ance of it by the patient. In my last case, reinversion was accomplished without the tapes being touched after their first adjustment.

Reduction takes place by the cervical method. Newnham advised this mode of reinversion in 1818. He said: "It is wisest and best to endeavour to reinvert the uterus by returning first that portion of it which was last expelled from the os uteri." Pressing on the fundus causes counter vaginal traction on the cervix, making it unroll gradually until the internal os is reached, where a little delay is caused by its being less dilatible. When this point is passed, the body of the uterus soon opens, and admits the cup. The last step must take place rather suddenly, for all patients say they feel that something has "given way," and comparative comfort is the result.

When the inversion has been reduced, the sooner the cup is withdrawn the better, for the cervix immediately begins to close round the rod or stem, and the cup becomes firmly grasped in the uterine cavity. The best way of removing the little cup, is to tilt it on end, and bring it through the os as you would a button through a button-hole. If it have been long retained, an anæsthetic will assist. It might be a good plan to have a hole made through the edge of the cup, through which a stout string might be passed, by which to make traction. Continuous elastic traction on this string would remove the most firmly grasped cup with certainty. The stem might be previously unscrewed from the cup.

When the cup has been removed, pass a thick sound into the uterus; and, by pressing the point of it forward, the rounded fundus will be felt through the abdominal walls. Being satisfied that complete reinversion has taken place, syringe out the uterine cavity with iodine water at 120° Fahr., which will cleanse its surface and make the whole organ contract.

I think, gentlemen, after considering these facts, you will come to the conclusion that every case of chronic inversion of the uterus can be cured by sustained elastic pressure exercised in the right direction; and I hope you will not think me too sanguine when I state my belief that the mutilation of a woman, by removing her uterus, will no longer be necessary in consequence of the impossibility of replacing this important organ when inverted.

AN ADDRESS ON CHOLERA.

Read before the Bath and Bristol Branch.

By W. B. BEATSON, M.D.,
Retired Surgeon-General, Indian Army.

MR. PRESIDENT AND GENTLEMEN,—When I was invited to read before you, on this occasion, a short paper calculated to give rise to a discussion on cholera, it was probably expected that I would offer for your consideration some new ideas on the etiology and treatment of that disorder, derived from my own personal experience and observation. If such an expectation have been entertained, I am afraid I must disappoint it by saying that, although I am an old Indian medical officer, although I have lived and practised during thirty years in parts of India where cholera was either endemic and ever present, occasional in violent epidemic outbursts, or conspicuous only by its absence; although I have seen and treated many cases of cholera, and have had it myself, I should never have arrived at any kind of conclusion as to the nature, prevention, and treatment of the disease, if my study of it had been bounded by the circumference of my own observation. The study of cholera can be pursued to advantage in no contracted field, nor by the light of any one man's researches; and the whole truth concerning it will, if ever brought to light, be the result of many investigations yet unmade.

It is now nearly five years since I have had anything to do with cholera, my last experience having been the outbreak among British troops which occurred in 1881 at Meer Meer, while I was Deputy Surgeon-General of the Lahore Division of the Army in India. I should but waste your time, were I now to give you a detailed history of that outbreak; I should do worse than waste your time, for I should lead you to look at one aspect only of a question which has many sides, and send you all away each one only convinced of the truth of his own preconceived opinions. This would be a very regrettable result, because we are all too much in the habit of blindly adopting the views of respected authorities, of framing theories and

adjusting facts to support them; of quarrelling with other people who decline to accept our conclusions; of becoming, in short, partisans in pathology. Should this be considered an unjustifiable assertion, let me ask you to consider the present attitude towards each other of those members of our profession who have made cholera their special study, and the method of argument by which some of them endeavour to support their own particular views.

The nosologists of cholera may be broadly divided into two camps, I might say hostile camps; considering, to use the words of one of the latest writers on cholera, the warmth of spirit and vigorous intolerance which have been too often displayed by the propounders and advocates of certain theories.

