I was assured such a phenomenon had never occurred. This province is extremely thickly populated, and there are many kinds of animals there which live together with human beings. One would suppose, then, that in this country, where cholera exists in all parts of it continuously, animals must often receive into their digestive canal the infectious matter of cholera, and, indeed, in just as effective a form as human beings, but no case of an animal having an attack of cholera has ever been observed there. Hence I think that all the animals on which we can make experiments, and all those, too, which come into contact with human beings, are not liable to cholera, and that a real cholera-process cannot be artificially produced in them. We must, therefore, dispense with them as a material for affording proofs.

But with this I do not by any means intend to say that no proof at all can be brought of the pathogenic action of comma-bacilli. I have already explained to you that, for my own part, I can form no other idea, even without these experiments on animals, than that a causal connection exists between the comma-bacilli and the cholera-process. Should it prove possible later on to produce anything similar to cholera in animals, that would not, for me, prove anything more than the facts which we now have before us. Besides, we know of other diseases which cannot be transferred to animals, *e.g.*, leprosy; and yet we must admit, from all that we know of leprosy-bacilli, that they are the cause of the disease. For this disease, also, we must dispense with experiments on animals, because as yet no species of animals has been found susceptible to leprosy. It is probably the same with enteric fever; I do not know that anyone has ever succeeded in infecting animals with it. We must be satisfied with the fact, that we verify

the constant presence of a particular kind of bacteria in the disease in question, and the absence of the same bacteria in other diseases. The bacteria in question must always coincide exactly with the infectious principle of this particular disease, and to this point I attach great value; the presence of pathogenic bacteria must be one corresponding to the pathological transformations in the body, and to the course of the disease. On the other hand, we know of diseases of animals, also, which cannot be transferred to human beings; for example, rinderpest and pneumonia of cattle. We meet here with a phenomenon widely spread in nature. Almost all parasites are restricted to only one or very few species of animals, which act as their host. I remind you of tapeworms; many kinds of animals have their own special tapeworm, which can only develop in one species of animal, and in no other.

We must, therefore, dispense with this part of the proof in a large number of infectious diseases, which number also includes the exanthematic diseases; and we can do this the more readily, because we already know a whole series of other diseases, which are caused by pathogenic organisms, in which, however, the conditions in other respects are the same, and of which we know with perfect certainty that the disease is occasioned by the micro-organisms belonging to these diseases, whilst we have never yet seen that the disease produces a specific micro-organism. I think that, after having become acquainted with a whole series of such diseases caused by micro-parasites, we are justified in drawing an analogous conclusion.

But, further, some observations are before us which are as good as experiments on human beings. We can look upon them as complete experiments which have taken place under natural conditions. The most important of these observations is the infection of those persons who are occupied in handling cholera-linen. I have often had an opportunity of examining cholera-linen, and have always found the comma-bacilli in enormous numbers, and generally in a regular pure cultivation, in the mucous substance which is found on the surface of the linen soiled by the dejecta, as you have been able to convince yourselves from one of the microscopic plates I have shown you.

If, therefore, an infection can be brought about by cholera-linen, then, as the comma-bacilli are the only micro-organisms in question, it can only be brought about by them. Whether the transmission was brought about by the laundress bringing her hands soiled with comma-bacilli into contact with her food or directly with her mouth, or by the water that contained bacilli splashing and some drops of it reaching the lips or mouth of the laundress; in any case, the conditions are the same here as in an experiment in which a human being is fed with a small quantity of a pure cultivation of comma-bacilli. It is indeed an experiment which a human being unconsciously performs on himself, and the same demonstrative power lies in it as if it had been intentionally made. This observation has furthermore been frequently made and by very various medical men, so that there can be absolutely no doubt of their trustworthiness. I can, besides, appeal to an observation of my own on this point. I succeeded in finding comma-bacilli with all their characteristic peculiarities in a tank that supplies water for drinking and household purposes for all the people living around, in the immediate neighbourhood of which a number of fatal cases of cholera had taken place. It was later shown that the linen of the first person that had died of cholera in the neighbourhood of this tank had been washed in the tank. That is the only time that I have as yet been able to find the comma-bacilli outside the human body. On the bank of this tank there were 30 or 40 huts, in which from 200 to 300 people lived; and 17 of these had died of cholera. It could not be ascertained exactly how many had been taken ill. Such a tank supplies those who live close to it with water for drinking and household purposes; but at the same time it receives all the refuse from the houses. The Hindoos bathe in it every day, they wash their utensils in it, the human fæces are by preference deposited on its banks, and when a hut is provided with a cesspool it drains into the tank. This was precisely the case with the tank in question. When the comma-bacilli were first found in tolerably large numbers and at different points of the bank, the small epidemic had already reached its maximum. A short time afterwards, when only isolated cases occurred, the comma-bacilli were only to be found at one spot, and in small numbers. When they were first found, they were so abundant that their number could not have depended alone on the dejecta that had flowed into the tank and on the wash-water from cholera-linen; an increase of them must have taken place. On the second investigation, on the other hand, their small number did not correspond to the numerous cases of illness that had preceded. If the latter had supplied the tank-water with bacilli, the bacilli must have been far more numerous this time, in comparison with the first time they were discovered. Hence it cannot be said in this case that the presence of the comma-bacilli in the tank was only a consequence of the cholera-epidemic. The relation was such that the epidemic must have been a consequence of the bacilli. We must lay the greater value on observations of this kind, especially on the infection through cholera-linen, because we shall perhaps never be in a position to make direct successful infection-experiments with comma-bacilli.

