

ports, floating hospitals are provided, for the purpose of isolating suspicious cases of infectious disease.¹

Almost all the best authorities now recognise the uselessness of sanitary cordons, and of general and indiscriminate methods of quarantine.

The most thorough isolation of the first cases which occur in any district is essential; after the disease has assumed the proportions of an epidemic, isolation is, of course, impossible.

2. The most scrupulous attention should be given to sanitary arrangements, both by private persons and by public authorities. It is unnecessary, after what has been said in the previous parts of this lecture, to dwell on this most important means of diminishing the severity of an epidemic of cholera should it unfortunately arise. It is, however, with great satisfaction that I can assure you that our local sanitary authority seems fully alive to the importance of this matter. Never since I have known Edinburgh, have our wynds and alleys, which are the sort of places in which the cholera poison might be expected to chiefly locate itself and propagate, appeared to me to be in such good sanitary condition, as they have been during the past year.

3. In times of epidemic, every member of the community should look carefully after his own health, and should endeavour to keep it in the highest possible state of efficiency. In particular, the diet should be plain and simple; all foods should be thoroughly cooked; articles of food, such as unripe or rotten fruits, which are likely to cause intestinal irritation, should be rigidly avoided. Anything which produces gastro-intestinal irritation and diarrhoea is a most powerful predisposing cause of the disease. All depressing causes, such as over-fatigue, exposure to cold, excesses of all kinds, anything in short which exhausts the nervous energy, or lowers the vitality and resisting power of the system, should be carefully avoided; for they undoubtedly predispose to the disease. The wearing of a flannel belt round the bowels is highly recommended by many experienced authorities. Amongst the predisposing causes there is none more potent than fear; persons of a nervous temperament, and those who are afraid, should leave the infected locality. No water should be drunk which has not been previously boiled, for the cholera-poison is usually distributed through water, and boiling is the most effectual way of destroying it. Milk and other fluids, which could by any chance be contaminated with the cholera-poison, should also be boiled. The ventilation of private houses, both the sleeping and sitting-rooms, must be carefully attended to.

4. Should any symptoms of illness arise, medical advice should be immediately summoned. Diarrhoea is usually the first symptom. There is no difference of medical opinion as to the treatment of cholera, but I altogether agree with those who think that the diarrhoea should be immediately stopped by appropriate remedies. This was the treatment which was adopted in my own case (for I was attacked with this symptom, which may or may not have been the commencement of cholera, that I cannot say); and it is the treatment which I should again adopt were I to be again attacked with any manifestations of the disease. During an epidemic of cholera, every one should apply to his medical attendant, and have the appropriate remedies ready in his house; much depends on the early treatment, for the disease is one which runs a very rapid course, severe cases often proving fatal within a few hours.

5. Thorough disinfection of the cholera evacuations; thorough disinfection, or, better still, destruction (burning) of all linen, bedding, etc., which has been soiled by the evacuations, or been in contact with the patient; thorough disinfection of infected dwellings, etc., should be carried out. Patients affected with cholera, and houses in which there are cases of cholera, should be kept scrupulously clean. The cholera-evacuations should, immediately after being passed, be mixed with a strong disinfectant; a solution of corrosive sublimate or of persulphate of iron is probably the best; or after being received in a vessel containing sawdust, they should be immediately burned. Linen bedclothes, etc., which have been in contact with cholera-patients, or soiled with cholera-discharges, should be immediately immersed in boiling water, or in an antiseptic solution, and should be then boiled. Should the nurse or attendants become soiled with the cholera-evacuations, the dress should immediately be changed and disinfected, and the parts of the body which have been exposed to the contagion, or better still, the whole body, carefully washed with an antiseptic solution.

Before meals, care should be taken that the hands are scrupulously

¹ The Tyne Sanitary Authority, of which my father was for many years the medical officer, was one of the first, I speak under correction, but I think I am right in saying the first, sanitary authority to adopt this arrangement, the great value of which I had many opportunities of observing during the four years and a half I was Visiting-Physician to the Tyne Floating Hospital.

clean; dishes, glasses, knives, forks, etc., should be immersed in boiling water before being used.

From what I have said, you will have perceived that (*a*) the isolation of early cases, (*b*) the destruction of the poison given off from infected persons, and (*c*) scrupulous attention on the part of every individual to avoid introducing any living cholera-germs into his own person, through food and drink, together with (*d*) the avoidance of all predisposing causes, and (*e*) attention to general sanitary matters, are the chief means by which we endeavour to limit the spread of this disease.

My Lord Provost, Ladies, and Gentlemen,—I had hoped, when I began to think over the subject of this lecture, that I might have been able to tell you something about the indirect or predisposing, as well as of the exciting, causes of disease; but that is both a large and important subject, and to treat it at all satisfactorily would occupy at least another hour. I dare not attempt to trespass further on your patience this evening. In conclusion, I can only say that I do most sincerely trust that the information I have given you may prove of real helpfulness and usefulness, to some of you at least, in the conduct of your future lives.

FURTHER RESEARCHES ON CHOLERA.