The first and most influential camp contains all the leaders who believe that cholera is caused by a specific virus, which, obtaining entrance to the body from without, multiplies itself within, and is capable, when thrown off in the shape of exhalations and discharges, of causing an extension of the malady, so as to give rise to epidemic outbreaks. Of this form of belief, there are several phases. Some authorities think that cholera is directly communicable from person to person, by contact with the sick, or with their recent dejecta; some, that the fresh dejecta contain no poison until they have undergone changes dependent upon the media into which they pass; others, that the dejecta contain no poison, either actually or potentially, but that the virus is a kind of ferment, produced by an organism altogether extraneous to the body of the patient, and created by the organism only under certain favourable circumstances, seasonal and local.

All or most believe that cholera is largely dependent upon, and follows the chief lines of, human intercourse; that it is conveyed from one part of the world to another by travellers, sick or healthy, either as a *contagium vivum*, or by means of contaminated clothing or other fomites; that each epidemic is an importation from some infected locality; that cholera never arises spontaneously anywhere, and that some part of India is the centre from which it is diffused.

The second camp consists of all who doubt or deny the existence of a specific cholera-poison or cholera-producing organism, and believe that the disease depends on no definite entity, but is due to local influences, miasmatic, telluric, or atmospheric, the nature of which is at present undetermined. Of necessity, they disbelieve in the diffusion of cholera by human intercourse, by water-carriage, or by infected articles of food and fomites. They hold that successive epidemics have no relation to one another, and that the attempts often made to stay the spread of the disease by quarantine regulations, are generally either utterly useless, or do harm rather than good.

I have thus put before you the salient points of difference between the contagionist and non-contagionist parties. I do not appear as advocate or defender of either side. I think it is possible that each may be right to a certain extent, and that each is wrong only when it claims exclusive possession of the truth.

There are probably many in this assembly who hold decided opinions on the points in dispute. I ask them all to come to the discussion with open minds, to lay aside all preconceived ideas, and to determine, as far as possible, on which side lies the greatest amount of truth.

It must, I think, be admitted that all contagionist theories are imperfect deductions; because, while they are founded on fact, they are supported by partial considerations of evidence, all evidence in their favour being brought forward by their upholders, all that is contrary being subdued or explained away. The one fact on which all contagionist theories are founded is the undeniable one, that there are some diseases that depend on a poison or specific virus which is communicable by contagion or infection. It is a reasonable induction that cholera may be such a disease, but it is not a proven fact. The pronounced contagionist assumes that it is a fact, and uses it as a foundation for his theory. He then produces evidence which goes to establish the truth of his doctrine, and considers that his case is proved. The result is very convincing till the other side of the question is considered, and the discovery made that while every theory is supported by a large amount of evidence, direct and circumstantial, it is opposed, as an impartial authority has observed, by a considerable number of obstinate facts. I will briefly state some of these theories, and a few of the opposing facts. It is asserted that cholera arises in India, and is carried over the world by means of human intercourse; but the apparent progress of cholera is from east to west, while human intercourse proceeds in all directions. When cholera breaks out at a great Indian fair, or concourse of pilgrims, and those pilgrims return to their homes, they do not spread the disease wherever they go; it accompanies them only in certain directions. When cholera appears in the cantonments of a British regiment in India, the regiment is at

once moved into a camp pitched in a district known to be generally free from cholera, and the disease ceases; if the regiment be prematurely recalled to its former quarters, the disease returns. Again, epidemic cholera is said to be always an importation; but cholera is often imported by the arrival of a sick person in a healthy district, without any epidemic result.

What are called sporadic cases frequently occur, which cannot be traced to importation, and are followed by no extension of the disease. Further, for some years past, means of importation have been greatly increased by the extension of the railway system in India, and by the connection of Europe with Asia by the Suez Canal. Cholera ought, therefore, to be more generally diffused in India, more frequent in Europe, than it was formerly; but it is not so.

Cholera has been said to be water-borne by rivers in India; but, when this theory was investigated by the light of facts, it was found that, in the outbreaks reported on, the apparent progress of the disease had been in many instances against stream.

