November 12th, 1881). Señor L. was directed, before his departure from England, to provide himself with a set of my nasal bougies, with which he was instructed to keep the passage as thoroughly dilated as possible.

THE CHOLERA COMMA-BACILLUS, AND ITS FUNCTIONS.

By G. F. DOWDESWELL, M.A., etc.

TOWARDS the end of last year, I obtained from Dr. Roux, of Paris, through the kind intervention of Dr. Maddox, cultivations of the so-called cholera comma-bacillus, with a view to ascertaining whether it possesses any specific action on the lower animals, as asserted by MM. Nicati and Rietsch, and others. I found the organism to be identically similar to that of preparations obtained from Dr. Koch, and its habit of growth in artificial cultivations, in all essential points, the same as described by him. In nutrient gelatine, of various composition—pepton, with and without sugar, *bouillon* of pork, beef, or rabbit, with or without the admixture of sodic chloride (Dr. Miguel's formula)—it grew in the typical manner described by Koch (*Deutsch. Med. Woch.*, 1884, p. 191, etc.); as readily, however, in neutral as in faintly alkaline media, whether liquid or solid, at temperatures ranging from 10° to 14° Cent., or at 35° to 37° Cent., beyond which I have not tried it. In different conditions of temperature and nutrition, the form and habit of growth of this organism are subject to greater modifications even than in the case of most others. The occurrence in certain conditions, of multiplication by longitudinal fission, as observed and described by Dr. Klein, is one of the most remarkable circumstances yet recorded in the morphology of the lower fungi, and would alone render this microbe worthy of further most careful study and observation, not only from a pathological, but from a biological, point of view, in respect to the weighty question of the constancy of species, and the occurrence of variations of form and function; these will be more fully described hereafter, in relation to the question whether its characters prove it to be specifically distinct from all other similar forms. It stains readily with the usual aniline dyes, both blues and reds.

Continuing the cultivations obtained from Paris through some "generations," as a first experiment, a rabbit and two guinea-pigs were injected, the former with three divisions (one division equals 0.09 cubic centimetres), and the latter with two each, of a Pravaz's syringe, of a liquefied gelatine-culture of the microbe, into the duodenum, a small incision being made in the abdominal wall to the right of the median line, and a loop of the intestine drawn out, under narcosis. The operation throughout was performed, under strict antiseptic precautions, by the Brown Professor of Pathology, Mr. Victor Horsley, whom I have to thank for the facilities and aid he has afforded me in these investigations. The following day, all the animals were feeding well, and apparently unaffected; the rectal temperature in the rabbit and one guinea-pig was unaltered; in the other guinea-pig, it had risen about 1.0° Fahr., to 102.2°, and, on the second morning, in this animal, to 103.0° Fahr., from which it fell on the following day to the normal, the two other animals remaining unaffected in any way, while kept under close observation for fourteen days; in each case, the wound in the abdominal wall healed by first intention, and produced no inconvenience in any respect.

The next experiment, some days subsequently, was likewise upon a rabbit and two guinea-pigs, using about the same quantities of an active recent cultivation of the microbe, the operation being performed by Mr. Horsley in a similar manner to the former. In this case, there was no appreciable variation in temperature, or other disturbance, in any one of the animals used, and the operation seemed in no wise to have deranged them after the effects of the anæsthetic had passed off.

After four days, one of the guinea-pigs, which had continued perfectly unaffected, was killed, and the intestine examined. The site of puncture was perceptible in the duodenum, and there was a slight hyperæmia of the muscular coat in its vicinity; the mucous membrane was healthy, and the contents of the intestine quite normal, the feces, in the lower part, being as firm as usual. The other guinea-pig and the rabbit remained perfectly unaffected as long as observed. After this, a terrier dog was, in similar manner, under narcosis, injected into the ileum with three divisions of a syringeful of

a cultivation; the effects of the ether used were somewhat prolonged. The next morning the animal was dull, and fed but little. The rectal temperature was 103.2° Fahr., but on the second morning it was again normal, the animal feeding as usual, and unaffected in any way, as it remained till ultimately killed. Subsequently, the injection was repeated in another dog in similar manner, and also in a rabbit, each receiving two divisions of a syringe of an active cultivation. Both these animals recovered rapidly from the effects of the operation, and the next morning were as well in all respects as previously. The rabbit was then killed and examined; the puncture of the needle was not perceptible, but around its site the outer coat of the intestine was slightly hyperæmic; beyond this, nothing was to be observed. The feces were perfectly normal, and firm in the lower intestine. The dog remained unaffected in any way.