By R. KOCH, M.D.,

Professor of Hygiene in the University of Berlin.

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

(Continued from page 8.)

It is only in the matter of experiments on animals that advance has been made, Nicati and Rietsch having succeeded in infecting dogs and guinea-pigs by injection into the duodenum of cholera-dejecta, or of pure cultivations of the comma-bacilli. These experiments have been repeated and confirmed here in the Sanitary Institute, and also by Babes, Flügge, and Watson Cheyne. Rietsch and Nicati at first believed that the infection could only be brought about by arresting the flow of bile; and therefore they ligatured the ductus choledochus. However, they afterwards found that the experiment succeeded without this ligature. Our experiments also were performed partly with and partly without ligature of the ductus choledochus, and gave the following results. Of ten guinea-pigs, in which the bile-duct was tied, and their cultivations of the comma-bacilli injected into the duodenum, six died of cholera in the first two days; the remainder died later, as the result of the ligature. You see here, preserved in alcohol, the abdominal organs of such a guinea-pig, which survived the injection, but quickly lost flesh, and died on the ninth day after the operation. The gall-bladder is enormously distended, as is also the ductus choledochus above the point of ligature. There is no trace of peritonitis in this case. In another guinea-pig, which died on the twelfth day after the operation, the gall-bladder had burst, and the abdominal cavity was filled with bile. The two remaining guinea-pigs died as a result of twist and closure of the intestine, in consequence of peritonic adhesions in the immediate neighbourhood of the seat of ligature. In these experiments it struck me that, the better the operation was performed, and the less extensive the manipulations, so much the less chance was there of the animals dying of cholera. Of the eighteen animals in which the injection was made into the duodenum, without ligature of the gall-bladder, thirteen died of cholera.

At the same time, control experiments were also made by injecting other forms of bacteria, for example, micrococcus prodigiosus, various forms of bacilli, etc., into the duodenum of guinea-pigs. Of these animals, in which the bile-duct was not tied, none died. This shows that the operation is not of itself dangerous to the animals. Klein asserts that the guinea-pigs in these experiments did not die of cholera, but of septicæmia. But, according to my experience, the danger of septicæmia after this very simple operation can be excluded with certainty, and one must proceed very clumsily to lose the animals from septicæmia.

In this set of experiments also (without ligature of the bile-duct) the results are so much the less positive, the less the disturbance, and the less the intestine is squeezed or torn in searching for and pulling forward the duodenum. Hence the experiment succeeds only exceptionally when one limits oneself to opening the abdominal cavity only to a small extent, and making the injection into the coil of intestine first exposed, instead of into the deep-lying duodenum. Of six guinea-pigs which were operated on in this way, only one died of cholera, the rest remained alive. The same experiment was then performed on four rabbits, without any of them dying or even becoming ill.

Although, therefore, a process analogous to cholera in man can be produced in the digestive tract of animals by injection of comma-bacilli into the duodenum, nevertheless, one must admit that this mode of infection corresponds very slightly to the natural process of infection. It is by no means an insignificant injury to open the abdominal cavity and search for and pull forward the duodenum, in order to introduce the infective material into this part of the intestine. I have, therefore, made every effort to infect the animals in a natural manner, and my attempts have been finally successful. It seems to me not unimportant to describe the way which ultimately led to the positive result. The earlier experiments had already shown that the comma-bacilli were killed in the stomach; for, if one feed animals with choleraic fluids, or with cultivations, and kill them after some time, one does not find any comma-bacilli in the stomach or intestine. They have died in the stomach. But the absence of infection could not be due to this cause alone; for in the injection into the duodenum the stomachic digestion was avoided, and, nevertheless, the infection did not always succeed. I have, therefore, in order to gain some idea of the conditions which come into play here, made a number of preliminary experiments, confining my observations to guinea-pigs, as these animals seem to be particularly susceptible to the cholera-infection. In the first place, it was necessary to study more minutely the natural digestive conditions in guinea-pigs. If one examine a recently killed guinea-pig, the stomach is found to be at all times filled with a firm mass of food, so that if anything, for example a fluid, be introduced into the stomach, it cannot at once pass through it. I had at first supposed that, if a large quantity of fluid containing bacilli were injected into the stomach of these animals, this fluid could be forced through the stomach; but I very soon convinced myself that this is quite impossible, for the stomach would be burst sooner than the thick firm mass which fills it could be forced to one side. In contrast to the stomach, the small intestine in guinea-pigs is almost empty. The contents of the stomach have a strongly acid reaction, the mucous contents of the small intestine are alkaline; but the cæcum, which is very large in these animals, has again distinctly acid contents; so that it is only in the small intestine that there would be any opportunity for the comma-bacilli to grow and increase. In order to learn something as to the time which the ingesta take to pass through the stomach and intestine, guinea-pigs were fed with different kinds of food alternately, with carrots and hay. In animals killed from one to two hours after the change of food, it was seen that the foods do not mix in the stomach, as is the case in digestion in man, but that they are pushed through the stomach slowly in layers in the order in which they are taken in, so that a red layer of carrots follows, and is sharply marked off from a dark-greenish layer of hay, or *vice versa*. The food passes remarkably rapidly through the small intestine into the cæcum. This could be very distinctly seen by feeding the animals with colouring matters.