Quarantine should, on the supposition that cholera is portable, be valuable as an excluding agent; but facts are against it. It has over and over again failed to stop the spread of the disease, and it is now very generally allowed to be useless. I do not, however, lay great stress upon this, because perfect quarantine is an impossibility.

As to the theory that cholera is directly communicable by contagion or infection, as small-pox, scarlatina, etc., are communicable, a great opposing fact is, that the attendants on the sick in cholera-hospitals, and those engaged in dealing with cholera-dejecta, are by no means frequently attacked by the disease. They are, of course, sometimes attacked, but not in greater proportion than are other persons in the same place not so employed.

I now come to a theory that there is a poison-virus or specific contagium, which causes cholera to be set up in the human system when taken into it. The great fact against this theory is, that no such entity has ever been proved to exist. We can carry small-pox on the point of a lancet, because we can always find it in the small-pox pustule. We cannot so carry cholera, because we do not know where to obtain it. It is, apparently, not contained in the excreta, notwithstanding the supposed proof that it is so, furnished by the celebrated Broad Street pump, and the accident reported by Macnamara, in which nineteen persons partook of water mingled with cholera-dejecta. In the first case it is obvious that, before cholera-dejecta could obtain entrance into the well, cholera-influences must have already existed in the neighbourhood, and that if the water were really contaminated with cholera-dejecta—which is not a certainty—it was also charged with other matters likely to be prejudicial to health. In the second case, in which nineteen persons in India partook of what may really be called cholera mixture, and five subsequently died at varying periods within seventy-two hours, it must be remembered that all were living in the presence of cholera-influences, and that fourteen out of the nineteen escaped without diarrhoea, cholera, or the slightest *malaise*. This experience chiefly proves that cholera-dejecta may be swallowed with impunity; the positive results in death are inconclusive, because, cholera being present at the time, the infection might have occurred in other ways.

An endeavour has lately been made by Dr. Koch of Berlin to prove that the cause of cholera is bacterial. In the course of a series of experiments conducted in India, Egypt, and Europe, Dr. Koch discovered a comma-shaped bacillus in the intestines of cholera subjects, and in the waters of places infected with cholera.

Now, to establish a bacterium as the cause of a disease, four conditions appear to be necessary. The bacterium must be found in the blood, or in the organic tissues of the subject, in connection with one specific disease and with no other; it must be a distinct organism, not springing from or convertible into another; and it must be capable of setting up its peculiar action of disease when transplanted. The comma-bacillus of Koch was found by him, not in the blood or organs, but in the dejecta, which are the last product of the choleraic process. It has been found in various morbid conditions, and in normal secretions, by other observers who doubt its true specific character. Pure cultivations of it have been swallowed without effect, by several experimentalists. Attempts to engraft cholera on human subjects, being inadmissible, have not, those of Dr. Ferran in Spain excepted, been made; on animals they seem to have generally failed, or to have ended in death from complicating causes. The comma-bacillus is believed to be destroyed, or retarded in development, by the acid secretion of the stomach. In experimenting on guinea-pigs, therefore, it has been found necessary to open the abdomen and inject the bacilli into the duodenum; sometimes to tie the ductus communis choledochus; sometimes to inject soda-solutions into the stomach, or alcohol, chloral, morphine, atropine, or opium, into the peritoneum, or

subcutaneously, in order to paralyse peristaltic action. Many guinea-pigs have died after being subjected to such proceedings, but that they died of cholera may be doubted.

Dr. Koch himself remarks that the better the operations were performed, and the less extensive the manipulations, so much the less chance was there of the animals dying of cholera. Of eighteen treated by opening the abdomen and injecting the bacilli into the duodenum, thirteen died, of cholera of course. Of another set of nineteen simply fed with cholera-bacilli, guarded by alkaline solutions, only one died of cholera; and it was found, on *post mortem* examination, that this one had aborted immediately before infection. In the cases of animals so killed, the small intestine is found markedly reddened, and full of watery flaky fluid.

Such dejecta constitute one symptom only of cholera in the human being; do they, therefore, when formed in the intestine of the guinea-pig, prove its death by cholera? Should they not rather be interpreted as the results of intra-abdominal inflammation, excited by operative procedure?