The fact that the whole etiology of cholera, so far as it is known to us, agrees completely with the peculiarities of comma-bacilli is, I consider, an essential support of my theory, that the comma-bacilli are the cause of cholera.

We have seen that comma-bacilli grow exceedingly rapidly, that their vegetation quickly reaches its maximum, then ceases, and that the bacilli are finally driven away by other bacteria. This corresponds exactly to what proceeds in the intestine.

It can be assumed that, just as in the case of other bacteria, very few individuals are sufficient—under certain circumstances, one single one—to cause infection. Accordingly, we can very well imagine that individual comma-bacilli reach the intestinal canal accidentally, and very speedily multiply there. As soon as they have multiplied to a certain degree, they occasion a state of irritation of the mucous membrane of the intestine and diarrhœa; but when the multiplication continues in increasing progression, and reaches the maximum point, then they culminate into the peculiar complex of symptoms that we characterise as the real attack of cholera.

We have already seen that comma-bacilli most probably, under certain conditions, cannot pass the stomach, at least in animals. This also agrees with all experience of cholera, for predisposition seems to play an important part in cholera-infection. It can be assumed that, of a number of people exposed to cholera-infection, only a fraction of them fall ill, and these are almost always those already suffering from some kind of digestive disturbance, *e.g.*, catarrh of the stomach or intestines, or those who have overloaded the stomach with indigestible food. Especially in the latter case, more or less undigested masses of food may pass into the intestinal canal, and possibly bring with them the comma-bacilli not yet killed in the stomach. You have certainly often observed that the greater number of cases of cholera occur on Mondays and Tuesdays, that is, on the days which have generally been preceded by excesses in eating and drinking.

It is certainly a strange phenomenon, that comma-bacilli confine themselves to the intestines. They do not pass into the blood, nor even into the mesenteric glands. How is it now that this bacteria-vegetation in the intestine can kill a man? In order to explain this, I call your attention to the fact that bacteria, when they grow, not only consume substances, but also produce substances of very various kinds. We know a great many of these kinds of productions of the vital changes of bacteria, which are of a very peculiar nature. Many of them are of a transient nature, and emit an intense smell, others produce colouring matter, others poisonous substances. In the putrefaction of albuminous liquids, *e.g.*, blood, poisons are formed, which must be products of vital changes of these bacteria, as putrefaction is only a consequence of bacteria-vegetation. Many phenomena go to show that these poisons are only produced by special kinds of bacteria, for we see that putrefied fluids can at one time be injected into an animal without producing any effect, whereas they prove poisonous at another time. In this light, I picture to myself the effect of comma-bacilli in the intestine, which depends upon the products of vital changes. In favour of this view, I possess special points of support. It so happened, that in one cultivation-experiment, the nutritive gelatine contained at the same time blood-corpuscles in tolerably large numbers and comma-bacilli. After this gelatine had been poured upon a plate, a number of colonies of comma-bacilli grew. The plate looked as if a reddish dust were suspended in it, as, when the light fell through it, one had a clear impression of the single blood-corpuscles. In this reddish finely granular layer, the colonies of comma-bacilli looked to the naked eye like small colourless holes. When they were examined under the microscope, the striking phenomenon was discovered that the colonies of comma-bacilli had destroyed all the blood-corpuscles within a pretty wide circle, and also to some distance beyond the limit within which they had liquefied the gelatine. From this it is seen, that comma-bacilli can exercise a destructive influence on the formed elements of the blood, and very probably also on other cells.

Mr. Richards, a medical man at Goalundo, in India, has also made an observation which supports the view of the presence of a poisonous substance in the contents of the cholera-intestine. Mr. Richards fed some dogs with large quantities of cholera-dejecta, without producing any effect on the dogs. Then he made the same experiment with pigs, which, according to his statement, died in cramps a very short time

from a quarter of an hour to two hours and a half, after being fed. This was clearly a case of poisoning, and not, as Mr. Richards supposes, of artificial cholera-infection. That this was really so, is especially seen from one of the experiments, in which the contents of the intestine of a pig, killed by being fed on cholera-dejecta, which, according to Mr. Richards's opinion, had the cholera, were given to another pig. This second pig did not suffer from it, so that a reproduction of the supposed infectious matter in the intestine of the pig fed first could not have taken place. If genuine cholera could be produced amongst pigs, it would be then possible to infect a second pig with the contents of the intestine of the first, and a third with those from the second, and so on. Although these experiments do not prove what Mr. Richards intended to prove by them, they are in so far very interesting, because showing that, in cholera-dejecta, substances can under certain circumstances be contained, which are poisonous to pigs. Dogs seem to be unaffected by them, mice and other animals also, as our experiments show. The power of resistance of other animals to this poison, and the susceptibility of pigs to it, ought to cause no surprise, when we remember that only pigs seem to be killed by the poison which sometimes forms in the brine of salt meat and herrings.