If, for example, a quantity of Chinese ink were injected into the stomach, one could follow with yet greater distinctness the passage of the food in layers through the stomach, and notice that the colouring matter, as soon as it had passed through the pylorus, could be found in a very short time in the cæcum. Similar experiments were made with small blue glass beads, and it was found that the beads passed through the stomach in a comparatively short time, but only with the layers of food with which they entered it, and that they then wandered through the small intestine very quickly, probably in a few minutes, to remain in the cæcum for a considerable time. For example, in one experiment, 250 beads were administered, and the animal was killed in three hours. At that time, only half the beads were present in the stomach, the small intestine did not contain a single one, while a large number were present in the cæcum. An experiment in which the animals were fed with anthrax-spores is also worthy of mention. It might be assumed that beads are bodies from the behaviour of which in the stomach and intestine one could not draw conclusions as to the behaviour of bacteria. Four guinea-pigs were fed with large quantities of anthrax-spores. One of these animals was killed after two hours and a half, and the contents of the stomach, small intestine, and cæcum were tested in plate-cultivations in nutrient gelatine as to the presence of anthrax-bacilli. In this case, many anthrax-spores were found in the stomach and also in the small intestine, and some spores had even reached the cæcum; thus, in the short space of two hours and a half, the mass of food had passed through the stomach and small intestine. A second animal was killed after three hours; the number of spores in the stomach was already markedly less; in the small intestine, they were still numerous, as also in the cæcum. The third guinea-pig was killed after three hours and a half, and then the stomach contained only a few spores; in the small intestine, they were much less numerous than in the other cases,

but, in the cæcum, there were considerable numbers. In the fourth animal, killed after five hours, there were only very few spores in the stomach and small intestine; in the cæcum, on the other hand, they were very numerous.

From these experiments it was evident that pathogenic bacteria, when given to a guinea-pig as food, pass the stomach and small intestine surprisingly quickly, but nevertheless remain long enough in the stomach to be destroyed by the gastric juice, unless they be in the resisting resting stage, like anthrax-spores.

It was now necessary to attempt to get bacteria free from spores, such as the comma-bacilli, to pass uninjured through the stomach. In order to render this possible, fluid containing bacteria was made up into pills, which were covered with keratin; and, as this method failed, other substances, such as collodion, caoutchouc, paraffin, etc., which are insoluble in the stomach, were used as coating materials, but all in vain. Thereupon, I attempted to destroy the acid reaction of the stomach, if only for a short time. In the first place, I ascertained what doses of the alkaline fluid could be borne by guinea-pigs without injury. A 5 per cent. solution of carbonate of soda proved most suitable for the purpose, and five cubic centimètres of this fluid could be administered without any noticeable bad effect. If, then, a sample of the contents of the stomach were removed by means of a catheter, it was found, as shown in a number of experiments, that even for three hours the reaction of the contents of the stomach remained alkaline. When we had ascertained this, we at once proceeded to feed animals, whose gastric juice had in this way been rendered alkaline, with cholera-cultivations, or to inject the same into the stomach by means of a catheter. In the first experiment, seven guinea-pigs were employed. These received five cubic centimètres of the soda-solution; and some time afterwards, in order not to bring the cholera-bacilli immediately into contact with the soda-solution, ten cubic centimètres of meat-infusion, in which cholera-bacteria were growing, were administered. These animals appeared quite well afterwards. As, even later, nothing seemed to be wrong with them, they were killed after twenty hours, and the contents of their stomach, intestine, and cæcum were examined by gelatine-plate cultivations. In six of the seven animals, the cholera-bacteria could be demonstrated in the small intestine. The experiment had thus in so far succeeded, that the cholera-bacilli had passed uninjured through the stomach; but they had not set up any disease in the animals. The experiment was then repeated in this way: two guinea-pigs received 2 per cent., and six guinea-pigs 5 per cent. of soda-solution; and then the cholera-bacilli. These animals also remained quite healthy. From these results, it was at least evident that there was no danger in introducing a syringeful of a 5 per cent. solution of soda into the stomach. Finally, a third experiment was made with four guinea-pigs, which also received, first a 5 per cent. solution of soda, and then the cholera-bacilli. These animals likewise remained healthy; but one of them appeared ill next day, looked shaggy, and did not eat. On the following day, it was very ill. It exhibited very peculiar symptoms, which were already known to me in the case of animals infected by injection into the duodenum. There was a paralytic weakness of the posterior extremities; the animal no longer supported itself on its hind legs, but lay quite flat, with its limbs stretched out. The respiration was weak and slow. The head and extremities were cold, the pulsations of the heart hardly perceptible, and the animal died after it had lain for a few hours in this condition. Immediately after death, an examination was made, and the most marked choleraic changes were found in the intestinal canal. The small intestine was markedly reddened, and full of a flaky, watery, colourless fluid. The stomach and cæcum also did not contain, as usual, firm masses, but a large quantity of fluid. Diarrhoea had not occurred; and, corresponding to that fact, there were still firm balls of feces in the rectum. The examination with the microscope and with gelatine plates showed that the contents of the small intestine contained a pure cultivation of numerous comma-bacilli. Now, it was very striking that, of nineteen animals, only one had become infected, and this by chance an animal which had aborted immediately before the infection. On *post mortem* examination, it was found that the abdominal walls were very flaccid, and the uterus still greatly enlarged. This led me to the idea that either the abortion *per se*, or perhaps its unknown cause, had acted on the other abdominal organs, more especially on the small intestine, in such a way as to produce a temporary relaxation, with arrest of peristaltic movement; and thus had rendered it possible for the comma-bacilli to remain longer, and gain a footing in the intestine.