Time will not allow me to say much more on the comma-bacillus; but, with regard to it, I ask you to notice the following points. One pathologist believes that he has established the bacillus as the true cause of cholera. His views are opposed by other observers, his facts are disputed or denied, and he replies that no other result could be expected; that his adversaries went to their work with predetermined conclusions, and judgment already decided. In effect, if not in words, he says that they did not know the things for which they had to look, and misinterpreted those which they found; and, lastly, that almost all their evidence has been overthrown by the unanswerable objections of another observer.

I desire to speak of the learned German professor and of his researches with the highest respect. I do not, of course, say that these are all the replies given by Dr. Koch to his opponents; I merely adduce some of them in proof of the controversial method by which the warfare of the two hostile camps is maintained.

Now, if you have followed my argument, I think you will be disposed to admit that there are good reasons against our enlisting under either the contagionist or the non-contagionist flag, and that we shall be wise if we stand aside for the present, and ask ourselves if there cannot be found a *modus vivendi* between the two parties, and a path along which they may march shoulder to shoulder, and in peace.

Such a path, I think, we may find if we incline a little to the party which doubts the existence of cholera as a separate entity, and hold in abeyance the belief in a specific cholera or poison-producing germ. We are, I think, too apt to look upon our diseases as self-existent entities, rather than as parts of ourselves. Cholera is so generally regarded as an entity, that it has acquired almost a personality. Cholera has a home; it dwells in Eastern Bengal; it takes its journeys, and marches across the world as a destroying angel; it slumbers, and it wakes again. Other diseases are similarly regarded as entities; but surely there are some which are merely perverted actions of the human organism having a tendency towards death; and is not cholera one of these? Surely there was a time when primeval man walked upon the earth in a state of savage health; when there was no overcrowding, no accumulation of filth, to produce epidemic disease. But, as man began to increase and multiply, and build cities to dwell in, conditions inimical to health arose, and specific diseases became developed, as the result, not of one, but of many, concomitant causes. May we not suppose that in such manner have arisen small-pox, leprosy, the sweating sickness, the black death, the plague, and, in later times, cholera? May we not also suppose that disease, which once originated in unwholesome surroundings, may again do so *de novo*? May we not thus account for sporadic cases of cholera, and for the epidemics which originate apparently without importation?

The acceptance of such views as these does not in any way oblige us to renounce belief in the production of a non-specific communicable poison or infection as the result of choleraic action. For it is quite conceivable that, under derangement of the vital chemistry of an organism, poisons of the nature of ptomaines, varying in kind and differing in degree of intensity, may be produced instead of healthy secretions.

Under such a supposition, I submit that the opposing facts of the general immunity of cholera-attendants, and of the well established instances in which the handling of linen soiled with cholera-dejecta has been shortly followed by the development of the disease, may be fairly reconciled. Further, if cholera is the outcome of many causes variously combined, one of them being such an uncertain quantity as the health-condition of the individual in which the disease originates, there is no longer reason to be astonished at its sometimes appearing, sometimes not appearing, under circumstances which do not seem to

vary from year to year, or at the search for a specific cause being always unattended by success.

Now, these are, I think, some very good reasons for inclining to non-contagionist doctrines, but there is yet one of still greater importance. It has, I know, been said that those of us who cannot altogether accept contagionist views desire to crush public opinion, to discourage investigation, to do nothing against cholera itself, and to keep international boards in ignorance, lest they should become troublesome by enforcing quarantine-regulations on travellers. The views I have suggested to you for consideration this evening have been described as nihilistic. I maintain that they are the very reverse. That which renders them most important is that they take us out of settled grooves of thought, and point directly to the only means we really possess of preventing epidemics of cholera, and of finally stamping out the disease.

And what is the method by which we are to attain an ending so desirable? Everyone, contagionist and non-contagionist, must reply, the maintenance of public health at its highest point by attention to the sanitary surroundings of individuals. The contagionist regards filth as the nidus in which the imported germ or poison festers; the non-contagionist, as one of the causes which, in conjunction with others, deteriorates health, and tends to originate in the human system that perverted action which we call cholera. Which is the wiser—to stand on guard against a foe that after all may not be coming, or to destroy the enemy already established within our gates?