Supposing that comma-bacilli produce a special poison, the phenomena and course of cholera can be explained as follows. The effect of the poison shows itself partly in an immediate manner, the epithelium, and in the worst cases also the upper layers of the mucous membrane of the intestine, being mortified thereby; it is partly reabsorbed and acts on the organism as a whole, but especially on the organs of circulation, which are as it were paralysed. The complex of symptoms of the attack proper of cholera, which is generally looked upon as a consequence of loss of water and the inspissation of the blood, is, according to my opinion, to be regarded essentially as poisoning. For it takes place also not unfrequently when comparatively very small quantities of fluid are lost during life by vomiting and diarrhœa, and when, immediately after death, the intestine also contains only a small quantity of liquid.

If, now, death follow in the stage of cholera-poisoning, then the phenomena met with in *post mortem* examinations correspond to those cases in which the mucous membrane of the intestine is little changed, and the contents of the intestine consist of a pure cultivation of comma-bacilli. If, on the contrary, this stage be prolonged, or if it be got over, the consequences of the mortification of the epithelium and of the mucous membrane show themselves; capillary hæmorrhage in the mucous membrane takes place, and some of the component parts of the blood mix in more or less abundance with the contents of the intestine. The albuminous fluid in the intestine begins to putrefy, and, under the influence of the putrefaction-bacteria, other poisonous products are formed which are also absorbed. But these have an effect differing from that of the cholera-poison; the symptoms caused by them correspond to what is generally called cholera-typhoid.

Corresponding to the view that the comma-bacilli only vegetate and unfold their effect in the intestine, the seat of the infectious matter can only be looked for in the dejecta of patients, only exceptionally in the vomit. In this, I think I am in accord with the more recent views. It is true that this view is still contradicted by some investigators, but we are in possession of incontrovertible proofs in its favour, above all, infection by means of linen; so that, apart from the comma-bacilli, there can be no doubt also that the dejecta really contain the infectious matter. For the further spread of the infectious matter, the first condition is that the dejecta remain in a moist condition. As soon as they are dry, they lose their effectual agency.

One of the commonest ways of spreading the infectious material, of which, too, we have had an example in the tank-epidemic, is water. How easily can cholera-dejecta or water used for cleaning cholera-linen get into wells, public watercourses, and other places for the supply of drinking-water and of water for household purposes. Thence the comma-bacilli find plenty of opportunities of returning into the human household, either in drinking-water or in water used for being mixed with milk, for cooking, for rinsing pots and pans, for cleaning vegetables and fruit, for washing, bathing, etc.

Besides these ways, the infectious matter can enter the human digestive organs by a shorter way; for comma-bacilli can, beyond a doubt, retain vitality for a considerable time on articles of food which have a moist surface, and it can easily be supposed that they are not rarely brought thither by being touched with dirty hands; and I do not consider it at all impossible that the infectious matter is transferred to food by means of insects, for example, by common flies. In most cases, certainly, the infectious matter enters the soil with the dejecta, and finds its way, somehow or other, into wells or tanks.

I start with the assumption that only moist substances, and these of most different kinds (I do not by any means confine myself to drinking-water) that are polluted in any way by moist dejecta, may also convey the infectious matter to the body. On the other hand, I do not think that the infectious matter of cholera can keep in a dry state, or, which is the same thing, that it can be transferred by means of the air. For the dispersion of an infectious matter can, as a rule, only take place by means of the air when dry, and in the form of dust. Experience is also in favour of the view that the infectious matter cannot be introduced in a dry state, for we know that hitherto cholera has never come hither by means of goods on the way from India; never as yet have letters or postal packets introduced cholera, even when not, as is now frequently done, pierced through and fumigated. If the origin of the separate epidemics be carefully looked into, it will be found that cholera has never reached us except through human beings themselves; and although people have not succeeded in the case of these separate epidemics in tracing the individual who brought the infectious matter, one must not conclude from this that this is an exception. For we must consider that it is not only the individual who dies of cholera, or who has an unquestionable attack of cholera, that is liable to transfer infection, but that all possible transitions up to this most violent form of the disease, even slight attacks of diarrhœa, take place, which are probably just as capable of giving infection as the worst case of cholera. Of course, we can only arrive at positive certainty on this important point, when the slightest cases have been proved to be real cases of cholera by tracing the presence of comma-bacilli.

There remains still the important question to be answered, whether the infectious material can reproduce or multiply itself outside the human body. I believe that it can. As the comma-bacilli grow on a gelatine-plate, as they can grow on a piece of linen, or in meat-broth, or on potatoes, they must also be in a position to grow in the open air, especially as we have seen that a comparatively low temperature enables them to develop. I would not certainly assume that the multiplication of the comma-bacilli outside the human body takes place in well-water or river-water without any assistance, for these fluids do not possess that concentration of nutritious substances which is necessary for the growth of the bacilli. But I can easily imagine that, although the whole mass of the water in a tank or reservoir is too poor in nutritious substances for bacilli to flourish in it, yet some spots may contain sufficient concentration of nutritive substances, for example, those spots where a gutter, or the outlet of a cesspool, opens into the stagnant water, where vegetable matter, animal refuse, etc., lie, and are exposed to putrefaction by bacteria. At such points, a very active form of life can develop.