In order to produce a similar condition experimentally, alcohol, chloral, morphine, atropine, and opium were employed in the form of subcutaneous and intraperitoneal injections, and it was found that opium answered the purpose best; opium must, however, be adminis-

tered to guinea-pigs in a special manner. Incredible doses can be given internally to these animals without producing any appreciable effect. This fact may, to a certain extent, find an explanation in the previously described conditions of the food in the stomach. The dose of tincture of opium administered cannot at once come into action, because it at first remains among the large quantity of food present in the stomach, and is only gradually absorbed; hence a sure administration of the drug is not possible, and I have therefore preferred to introduce it by injection into the peritoneal cavity, which is very easily carried out, and without danger.

I employ opium in the form of tincture, and in the dose of 1 cubic centimetre to every 200 grammes of the body-weight of the animal. After such a dose, a marked narcosis occurs in a very short time, and lasts for half to one hour, after which the animal becomes as lively as before. Thirty-five guinea-pigs were experimented on by introducing the soda-solution and the cholera-cultivation into the stomach, and subsequently injecting tincture of opium; of these, thirty died of cholera. The clinical symptoms and the *post mortem* appearances were the same as in the case of the guinea-pigs where the injection was made into the duodenum, and of the guinea-pig before mentioned, which died after the introduction of the soda-solution and cholera-cultivation alone.

If the dose of the soda-solution or of the cholera-fluid be reduced, the result is somewhat more uncertain. Thus, for example, fourteen guinea-pigs were fed with 5 cubic centimetres of the soda-solution, and then with a fluid containing only one-third of a drop of the cholera-cultivation. Of these only seven died of cholera, the remaining seven remained well.

In yet another experiment, in which the dose was still further reduced, twenty-seven guinea-pigs were employed, of which only seven died. In all, eighty-five guinea-pigs have up to this time been infected in this way with cholera, and have always died with the same characteristic symptoms and *post mortem* appearances. I will only mention in addition, that one can also successfully carry the infective material from one animal to another. In the place of the cholera cultivation, the contents of the small intestine of a guinea-pig which had died of cholera were administered to another, which in like manner died of cholera.

Very remarkable results are also obtained when other bacteria are administered to these animals along with the soda-solution and tincture of opium, in the same manner as in the case of the cholera-bacilli.

Finkler's bacilli can also kill guinea-pigs by this method of infection, but they are not so virulent as the cholera-bacilli, for of fifteen animals infected therewith only five died. The *post mortem* appearances were also different in this case from those which died of cholera. The intestine was indeed likewise filled with a watery fluid, but it had a pale grey appearance; the vessels were not nearly so markedly injected as in the cholera animals, and the intestinal contents had a penetrating putrefactive smell, which corresponded exactly to the smell produced by Finkler's bacteria in nutrient jelly.

The curved bacilli, cultivated by Deneke from old cheese, were also tried, as well as those obtained by Miller from a hollow tooth, which latter are apparently identical with Finkler's. Of fifteen animals infected with Deneke's bacilli, only three died; of twenty-one infected with Miller's, only four.

The guinea-pigs which survived these experiments were afterwards fed with cholera-bacilli, and all succumbed to the cholera-infection.

Pathogenic bacteria which, under ordinary circumstances, do not act on guinea-pigs from the intestine, can be made to do so by the use of this method; as, for example, anthrax-bacilli free from spores, and the previously mentioned Brieger's bacteria. Others, such as the bacteria of chicken-cholera, of osteomyelitis, of rabbit septicæmia, and of erysipelas, produced no effect under these conditions. The bacilli of typhoid fever even, which have not as yet been successfully inoculated on animals, gave a doubtful result, and hence the experiments with them must be repeated.

I would only mention, in addition, that attempts were made in other ways to set up, in the intestine, conditions suitable for the development of the cholera-bacteria. For example, we gave animals castor-oil and croton-oil, or fed them with yeast, in order to set up an intestinal catarrh. Further, we have injected turpentine-oil, tincture of iodine, glycerine, alcohol, etc., into the peritoneal cavity, and, in a few cases, have had successful results therewith; by the use of alcohol, we were most successful in making the animals susceptible to the cholera-infection, but, as a whole, the effect of alcohol was not so good as that of tincture of opium.