In these injections, the cultivations employed were liquefied gelatine. In each case they were examined microscopically, and found to be pure and active; their purity, too, was readily shown by their macroscopic characters; they were of different ages, four days, fourteen days, and one month.

An injection, in similar manner, was also made in another guinea-pig, but the animal never recovered the effects of the anæsthetic and the shock, and died within about four hours. On examination, pronounced peritonitis was observed, with a considerable effusion of serum with numerous red blood-corpuscles; the fecal matter was normal and firm as usual. A further experiment was made by feeding the dog last mentioned and two guinea-pigs with liquefied gelatine-cultivations of the microbe, the dog receiving about one cubic centimetre of it, and the guinea-pigs each about half that quantity. Twenty-four hours afterwards, one guinea-pig was killed, and the stomach and intestine minutely examined. Neither in the stomach nor in the duodenum, the reaction of which was neutral, was a single comma-bacillus to be found, and in the jejunum and ileum only one or two here and there, but very rare. Amongst the large numbers of septic organisms, in different spots of the large intestine, it was the same thing, one or two here and there, but never more. The next day, forty-eight hours after they had been fed with the cultivations, the other guinea-pig and the dog were killed, and examined in a similar manner, but in neither was a single comma to be found.

In another experiment, two guinea-pigs, both starved for twelve hours previously, received each by injection into the duodenum 0.10 cubic centimetre of an active cultivation of the microbe. No. 1 never recovered the effects of the operation, was dying in the evening, and found dead the next morning. On examination, the stomach and small intestine were found devoid of food, and only some feces, which were quite normal, in the lower bowel; the puncture of the needle in the duodenum was not perceptible, its outer coat was somewhat inflamed, with considerable desquamation of the mucous membrane; its reaction was distinctly alkaline. Examined by the microscope, a few commas were observed here and there, but not in any number; only one or two in several fields of view, and none in the jejunum or ileum. The animal had evidently died from the effects of the operation and exhaustion; but even under these conditions of depression of the vital powers, the microbe had not developed to any extent. The other animal operated upon recovered, though slowly, and by the evening was running about and feeding. The next morning, it was killed; the incision in the abdomen had not healed as usual, the dressing was displaced somewhat, apparently by the animal scratching at it; the duodenum was slightly inflamed externally, but the mucous membrane was healthy; the puncture of the needle was not perceptible. In the stomach was a little partially digested food, but the animal did not appear to have fed as much as usual. Neither in the duodenum, the reaction of which was faintly alkaline, nor in the rest of the small intestine, could a single comma be observed with the microscope amongst the other microbes.¹ The contents of the intestine were perfectly normal.

The result of these experiments appears to show clearly that the microbe here in question is not pathogenic when injected directly into the small intestine of the animals here employed, and with the results of feeding, raise a presumption that it does not find a congenial soil for development in this situation. It is, too, well known to all who have resided in India, that dogs and pigs at least, are not susceptible to this disease by feeding on choleraic excreta; for, throughout the native towns and villages, in the absence of any latrines and sewers, these animals are the principal scavengers, but possess perfect

¹ There is often to be met with, in the intestines of these animals, a comparatively large curved cell, apparently fungoid, but two or three times the size of Koch's commas, less thick in proportion, and more acuminate; it cannot be confounded with the latter; their occurrence, in moderate numbers—five, ten or more in a field, is frequent but not universal.

immunity from any choleraic symptoms. The circumstance, however, that the microbe will not multiply in the intestines of some of the lower animals, and their immunity from cholera, together with the occurrence of the organism so largely in cholera-cases in man, may tend to support the view that it bears a relation to the disease in man, and that the lower animals owe their immunity from it to the inability of the microbe to develop in them.