I have often formerly made such experiments, and it has often happened that a water contained almost no bacteria at all, whilst remains of plants, especially roots or fruits swimming in it, teemed with bacteria, especially kinds of bacilli and spirilla. Even in the immediate neighbourhood of these objects, the water was rendered turbid by swarms of bacteria, which clearly received their nourishment from the nutritive matter scattered by diffusion at a very small distance.

I think that we can in this way most easily explain the relations of subsoil-water to the spread of cholera. Everywhere where water is stagnant on the surface or in the ground, in marshes, in harbours which have no outflow, in places where the ground is trough-shaped, in very slowly running streams, and such like, the conditions described can develop. There, in the neighbourhood of animal and vegetable refuse, concentrated nutritive solutions will be most easily formed, and will give the micro-organisms opportunity for forming colonies and multiplying. On the other hand, wherever the water at the surface as well as at the bottom is in a state of rapid motion, and subject to continuous change, these conditions are less easy, or do not occur at all; for the continuous flow of the water prevents the formation of a local concentration of nutritive substances in the liquid sufficient for pathogenic bacteria. The connection between the falling of subsoil-water and the increase of several infectious diseases, I would explain as follows; that, when the subsoil-water falls, the current that takes place in the subsoil-water is much less significant. Besides, the quantities on the surface are much diminished, so that those concentrations, which I assumed to be necessary for the growth of the bacteria, must much sooner take place.

If we assume that a special specific organism is the cause of cholera, we cannot think of an autochthonous origin of the disease, emanating from any particular locality. Such a specific organism, even if it be only a comma-bacillus, follows the laws of vegetation, just as the most highly developed plant. It must always propagate itself from something of the same nature, and cannot spring up at haphazard from other things, or from nothing. But, as comma-bacilli do not belong

to micro-organisms that are distributed everywhere, we are forced to trace back the disease that depends upon them to special localities from which these specific micro-organisms are brought to us. We cannot, therefore, imagine that, because the delta of the Nile resembles the delta of the Ganges in some points, cholera could spontaneously spring up there by way of exception, as was seriously maintained last year. Just as little reason have we for supposing that cholera should spring up here in Europe, without the comma-bacillus having been previously introduced. An attempt has already been made to represent a cholera-epidemic that broke out in Europe, which had apparently originated in Poland in an isolated manner, as autochthonous, but it was afterwards seen that it did not do to allow this kind of origin to pass as tenable. For the cholera had been prevailing in various places in Russia in the form of epidemics that were not noticed, and was introduced into Poland by the troops. I have recently experienced something similar. Ten years ago, cholera broke out suddenly in the town of Hama, in Syria, and nobody knew how it came there. It is still maintained by many people that it broke out autochthonously. I was asked, a short time ago, by medical men in France, for my opinion on the subject, and could only reply, as there is nothing certain written on the origin of the epidemic, that the mode of introducing the plague in this case had not yet been explained; but I expressed my conviction that the origin of cholera in Syria must also be attributed to India, at the same time pointing out how the epidemics in Syria and Egypt, apparently of autochthonous origin, keep to the road of traffic between India and Europe or its immediate neighbourhood, but never originate in places which have no relations with India. Very soon afterwards, I was able accidentally to obtain a satisfactory answer concerning the origin of the epidemic in Syria. Professor Lortet, who was himself at Hama during this epidemic, and had inquired into its place of origin, informed me that the cholera had been brought to Hama from Djedah by Turkish soldiers.

So far, we know of no cholera-epidemics that have broken out spontaneously outside India; hence in this point also, experience agrees with the presumption that cholera is caused by a specific organism, having its habitat in India.

Now the conditions in regard to cholera in India are of a very peculiar nature. I do not think that the whole of India is the native country of the comma-bacillus. It was formerly maintained that cholera was indigenous in Ceylon, Madras, and Bombay, and that it was then spread over nearly the whole of India; but, on the other hand, this has been very rightly disputed. Only about the province of Bengal there is no difference of opinion. All authors are unanimous in holding that the Delta of the Ganges is the real home of the cholera. I also have become convinced that it is in reality so, and that other cholera habitats do not exist in India. For the only region in India where the cholera continuously prevails, year by year, in an uniform manner, is the Delta of the Ganges; in all other places it fluctuates considerably, or often dies out entirely for longer or shorter periods. At individual places, for example, Bombay, it also never entirely disappears, but it is extremely probable that, by the extreme active traffic with the rest of India, it is continually introduced anew.

On the map of the province of Bengal you will see the Delta of the Ganges, which is bordered on the west by the river Hooghly, an arm of the Ganges, and on the east by the Brahmaputra.

The cholera prevails continuously in the whole of this region, and on the banks of the Ganges as far up as Benares. On inspecting the map more closely, it must strike an observer that the upper part of the Delta is not thickly set with villages, whilst the base of the triangle seems entirely uninhabited. This uninhabited district, called Sundarbunds, comprises an area of 7,500 English square miles, and is separated from the thickly populated northern portion of the Delta by an extremely sharp line. Here the large rivers, Ganges and Brahmaputra, break up into a network of streams in which, at the tides, the sea-water, mixing with the river-water, flows backwards and forwards, completely flooding wide tracts of the Sundarbunds at high tide.