We can now conclude, from these experiments on animals, that the cholera-bacteria possess very potent pathogenic qualities, and are able

to show them when they enter uninjured into the intestine, and find it in a condition in which they can obtain a footing and develop. In guinea-pigs, these conditions can only be produced artificially; but, in man, the conditions of gastric digestion are quite different from those in the guinea-pig. The human stomach is not constantly filled with a strongly acid fluid, like the stomach of our experimental animals. Probably, it is often in a condition in which the contents have a neutral or even alkaline reaction; for example, always after the end of true gastric digestion, and the passage of the chyme into the small intestine.

To Professor Ewald, who has recently busied himself with this question, I am indebted for some interesting facts with regard to this point. He found that, when water was introduced into the empty stomach by means of the stomach-tube, it remained for a considerable time neutral, or even took on an alkaline reaction. At the same time, the quantity of water present in the stomach gradually diminished, showing that the stomach was constantly emptying a certain quantity of its contents into the small intestine. Possibly this slow diminution of the water might also be produced by absorption from the stomach, but in about one hour to one hour and a half there was suddenly a rapid diminution of the fluid in the stomach, even before an acid reaction had occurred. Evidently the pylorus had then opened, and permitted the passage of the gastric contents into the small intestine in large quantities. If we now assume that cholera-bacilli were accidentally present in this water, they would undoubtedly have entered the duodenum in a living state, and might there have possibly brought about a cholera-infection. In man, therefore, artificial preparations are not necessary for infection, as they are in the case of the lower animals.

It may also be further concluded from this, that man will not, as a rule, behave quite in the same manner according to the condition of the gastric digestion. The different individual predisposition of man depends, perhaps, in great part on the state in which gastric digestion happens to be when the infective material entered the stomach; further, on the state of the intestine, whether it perhaps more or less nearly approaches the condition in which the intestine of guinea-pigs is after the injection of tincture of opium. In this way, we obtain a certain conception of the mode of infection; and I doubt not that by further experiment in this direction we shall arrive at further results, and perhaps be able to clear up much which is as yet dark to us. Further, now that we have succeeded in infecting animals through the stomach, we shall be able to test experimentally the effect of medicines on the cholera-process.

As I have already formerly stated to you, we can only imagine the action of the cholera-bacilli which do not pass into the blood, to be by the production of a poisonous substance belonging to the group of ptomaines, which is absorbed, and then acts on the organism as a whole. In order to obtain some basis in fact in regard to this view, I have busied myself in trying to demonstrate the poisonous products of the cholera-bacilli which we must suppose to exist; but these investigations have not as yet advanced far. Only so much has already been found, that it is possible to make cultivations of comma-bacilli, which act intensely poisonously, and which, when injected into the animals subcutaneously, or into the peritoneal cavity, produce in a few minutes the same set of symptoms which occur in animals sick of cholera in one to two days after injection, namely—semiparalytic weakness of the hinder extremities, coldness of the head and of the limbs, and slowing of respiration—conditions which are generally followed by death in a few hours.

I may add a few remarks on other experiments which do not stand in immediate connection with the infective experiments. In the first place, with regard to the resisting power and vitality of the cholera bacteria. On this point experiments have recently been made by Nicati and Rietsch; and by Babes. We have also again taken up this question, and have in the first place tried to learn how long the cholera-bacteria retain their vitality in well, river, and canal water, in faeces and in the contents of cesspools; and our experiments have shown that cholera-bacilli mixed with well-water can be demonstrated therein up to thirty days afterwards. In the water of the Berlin Canal they remained alive only six to seven days, mixed with faeces only twenty-seven hours, and in the contents of cesspools they could no longer be demonstrated after twenty-four hours.

Experiments have also been made to keep the cholera-bacteria alive for some time on articles of clothing, as, for example, linen, cloth, etc., in a moist state. As far as we have as yet gone with these investigations, we have found that the vitality of the cholera-bacteria is not of very long duration. They were already dead after three to four days.

At our first conference I mentioned, as you will remember, the

strikingly rapid death of the cholera-bacilli in the dry condition, and pointed out the practical bearings of this peculiar circumstance. My statements were at first much doubted; but all trustworthy observers have convinced themselves of their accuracy, and I may take this opportunity of reiterating the statement, and beg that the most extensive use of this fact be made in combating this pest.

With regard to disinfection, I can report some results obtained with carbolic acid. The cholera-bacteria are killed in a few minutes in fluids containing 5 per cent. of carbolic acid. Sulphate of iron, sulphate of copper, and other metallic salts, are also potent, but not nearly so certain, because a more or less considerable part is precipitated by constituents of the nutrient solutions. This would naturally also occur in disinfection of the cholera-dejecta; hence I give the preference decidedly to carbolic acid.

Finally, I would direct your attention to an observation with regard to the action of the cholera infective material on man.