In no case was there any fall in temperature whatever, nor, excepting a slight nausea in the one dog, and the death of the two guinea-pigs as recorded, was any appreciable effect apparent after recovery from the operations, which, as above stated, were performed by Mr. Horsley, antiseptically; and it is remarkable that a series of such serious operations as taking out a loop of the intestine and injecting it, should have been performed in these cases without any inconvenient results, and is a striking instance of the immense value of the antiseptic system of surgery. Everyone who has used rabbits for purposes of experiment knows how liable these animals are to septicæmia after operations—even the slightest—performed in the usual manner, but which in these cases were without any appreciable effect. This raises the strong presumption that, in those cases where, as stated by other observers, but without details of the method of operation, they have been followed by so-called choleraic symptoms, this has really been due to septicæmia, for it would probably be quite impossible to perform any number of such operations upon rabbits, omitting antiseptic methods, without fatal results.

I had previously attempted to induce catarrh of the intestine in the animals to be experimented upon by drugs, but I found that, with rodents especially, their action was irregular, and would greatly complicate the results of the experiment.

With regard to the statement of MM. Nicati and Rietsch (*Comptes Rendus*, tome xcix, 1884, p. 928), that cultivations of the microbe emit the distinctive odour, somewhat ethereal and not unpleasant, of cholera-evacuations, I have not been able to detect, in cultivations of any age, in whatever medium, any marked or distinctive fetor or odour, if, indeed, any such exists in the evacuations referred to.

With respect to this microbe being specifically distinct from all other forms, it is now amply shown that many identically similar, morphologically, occur in various situations, but it is asserted that their habits of growth are different and distinctive. This, however, is very doubtful. This character is as variable, under slightly different conditions, as are the form-phases of these organisms; the comma-bacillus, indeed, grows in the habit exactly described by Koch under certain conditions, that is, in nutrient gelatine at a given temperature; in other slightly altered conditions, its habit is widely different, and, if it be regarded, as shown in the case of Finkel and Prior's microbe, that the organisms are specifically distinct, it is not so with some similar forms that occur elsewhere; nor, indeed, were this done, would it of itself suffice, by much, to prove that it does constitute the actual contagium of cholera; for, judged by the canons very clearly laid down by Koch himself (*Wundinfektionskrankheiten*, etc., p. 22), much beyond this is wanting to justify this conclusion. Koch's reports, indeed, raised a strong presumption in favour of its having a causal relation to the disease in which it had been observed, but the subsequent observations of the English Cholera Commission have greatly shaken this; and Dr. Klein's demonstration of cultivations of the comma-bacillus from the mouth, first noticed by Dr. T. Lewis, goes a long way towards disproving this, and has shown that the habits of growth of the two organisms are so very similar, that they can scarcely constitute a specific difference.

Its invariable occurrence in cholera cases is stated alike by both, but with respect to the numbers in which it is found, and that more especially in the most typical cases, the observations of the two are directly at variance, as is also the case with the most important point of their occurrence within the tissues of the intestine. Indeed, judged by the experience and explicit statement of Dr. Klein, the minute straight bacillus, which he has observed and described, has fully as good a title to be considered as constituting the virus as has the comma-bacillus. It cannot, indeed, be asserted that an organism, which is not proved to be pathogenic to the lower animals, may not, or does not, constitute the contagium in man; for, if this were admitted, we should be precluded from forming any final conclusion upon the etiology of such diseases as admittedly are not communicable to the lower animals. Nevertheless, in such cases, the difficulty of the investigation is increased, and more stringent proof that it fulfils the other requisite conditions is necessary.