A luxuriant vegetation and an abundant variety of animal life have developed in this uninhabited district, a district which is not only unapproachable for man, owing to the inundations and numerous tigers to be found here, but is chiefly avoided owing to the pernicious fevers which attack anybody who stops there for only a very short time. One can easily imagine what quantities of vegetable and animal matter are exposed to putrefaction in the boggy district of the Sundarbunds, and that an opportunity, scarcely to be found in any other place in the world, is offered here for the development of micro-organisms. In this respect, the boundary between the inhabited and uninhabited part of the Delta is especially favourable; where the refuse from an exceptionally thickly populated country is floated down by the small streams, and mixes with the brackish water of the Sundarbunds that flows backwards and forwards, and is already saturated with putrefied matter. Under peculiar circumstances a thoroughly special fauna and flora of micro-organisms must develop here, to which in all probability the comma-bacillus belongs. For everything points to the supposition that cholera derives its origin from this frontier territory. All the greater epidemics commence with an increase of cholera in the south of Bengal. Jessore, from which place the first news about the epidemic of 1817 came, lies on the borders of the Sundarbunds, and Calcutta, which is now the constant habitat of cholera, is connected with the neighbouring Sundarbunds by a boggy and scantily inhabited tract of land.

But further, the comma-bacillus finds, in the districts adjoining the supposed habitat, the most favourable conditions that can be imagined for obtaining a footing and transferring itself from one man to another. For lower Bengal is a perfectly flat country, only very slightly raised above the level of the sea, and during the tropical rains almost the whole extent of it is under water. Hence everybody who settles there, must, in order to protect himself from these annual inundations, build his hut on raised ground. This kind of building is seen in all the villages of the Delta, even in Calcutta itself, especially in the immediate neighbourhood and in the suburbs of Calcutta, which more or less bear the character of villages. Every house or group of houses stands on a flat rising ground, which has been made by taking the earth from a spot near where the hut is built, in order to raise the ground on which the house stands. The excavation thus made becomes filled with water, and forms a so-called tank.

Every hut, or group of huts, must therefore have a tank, more or less large, and the number of these tanks is accordingly very great. The town of Calcutta alone had, till a short time ago, about eight hundred tanks, although a large number of them had been filled up for sanitary reasons. In the suburbs of Calcutta, there are, besides, more than a thousand tanks still existing. I have already mentioned the part that the tanks play in the household of the Indian population, and how adapted they are for spreading the cholera.

It is perfectly clear that an improvement in the supply of water in those districts would exercise a decided influence on the conditions of cholera. In reality, this has been shown in Calcutta. This town, situated on the river Hooghly, has about 400,000 inhabitants, and almost as many people live in the suburbs. Up to the year 1870, Calcutta—*i.e.*, the inner town—had annually from 3,500 to 5,000 deaths from cholera, and the suburbs a corresponding number. From 1865, drainage was begun, first, in that part of the town chiefly inhabited by Europeans, where the houses were separated at greater distances from one another. Later on, the rest of the town was gradually provided with main-pipes; but, up to 1874, there were not many houses in that part of the town—that is, inhabited by the natives—that were really connected with the main pipes. The net of drains was only completed in a few districts of the town. Since then, the works have been continued without interruption, and they have now made great progress.

I must use this opportunity for mentioning a peculiarity of Calcutta. In the interior of the town, amongst massive houses and villas looking like palaces, groups of huts, crowded together and resembling villages, are to be found, exclusively inhabited by natives. These villages, in the interior of the town, are called bustees. The huts of a bustee possess no drains, sewerage, or anything of this kind. All the dirt is collected between the houses, and, owing to the narrow mode of building, can only be removed in a very incomplete manner; and, finally, reaches the tanks directly, or is brought thither by the rain; so that the tanks become the natural places of collection for all liquid filth. There can be no question of connecting such huts with the drainage.

The construction of waterworks for Calcutta was begun at the same time as that of the drainage. The water is taken from the Hooghly several miles above Calcutta, is well filtered, and then supplied to the town. The waterworks were opened in 1870.

From 1865 to 1870, the effect of the drainage, that was being more and more extended, was not visible on the cholera-mortality of Calcutta; but immediately after the opening of the waterworks the cholera diminished, and since then has been, on an average, one-third less than what it used to be. The drainage, which has been considerably improved since 1870, has not increased the diminution of cholera that took place after the introduction of good drinking-water. Hence the favourable effect can, in this case, only be ascribed to the waterworks. If, notwithstanding, cholera still occurs comparatively frequently in Calcutta, the fact can be explained in this way—that the population do not take their water from the waterworks'-pipes, but, according to their old custom, from the Hooghly, or from the numerous tanks.

In the suburbs, which are in immediate connection with the town

and have a most animated traffic with it, but have no share in the supply of water from the waterworks, the rate of mortality from cholera has remained at the old height.