Ample experience in this direction was supplied to us in our former conference, by facts with regard to the infection of those persons who have to do with cholera-linen, and many examples of this kind were furnished during the last epidemic in France and Italy. It was therefore not impossible that, in working with the cholera-bacilli, an unintentional infection might occur. Bearing this in mind, all precautions were adopted to prevent this danger during the cholera-courses held here in the Sanitary Institute. But in spite of all our care an infection did occur, which fortunately has not been followed by bad consequences. Before I give the details of this case, I will remind you that some investigators have made infective experiments on themselves. Thus Bochefontaine, in Paris, swallowed cholera-dejecta in the form of pills, without becoming infected with cholera; Klein, according to an announcement in the *Indian Medical Gazette*, when he arrived in Bombay, drank a fluid which was supposed to contain cholera-bacilli. Apart from the fact that in these cases it was not at all proved that the true cholera-bacteria were present in the material taken, the negative results prove, of course, nothing, as it is extremely probable that a healthy stomach, during the period of digestion, destroys the bacilli, and therefore it is not to be expected that, when cholera-bacilli are introduced into the human stomach, infection must follow in every case. But, even though the results of the experiments had been positive, they would have been just as little convincing, because they were done in places where infection could have occurred in other ways.

An experiment of this kind, performed in a place subject to cholera, can only have any force when the infection concerns, not a single individual, but a number of men at the same time; because then the probability that all who were taken ill were infected in consequence of an accident apart from the experiment, becomes so much the less the greater the number of those infected. Hence I lay great value on the instance of cholera-infection cited by Macnamara, which I may recall to your recollection at this time.

I have corresponded with Macnamara himself with regard to this case. The statements in his work on cholera with regard to it are somewhat short. For instance, he does not say where the case occurred; he mentions no names; but he might have reasons for doing so. I have been always assured in Calcutta, where there was much talk of this case, that it is a fully trustworthy observation, and that the facts were exactly as Macnamara reported. He himself has written me that he is at any time ready to make privately more exact communications on the subject, which must remove every doubt; hence I am convinced that everything occurred as Macnamara has stated, and that this observation can be unhesitatingly accepted as of scientific value. In the *Dictionary of Medicine*, Macnamara makes the following statement with regard to it.

"By an accident"—what sort of an accident it was is not mentioned—"cholera-dejecta became mixed with water. This water remained for a whole day exposed to the heat of the sun, and was then drunk by nineteen persons, five of whom were taken ill with cholera within thirty-six hours."

As I have been assured, after special inquiry, almost no cholera was present at that time, and particularly in the place where this accident happened. Further, all the persons acquainted with the Indian conditions, to whom the case was known and whom I questioned with regard to it, had no doubt that these individuals were taken ill really in consequence of drinking the water contaminated with cholera-dejecta.

In our case of cholera-infection, observed during the cholera-courses, we had to do not with an illness affecting a number of individuals, but only with the infection of one person. Nevertheless, this case is of great significance, because it occurred at a place and at a time when every other source of cholera-infection, than the manipulation of the cholera-bacilli, was absolutely excluded, and because this is as yet the

only case in which in Germany the true cholera-bacilli have been demonstrated in the dejecta of one suffering from cholera.

The physician in question (whose name and place of residence you will, of course, excuse my mentioning) had been five days in Berlin, when he was attacked by slight digestive disturbance, accompanied with diarrhoea. The evacuations were loose, and occurred several times a day; so that his condition excited no alarm. But, on the last day of his attendance here, more frequent quite thin watery evacuations occurred. He thought, however, that he was able to travel, he did so, and reached home, but was then seized with a true attack of cholera. For two days he had very frequent watery and colourless stools; there were great weakness and unquenchable thirst; the urinary secretion was reduced to a minimum. True cramp of the calves of the leg did not occur; but there was marked contraction of the sole of the foot, and cramp in the toes. As he felt too weak to examine his evacuations himself, he put a small quantity in a well-cleaned flask, and sent it here. The vessel was sent off in the evening, arrived here on the following morning, and was at once investigated. As the transit only occupied a night, and that in the cold season of the year, the contents could not have been materially altered by the transport. The investigation of the dejecta, which was made by means of cover-glass preparations, and also by plate-cultivations and cultivation in cupped slides, showed by each method the presence of very numerous true cholera-bacilli. One of the pure cultivations of cholera-bacilli shown to-day came from the dejecta of this case. I will only mention, in addition, that the disease passed on to recovery. The diarrhoea ceased, but there remained for a long time a marked weakness. I cannot omit to point to this case as a warning to those who experiment with cholera-bacilli, and who do not go to work with the greatest caution.