That the comma-bacillus does not convey infection to man by inhalation, appears to follow from an experiment I have recently unconsciously made upon myself. In separating some other microbes by

fractional cultivation in gelatine, I found that I had accidentally got a growth of typical comma-bacilli, as far as shown by the characters of the colonies, on the surface of the gelatine. This could only have arisen from atmospheric contamination, as the capsules and vessels employed were guileless of previous cultivations, and the only instruments used were capillary pipettes, freshly drawn out. This occurred in the room of the laboratory in which I had been sitting several hours daily for some weeks past; and if present in the atmosphere in sufficient numbers to contaminate a cultivation contained in a small capsule, not exposed frequently, or for long together, it appears impossible that I, or others frequenting the room, could have avoided the introduction of some of these organisms into the respiratory passages, in whatever form they may have existed in the air.

To Dr. Koch belongs the eminent merit of having observed and described the comma-bacillus, which had hitherto escaped the notice of the numerous previous workers at this subject, and which, from their numbers and apparently invariable occurrence in this disease, deserves the most careful attention as one of its symptoms. His own investigations raised a strong *prima facie* case that they did constitute the active contagium. This conclusion has not been confirmed by subsequent observation; neither, on the other hand, is its possibility finally disproved, and the nature of the virus must be regarded as still quite undetermined. It may perhaps be, as stated by Koch in his second official report from Egypt (*Deut. Med. Woch.*, 1883, No. 42, p. 616), that though the microbe bears some relation to the disease, it is not its actual cause. Any further opinion upon this point must rest, for the present, upon the final determination of the question whether the microbe is, or is not, specifically distinct from all other similar forms.

Beyond this question of the action of any particular organism or substance, lies the primary one, whether cholera is in any sense a communicable disease; this is yet by no means finally settled, and there are many circumstances patent to all residents in India, which must render this somewhat doubtful. Foremost amongst these is the state of the tanks of drinking-water, which has been referred to in connection with the occurrence in them of the microbe here in question; contamination of these with choleraic matter during the prevalence of the disease must invariably occur largely through the habits of the natives, more especially the Hindoos, both by bathing and constantly washing their linen in them; and were the disease essentially capable of propagation through the excreta, as frequently asserted and generally believed, the epidemic must spread much more rapidly and widely than often occurs. This consideration applies more strongly to the case of a figured or organic ferment, capable of reproduction and multiplication, than to a chemical or soluble one, which may be modified by large dilution; and this circumstance may tend to account for the discrepancy in the positive statements that have been made as to the occurrence of infection from choleraic matter in some cases, and the contrary.

This investigation was made at the Brown Institution, Wandsworth Road.

ON THE FIRST DISCOVERY OF THE COMMA-BACILLUS OF CHOLERA.

Read at a Meeting of the Royal Microscopical Society, March 11th, 1885.

By FRANCIS FOWKE, F.R.M.S.

LIKE many microscopists, being most deeply interested in the recent researches relating to the cholera-bacillus called the "comma-bacillus," by that eminent investigator Dr. R. Koch, of Berlin, it occurred to me to examine the current medical literature of the last epidemic of the disease in this country in 1849.

In the pages of the *Provincial Medical and Surgical Journal* and other periodicals for that year I found such evidence in the descriptions and corroborative woodcuts furnished by Drs. Brittan and Swayne, in their original and independent investigations, that I now venture to claim for them the priority in discovery of the cholera bacillus.

In laying before you some of the quotations and statements from various medical journals of that date in support of my views, I entirely disclaim all intention of impugning the originality and independence of Dr. Koch's recent discovery and researches upon this subject.

In the volume of the *Provincial Medical and Surgical Journal* for 1849, on page 600, will be found a report of a Subcommittee appointed by the Bristol Medico-Chirurgical Society to investigate the nature of cholera by means of microscopic observations. The Subcommittee consisted of Mr. James F. Bernard (Chairman), Mr. J. C. Swayne, Dr. Frederick Brittan, Dr. J. G. Swayne, Mr. Augustin Prichard, Dr. William Budd, Dr. J. A. Symonds, and Mr. John Cash Nield (Secre-