The influence of the water-supply has been more plainly shown at Fort William, which is situated on the Hooghly, almost in the middle of the town. The fort itself is not drained; and, owing to the distance from the nearest town drainage-pipes, cannot be influenced by the town-drainage. The condition of the subsoil-water must be the same as when the fort was built. Formerly, the garrison suffered severely from cholera every year; but, some years ago, the attention of the officers was drawn to the drinking-water; it was kept as much as possible free from pollution; and since then cholera has perceptibly fallen off. Thus the fort received a reliable water-supply at the same time as the town did, and since then cholera has disappeared from the fort. This case can serve as a normal experiment, in which all conditions have remained the same except the drinking-water. If cholera do not invade the fort any more, the fact can only be attributed to the change of drinking-water.

There are also similar, if not so decisive, examples of the influence of drinking-water on cholera in other Indian towns. In Madras, the cholera has fallen off significantly since the introduction of waterworks. The same is true of Bombay. The condition of Pondicherry in this respect is especially interesting. Cholera was formerly very prevalent in this town; some years ago, artesian wells were introduced there, having a depth of from 300 to 400 feet, and from this time cholera entirely disappeared from Pondicherry; but last spring it was suddenly reported that the immunity of Pondicherry, which had been accepted as certain, had been shown to be untenable, as cholera had again broken out there. In consequence of this, I applied to Dr. Furnell, of Madras, who has specially occupied himself with the relations of cholera in Pondicherry, and has always traced it, and received from him the information that a number of cases of cholera had indeed occurred in Pondicherry, but exclusively in those portions of the town which are not yet supplied with artesian wells.

If I have cited to you here some examples in favour of the advantage of a good provision of drinking-water, I need not, after the details I have already given, assure you that I am not a supporter of the exclusive drinking-water theory. I should like to avoid touching every point of view which is one of principles, but I think that the ways in which cholera can spread in a place are extremely diverse; and that, as almost every place has its own peculiar conditions, which must be thoroughly searched out, the measures which are of use for protecting the particular place from the pestilence must correspond to these conditions.

In India, also, the spread of cholera depends upon the intercourse of human beings, and is chiefly caused by the pilgrimages, which have developed in quite an unusual manner in India. We can scarcely form an idea how widely diffused these pilgrimages are in India. In order to give you an example, I will only cite the two chief places of pilgrimage, Hurdwan and Puri. These are places to which hundreds of thousands of people, sometimes more than a million, flock annually from all parts of India. The pilgrims remain there for several weeks; they are penned together in a confined space, and live in a most miserable manner. Tanks are to be found everywhere in these places also, in which thousands of people bathe; they also drink the water from these tanks. It is by no means astonishing if, under these conditions, the disease, when it does break out amongst the flocks of pilgrims, is speedily scattered over the whole of India, and reaches every place.

Originally the cholera took the longer road across the frontiers of India, through northern India, to the centre of Asia, and thence to Persia, and then further on to the south of Europe. All this is changed since commercial traffic no longer takes the caravan-road through Persia, but proceeds over the ocean, through the Red Sea and the Suez Canal. I scarcely think that there is any more fear of the cholera being introduced by land through Asia. It is not exactly impossible that it should again take this road, but it is not probable. On the other hand, the other way, the sea-road from India to Europe through the Red Sea, and especially from the chief harbour, Bombay, will be more dangerous every year, in my opinion. One can now travel from Bombay, which is seldom free from cholera, to Egypt in eleven days, to Italy in sixteen days; and, in eighteen, or, at most, twenty days, one can be in the south of France. Those are spaces of time which are so extremely short compared to those required formerly, that the danger of direct importation of cholera from India to Europe will become greater and greater. As the course of cholera on board ship is of especial interest for these conditions, I beg to add a remark on this point.

It has always struck me that real cholera-epidemics only take place in ships which have a large number of people on board, whereas, in ships with only a small crew, as in all merchant vessels, even when cases of cholera occur in the first days of the passage, epidemics never develop which last for weeks. Because this matter is of the greatest importance, not only for the etiology of cholera, but also for marine traffic, I have made as many inquiries as possible on the subject, and have found this observation perfectly confirmed.

When, therefore, we have to deal with questions that refer to cholera in ships, we must direct our attention to ships used for the conveyance of masses of men, such as transport-ships, pilgrim, coolie, and emigrant-ships. Cholera does not occur so seldom on these ships, when they start from cholera-infected ports, as is often supposed. Sometimes one finds an effort made to represent ship-traffic as quite without danger in regard to the introduction of cholera, it being calculated that, out of a given number of vessels free from cholera, only one is found on which cholera has broken out. To this calculation it must be opposed that, although amongst a thousand vessels only one has cholera on board, this one ship can, of course, cause as much mischief as if the whole thousand had been infected with cholera. But if we confine ourselves to calculate the relation between those free from, and those infected with, cholera, amongst transport-ships conveying large masses of individuals, then the result is much less favourable than is generally supposed.

In the Reports of the Sanitary Commissioner of the Government of India of the year 1871, there is a very interesting digest on the cholera on board the coolie ships that left Calcutta. These vessels are not very large, but nevertheless convey from 300 to 600 Indian workmen, so-called coolies, for the most part to the English colonies in America. In the course of ten years, 222 of these vessels sailed, and 33 of them had cholera-epidemics, the epidemic lasting on board 16 of them for more than twenty days. One can easily form an idea from this of the greatness of the danger of the introduction of cholera for Europe, which is much nearer, if such a transport of workmen were to go from India to Egypt, for instance, or to any Mediterranean port.