As the question of the presence of a resting form of cholera-bacilli is down in our programme, I will say a few words with regard to it. On account of the importance of this question, I have still done my utmost to find something which could be looked on as a resting stage of the cholera-bacilli, analogous to the spore-formation of other bacilli; but I have, just as in my former investigations on this point, only obtained negative results. All the statements as yet made by other observers on the resting forms and spore-formation depend evidently on errors. Thus Ceci believed that he had observed spores in the cholera-bacilli. He looks on the bacilli which almost regularly occur in old cultivations, and which show an unstained spot in the middle after staining with aniline dyes, as spore-bearing. These peculiarly stained bacilli also struck me in my first cultivations; but I very soon convinced myself that the part of the bacillus which remains unstained is not the result of the formation of a spore, but arises when the bacillus becomes somewhat thicker or fatter. Probably, as the result of this, swelling occurs in consequence of absorption of water, and a separation of the plasma takes place into a thicker, more intensely staining part, at the ends of the bacilli, and a less concentrated substance lying in the middle. A similar appearance is observed in the bacteria of rabbit-septicæmia, which constantly take on the stain in such a way that the middle is little or not at all stained. As in the case of the cholera-bacteria, this appearance only occurs in old cultivations; one must look on it as a form of involution, or as an alteration in the dying and dead bacilli. What is decisive in favour of this view is the fact that cultivations which contain bacilli of this kind are not in the least more resistant to hurtful influences, such as dryness, heat, and chemical substances, than are the ordinary comma-bacilli. Ceci has also himself found that his apparently spore-bearing bacilli died after a very short time when they were dried, and were, therefore, not in a resting form.

At this opportunity I may also mention, as curiosities, that Klein thinks he has observed longitudinal division in the cholera-bacilli; and that, according to Ferran, the cholera-bacteria belong to the cycle of development of a fungus, a peronospora. Both these supposed discoveries rest on erroneous interpretations of the involution forms of the cholera-bacilli.

Although a true resting-stage of the cholera-bacteria has not as yet been found, and does not seem to exist, nevertheless other experiences enable us to explain the temporary quiescence of a cholera-epidemic, which may last at times for months, or even through a whole winter. The cholera-bacilli can, in contrast to their slight resisting power to dryness, retain their vitality under certain conditions for a long time in a moist state. It has already been found by Nicati and Rietsch that the cholera-bacteria may remain alive for eighty-one days in the harbour-water of Marseilles. We have found, on testing old cultivations made on agar-agar, that, even after 144 days, cholera-bacteria capable of development were still present in these cultivations. Only on examination after 175 days were the cultivations found to be dead.

One can, accordingly, easily understand that in the superficial

layers of the ground, in swamps, etc., the cholera-bacteria may find conditions in which they may, just as in our moist agar jelly, and perhaps much better, be preserved from death for five months, or even longer.

INFLAMMATION OF THE MUCOUS LINING OF THE FALLOPIAN TUBE, WITH PURO-FIBRINOUS EXUDATION.

By JAMES OLIVER, M.B. Ed., M.R.C.P. Lond.,
Assistant-Physician to the Hospital for Women, London.

In subacute or chronic pyosalpingitis, an exudation containing many pus-cells is secreted by the altered mucous lining of the Fallopian tube. The fluid so effused may escape into the uterus, and be expelled. It may, however, be detained in the tube itself, and, accumulating, distend this structure. The disease is, in the majority of cases, secondary, and appears most commonly to accompany those inflammations of the genital tract which are of a specific nature, gonorrhoeal or syphilitic. It rarely develops as an independent disease in females previously healthy, yet it may accompany simple inflammation of the uterine lining in persons of a strumous habit of body, or those otherwise debilitated.

In inflammation of the mucous membrane of the Fallopian tube, with puro-fibrinous exudation, there is usually, in the early days, some elevation of temperature and increased frequency of pulse. The disease, however, may exist for months with no other subjective signs than pain and a varying amount of purulent discharge. In the later days, when fluid has begun to accumulate in and distend the tube, and inflammatory changes are apt to invade the surrounding structures, symptoms of a definite and truly characteristic nature invariably develop; and it is to these more especially that I now wish to draw attention.

Pain is a constant and troublesome symptom. Its seat and character, however, vary according to the extent and manner in which the pelvic nerves severally become involved. It is more or less constant or periodic, and is usually referred to that iliac region in which the diseased condition is to be detected. If both Fallopian tubes are the seat of change, as is commonly the case when the disease is due to specific inflammatory extension, pain is complained of in the neighbourhood of both groins. In many cases, the attacks of pain are periodic, lasting six or seven days, with an interval of immunity of like duration. The onset of the recurrence is generally accompanied by a feeling of faintness. The pain, as a rule, is materially altered by position, the patient experiencing relief and a feeling of comfort when lying on the affected side. It is aggravated by the upright posture and by locomotion; and a dragging sensation is often developed when the patient is lying on the opposite side to that on which the disease exists. Sometimes there is backache; occasionally there are sharp cutting pains in the vagina; usually there are radiating pains referred to the inside or outside of the thigh—as far as the knee—corresponding with the side on which the change in the Fallopian has arisen. Menorrhagia is the symptom, however, which is most likely to attract special attention; but, depending as it does upon an interference with the nerve-supply to the uterus, it seldom appears until the distension of the Fallopian tube has become marked, and the condition unmistakable. The hæmorrhage, as a rule, when once established, is more or less constant. Considering the close proximity of the pelvic viscera to each other, and the manner in which each derives its nerve-supply, it is not to be wondered at that change arising in one of these organs is more or less likely to affect and influence the functions of the others. In pyosalpingitis, there is usually a frequent desire to pass urine; but, during the time of hæmorrhage, the patient is likely to experience a difficulty in emptying the bladder, it being essential to strain much, and call into action those volitional muscles which may aid in the performance of the act. In some cases, there may even be complete retention of urine. It is but seldom that pain is complained of during the time of voiding urine; but, should the patient be obliged to retain the urine longer than usual, so that the bladder becomes somewhat distended, pain will be felt in the region of the diseased tube. Sometimes there is diarrhoea; occasionally there is pain before voiding motions, more especially if the rectum be loaded, and the disease exist in the left Fallopian tube, which may, by inflammatory adhesions, have become closely attached to the lower bowel. In a few cases, stiffness of the thighs is to be noted. In those cases in which there is frequent desire to pass urine, cold aggravates the condition.