During the last three years, cholera has appeared in Egypt and in Europe, imported, some say, from India. But in Egypt it began at Damietta, which is not in direct communication with India; in Europe, at places noted for defective sanitation. A thousand have fallen beside us, and ten thousand at our right hand, but it has not come nigh to us in England; because, it is said, of our careful attention of late years to sanitation. But have we done, and are we doing, all that should be done? We have a Pollution of Rivers Act and a Public Health Act, and we have local sanitary bodies to carry out the provisions of such Acts. Yet it is only lately we have heard of a river near London, once a pleasant stream, now a noxious sewer. Is this a solitary instance? Is the Lea the only filthy river in England, and is the Avon pure? Our sanitary authorities no doubt do their duty in establishing systems of town-drainage; but do they give sufficient attention to house-sanitation? Are they always ready to remedy defects brought to light by private individuals? Do they assist or thwart the endeavours of the sanitary associations which have lately been established with such good effect in some of our cities?

These are questions which you may well discuss, for they concern the future health of England. It may be said that sanitation is well attended to amongst us, that filth-diseases are less prevalent than they were, and that the average duration of human life is increasing. All this is true, but still we have rivers polluted with sewage; and in all old cities, such as Bath and Bristol, houses made unsanitary by imperfect drainage. It may be said that these things do not of themselves cause cholera, and that cholera has not been epidemic in England for years. Well, admit they do not cause cholera; but can it be doubted that they conduce towards it; that, other causes being added, the disease is likely to arise where they exist, and that they are evils which are yearly being added to by an increasing population? And has there really been no cholera in England for years? Do we never hear of cases of cholera called "*nostras*," and not Asiatic, because they do not present all the symptoms of complete and fatal cholera given in our text-books? I confess that I never hear of cholera *nostras* without thinking of the cases which commonly precede epidemic outbreaks in India, and are there called "*suspicious*." One of the last lessons I learnt in India was, that the suspicions aroused by such cases were very generally confirmed; and I have come to think that the difference between cholera *nostras* and cholera Asiatica is one of degree rather than of kind.

Now, this difference in name between cholera *nostras* and cholera Asiatica, or between cholera Asiatica and a suspicious case, is, I think, one of great practical importance. It is especially so in India, where the lives of hundred of soldiers hang upon the question of removal from quarters into camp. A suspicious case is one which presents some, only, of the symptoms of cholera, and does not perhaps end in death. Going into camp is to be avoided, if possible, because it is not only costly, but attended by its own special dangers of insolation, fever, and other diseases incidental to exposure to overfatigue, extreme heat, heavy rain and chill. Time is lost in discussing the suspicious cases, and lives are sacrificed because the move is not made soon enough. In England the case is different; we cannot move the population of a city, but those who are not called upon by duty to remain, may, if they desire to secure their own safety, depart. It is well,

perhaps, that we should recognise cholera as "*nostras*," as a product of our own life habits; but not that we should call it "*Asiatica*," for that may imply that it comes to us only from without, and through the faults of other people. I ask you, therefore, whether it would not be well to reconsider the definition of cholera usually given in standard works on medicine.

Such definitions are too long for me to quote now, and I will only say that the definition of a disease ceases to be a definition when it is made to include a statement of a theory, the etiology, and semeiology of the disease and the history of a fatal case. The Royal College of Physicians has more concisely defined cholera as "*an epidemic disease, characterised by vomiting and purging, with evacuations like rice-water, accompanied by cramps, and resulting in suppression of urine and collapse*." But cholera is not always epidemic, is not invariably accompanied by all the symptoms detailed, and does not always result in suppression of urine and collapse. Such being the case, I would venture to suggest this amended definition: Cholera, a morbid action of the human system usually characterised by *malaise*, purging and vomiting of fluid, usually like rice-water, and having a tendency towards collapse and death.