On one question of cholera-etiology, which is more of theoretical interest, I have not yet had an opportunity of saying anything, and should therefore like to touch upon it. It is, namely, the explanation of the noteworthy fact, that outside India cholera always dies out after a comparatively short space of time. This disappearance of the plague seems to me to be due to a variety of factors.

In the first place, I consider it established that the individual, as in many other infectious diseases, after having once had cholera acquires a certain immunity. This immunity does not seem to be of very long duration, for we have examples enough, that a man who was attacked during an epidemic caught the cholera again in a second epidemic; but one seldom hears of a man being attacked twice during the same epidemic. But precisely in cholera several attacks ought to occur, because a man who has got over the attack, as a rule, returns after a few days to the same conditions, and exposes himself to the same dangers and the same source of infection. Some experiences made in India, moreover, are in favour of the view that a certain immunity is obtained after having got over an attack of cholera. In the same manner as an individual can obtain immunity, so can whole localities, as a good deal of experience proves, become more or less free from cholera for a certain period. It is often seen that, when a place has been attacked by cholera, and been thoroughly infected by it, this place is often spared the next year, or it only suffers slightly, when the cholera returns.

As a second reason for the extinction of a cholera-epidemic, we must take the absence of a permanent state, capable of assisting the infectious material in surviving the period of the immunity of the population that is unfavourable to its development.

Finally, we must take note of the circumstance that temperatures under 17° C. (62.6° Fahr.) have such an unfavourable effect on the growth of the bacilli outside the body, that their multiplication can no longer take place. When all these factors work together; when, therefore, winter comes on, and only a population remains which is more or less non-liable to the epidemic, then the epidemic must die out, as the infectious matter possesses no permanent state.

Before I conclude, I should like to add a few words as to how we can utilise the discovery of the comma-bacilli. The cry we commonly hear is—Yes, but what is the use of this discovery to us? We certainly know that the cholera is caused by comma-bacilli, but, nevertheless, we are in no better position for curing this disease than before. I remember that these were often the expressions used about the discovery of tubercle-bacilli.

Anybody who looks upon these matters from the point of view of the medical man who has to write a prescription, is certainly right in saying that he has as yet no perceptible utility before him; but these critics ought to consider that rational therapeutics for the

majority of diseases, and especially for infectious diseases, cannot be obtained till we have found out their causes and nature. But, apart from this, I imagine we already have a very considerable advantage from the discovery of the comma-bacillus. I think, first, of how we can utilise it in a diagnostic direction. It is highly important that a correct diagnosis should be taken of the first cases which occur in a country or locality. According to my view, by showing the presence or absence of cholera-bacilli, we can say with certainty whether we have cholera before us or not. This seems to me to be a very essential advantage.

I further think that, after having become acquainted with the real cause of the disease and its qualities, the etiology of cholera can be constructed on definite and fixed lines, and that we shall at length get rid of many contradictions. We shall now obtain a firm basis for an uniform action that knows the end at which it is aiming. I anticipate special advantage from the observation that comma-bacilli are killed by being dried.

It is true that the fact that the infectious matter of cholera is destroyed by being dried, ought properly to have been discovered before by experience, but experimental supports were wanting, and people were never certain on this point. Now we can regard this as a positive property of the infectious matter, and in future take this property into account. But, above all, we can deduce this advantage, that an end will at length be put to the fearful squandering of disinfectants, and that millions will not again, as in the last epidemics, be poured into gutters and cesspools without the slightest advantage.

For the rest, I hope that the recognition of the comma-bacilli can be turned to account therapeutically. We shall be able in future, even in the less severe cases, and in the first stages, to take a diagnosis. In accordance with this, therapeutic experiments, also, will have more certainty when it is known that the patient is really suffering from cholera. An early diagnosis must, however, be of all the greater value, as the chance of therapeutic success is precisely greatest in the first stages.

Professor VIRCHOW, after thanking Dr. Koch for giving this long and detailed account of his views, said: "I say decidedly for myself that, from the beginning, I thought it very probable that the bacillus was, indeed, the *ens morbi*: but, from what I have heard to-day, my conceptions on the subject have arrived at a much greater degree of certainty."

STATISTICS OF EXAMINATIONS.—At the recent pass-examinations for the diplomas of the Royal College of Surgeons of England, 352 candidates presented themselves; from St. Bartholomew's Hospital, 53; University College, 39; Guy's Hospital, 27; St. George's Hospital, 23; King's College, 19; St. Thomas's Hospital, 15; London Hospital, 15; Charing Cross Hospital, 13; Middlesex Hospital, 13; St. Mary's Hospital, 12; and the Westminster Hospital, 11. The provincial schools were represented by Manchester, 24; Cambridge, 13; Leeds, 11; Birmingham, 8; Bristol, 8; Liverpool, 7; Newcastle, 5; and Sheffield, 3. Edinburgh and Glasgow sent respectively 21 and 4; Dublin, 3; Madras, 2; Calcutta and Bombay, 1; McGill, Montreal, 3; Toronto, 2; and Amsterdam, 1. Of the 352 candidates, 60 passed in surgery, and, when qualified in medicine and midwifery, will be admitted members of the college. Of those who failed to acquit themselves to the satisfaction of the examiners, 1 was referred for twelve months, 6 for nine months, 108 for six months, and 40 for three months. The total number admitted members was 137, against 120 at the corresponding period last year.