CLINICAL MEMORANDA.

RELATIVE PROTECTION OF VACCINATION AND SMALL-POX.

FIVE years ago, I had occasion to revaccinate one hundred and fifty boys, between nine and fourteen years of age. On examining their arms on the eighth day, I noticed nine boys with primary vaccine vesicles, the same as if they had been vaccinated for the first time. On making inquiries into the cause, I found that each boy had been vaccinated when a baby, and had suffered from small-pox afterwards.
A. WYLIE, M.D., Clapham Road, S. W.

FATAL CONVULSIONS DUE TO ROUND WORMS.

By a strange coincidence, the mail brought me the JOURNAL of October 31st, containing an annotation on the above subject (p. 842), about three hours after I had made a medico-legal investigation of a similar case.

B., aged 3, a Hindu child, was suddenly seized with convulsions, at 4 A.M., on November 20th, 1885. The day before, she had been running about as usual, but would eat little or nothing. The arms were extended, and the mouth tightly closed. There was no vomiting. She died about 11 A.M. The necropsy at 1 P.M. showed some pleuritic adhesions at the right base, but the lung-substance was healthy. The stomach and small intestines were practically void of food, but the latter contained large quantities of common round worms, in some places to be felt or seen as lumps through the intestinal wall. The other viscera were healthy. The brain was carefully examined, as the child had received a blow on the head the week before, but no cerebral lesion could be found. By exclusion, therefore, the worms seemed necessarily to be regarded as the primary cause of death.

BEAVER RAKE, M.D. Lond.,
Government Medical Officer, Trinidad.

NOTES ON A CASE OF HÆMOPTYSIS.

THE perusal of the report of Dr. West's paper on hæmoptysis, read before the Medical Society on December 14th, has induced me to make this communication.

My patient was an active healthy looking man, aged 42. He commenced to spit up blood, without any warning, whilst sitting over a fire on the evening of November 29th. He spat up three or four ounces of bright frothy blood during the night, and the hæmoptysis continued next day, at intervals of a few hours, to the amount of five or six ounces each time. In the morning, I had ordered one-drachm doses of sulphate of magnesia every four hours, but in the evening I thought it right to make some further effort to arrest the hæmorrhage. Accordingly, I injected into the deltoid muscle seven and a half minims of a solution of Bonjean's ergotine (gr. j in ℥ 2½), and in a few minutes the hæmoptysis had quite ceased, and it did not return during the night. Next day, however, the blood-spitting recurred at intervals of a few hours. On two more occasions I injected ergotine, and each time the hæmoptysis ceased in a few minutes, and the interval between the attack and the next was prolonged. On the fifth day, as I could not be present often enough to control the hæmorrhage by repeated injections of ergotine, I determined to try the effect of hazeline, and ordered half-drachm doses every two hours as long as the hæmoptysis continued, and then every four hours. The man soon ceased to cough up blood, but when the frequency of the dose of hazeline was reduced, the hæmoptysis returned. Next evening, whilst the patient was spitting up blood in larger quantity than before, I repeated the dose of hazeline every hour as long as the blood-spitting continued, and when it had ceased, as it very soon did, the dose was taken every two hours for twenty-four hours. The patient continued to take hazeline, in gradually diminishing doses, for the next few days, and all that he spat up was a little dark phlegm.

My opinion of the power of ergotine in arresting hæmorrhage from the lungs is not based upon this case alone.

When I was house-physician at the Royal Chest Hospital, and at St. Bartholomew's, I used almost always to inject from three to five grains of ergotine in cases of serious hæmoptysis, and I can remember only one case (of very profuse hæmorrhage in a case of advanced phthisis) in which it failed to be of signal service in arresting the bleeding. It is advantageous to follow the injection by one of morphia in many cases, with a view of keeping the patient more completely at rest.

My experience of hazeline in the treatment of hæmoptysis is not so large, but in this case it certainly appeared to me to control the hæmorrhage in a remarkable manner in a short time, and I shall be glad if this communication shall lead to its more extended trial in similar cases.

ROBERT J. COLLYNS, Dulverton.