ST. THOMAS'S HOSPITAL.—The following appointments have been made, for three months, from September 2nd. Resident House-Physicians: G. D. Johnston, M.R.C.S., L.R.C.P.; F. F. Caiger, M.B., B.S., M.R.C.S., L.R.C.P. Non-resident House-Physician: F. W. S. Stone, M.R.C.S., L.R.C.P. (ext.). Assistant House-Physician: R. Lawson, M.R.C.S., L.S.A. House-Surgeons: J. Orford, M.R.C.S., L.R.C.P., L.S.A.; H. B. Robinson, M.R.C.S., L.R.C.P. Assistant House-Surgeon: C. D. Green, M.R.C.S., L.R.C.P. Resident Accoucheur: W. Hull, M.B., M.R.C.S., L.R.C.P. Clinical Assistant in the Skin and Ear Department: T. Glover Lyon, M.R.C.S., L.R.C.P. (ext.). Clinical Assistant in the Special Department for Diseases of the Throat: H. W. G. Mackenzie, M.A., M.B.

THE MANUFACTURE OF TEETH.—It is said that the tooth manufacture of the United States amounts to 10,000,000 teeth per annum. The value of these teeth is a million dollars. The materials of which they are composed are feldspar, kaolin, and rock crystal. There are twelve manufactories, but one of them, founded in 1864, makes half the number.

BRITISH MEDICAL ASSOCIATION.

FIFTY-SECOND ANNUAL MEETING.

PROCEEDINGS OF SECTIONS.

THE SURGICAL AND ORTHOPÆDIC TREATMENT OF INFANTILE PARALYSIS.

Read in the Section of Surgery.

BY BERNARD ROTH, F.R.C.S.

I WISH to refer in this paper to the treatment of infantile paralysis after the acute stage has passed, when it is possible to recognise which muscles are likely to recover more or less complete power, and which will be more or less hopelessly destroyed as far as voluntary contraction is concerned.

It is during this stage, while the previously paralysed muscles are beginning to recover their power, various deformities of the trunk and limbs are prone to occur; deformities which frequently cause more grief and hardship than the permanent paralysis left, and which yet, with care and perseverance, can be nearly always prevented. The two guiding principles in the surgical and orthopædic, or after-treatment of infantile paralysis, are:

1. To improve the power of those affected muscles which have still some voluntary power left.

2. To prevent the onset of any deformity, or, if this has already occurred, to reduce it to a minimum.

With reference to the diagnosis of infantile paralysis, this presents little difficulty when the chronic stage has been reached. In all cases a thorough examination should be made of the trunk and limbs; each joint in turn should be examined; and the patient's will is to be strongly urged to exert voluntary contraction to effect all the movements possible in the given joint, *i.e.*, flexion, extension, rotation, etc. By first doing the movements on the opposite limb, if that be normal, he will more readily understand what is required of the partially paralysed one. When the patient is unable of himself to execute a movement, the surgeon should do it passively, and if there be any contraction or limitation of motion in a joint, this will be at once noticed, and errors of diagnosis avoided.

1. *To Improve the Power of those Affected Muscles which have still some Voluntary Power Left.*—The first thing to be done is to correct the lowering of temperature, nearly always present, of the limb or limbs. If one leg is affected, the parents should be told not to be satisfied unless it is as warm as the healthy one. It will be generally found that, after a night's rest in bed, the paralysed leg will be perfectly warm. The patient should be quickly sponged all over, on rising, with tepid water, followed by good rubbing and drying, and sufficient extra clothing applied to the affected limb. Loosely knitted woollen stockings, thick cloth leggings, or, best of all, cloth leggings lined with cat's skin, or other fur, should be constantly worn during the day.

Warm baths, temperature 98° to 100° Fahr., for from ten to twenty minutes every evening, are most useful. For young patients of from five to ten years old, a small barrel standing on end answers admirably, as the patient can stand in it up to the waist or neck in water if required, and a smaller quantity of hot water is needed. The bath should be followed by a rapid sponging of the whole body with cold water, so that no undue sensitiveness to changes of temperature be induced. If properly applied, the cold sponging produces such a reaction after the bath, that the pale paralysed limb becomes as red as a boiled lobster.

Next, *massage* or rubbing is specially indicated, and although many medical men employ it, it is seldom practised sufficiently long daily, or with enough force. Half-an-hour, or an hour, twice daily, is often hardly enough. In every case the rubbing should be practically superintended by the surgeon for the first time or two. In cases of long standing, more or less of the situation of the wasted muscle is frequently taken up by tenacious adipose tissue, through which the pressure of the rubber's hands has to be transmitted to reach the diminished mass of the muscle.

The rubbing or massage I employ may be roughly classed as kneading, circular friction with the thumb, fulling, and firm stroking down. "*Kneading*" is a combination of grasping, and large pinching and pressure, with the two hands used alternately, one after the other, so that whatever is left of the wasted muscle, is thoroughly squeezed and