I claim for this definition the virtue of simplicity. It is a reduction of a complex statement to its lowest terms. It declares no theory, it deals only with facts; it furnishes, I think, a complete key to the rational treatment of a patient threatened by or suffering from cholera—a key which may be used not only by a physician, but by any person of ordinary intelligence.

Some months ago, when the approach of cholera was dreaded in England, I was asked by a lady, living in an isolated part of the country, what she ought to do in the event of any of her household being attacked by the disease. Such a question is often asked, and not always satisfactorily answered, because the person consulted is wedded to a theory, or is perhaps polygamous in the matter of theories, and is wedded to so many, that he does not know which to favour most, or what kind of special treatment is really the best to recommend; but surely there is a plain practical answer.

The treatment of cholera is simple, if, bearing in mind and following my definition, we lay aside all theories, and confine ourselves to the one fact before us—the condition of the patient to whom we are called. We need not think about causes, when we have to deal with cases. Recurrence to theories will land us only in confusion and uncertainty. Special methods of treatment founded upon them are fallacious, and may do harm instead of good; all have been tried over and over again, and found wanting; all fancied specifics have failed.

The condition of the patient will be either one of *malaise*, or of collapse. In the first, his most urgent symptom will be profuse diarrhoea; in the second, this will have been succeeded by purging and vomiting of quantities of serous fluid, in consequence of which his blood has become so altered in quality that it circulates with difficulty, and no longer maintains the nutrition of the body.

Now, if there be any one point in the treatment of cholera on which a consensus of opinion may be said to exist, it is that, when cholera-influences are abroad, diarrhoea should always, if possible, be stopped. Diarrhoea may be preceded, perhaps, by many days of *malaise* and indigestion. These symptoms should be treated by careful regulation of the diet, by tonics, perhaps by the occasional administration of rhubarb and magnesia, combined with compound spirit of ammonia and aromatic tinctures; but aperients of any kind should be used with the utmost caution, and diarrhoea tending to be profuse and exhausting should be always stopped, if possible, by astringents and opiates, combined with warm stimulants. Nothing can be better for the purpose than the ordinary compound chalk mixture so combined, or a mixture of sulphur in ether-spirit of ammonia, and the tinctures of opium and catechu in equal parts. This is a most convenient preparation to carry about, and may be given in doses of one fluid drachm or less, according to age and the requirements of the case. If you thus treat cases of threatening cholera, keep them warm in bed, and do not neglect to nourish them with proper amounts of soup and stimulants, you will save many that would, if neglected, go on to a fatal issue. When the condition is that of collapse, all our efforts must be directed to counteract the tendency towards death. We must do our best to restore warmth, to promote the circulation of the blood and the nutrition of the body, to alleviate thirst, and to allay pain, cramp, and restlessness. Cover the patient with blankets, place him in bed near a fire, rub the body and limbs with dry ginger powder; give frequently, in small quantities, milk or milk and soda-water and good beef-tea. Do not deny water to quench thirst, but do not give it in large quantities, for fear of promoting vomiting. To relieve cramp, administer chloroform by inhalation, but do not carry it to full extent. Do not add alcoholic poison to the already

vitiated blood, by giving quantities of brandy, or paralyse the too fast failing heart with opium.

Attempts are made by some to check the serous purging and vomiting by the internal administration of astringents, such as acetate of lead and nitrate of silver. I do not believe that these substances have any power in this respect, or that they can be employed without interfering with the restorative treatment of which I have indicated the outlines, and by which alone we may hope sometimes to achieve success. Similar objections lie against all forms of medicinal treatment, whether mercurial, alkaline, acid, saline, or specific. In cholera-collapse, there is a tendency towards death from progressive disorganisation of the blood, and that we can only hope to overcome by supporting the powers of life.

THE REACTION OF THE GASTRIC SECRETION, AND ITS RELATION TO CHOLERA-INFECTION.

By MATTHEW HAY, M.D.,

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IMMEDIATELY previous to Dr. E. Klein's departure to India in 1884, for the purpose of studying the genesis of cholera, a letter, signed E. K., appeared in *Nature* (July 17th, 1884), in which it was attempted to prove that Koch's statements relative to cholera were "in hopeless contradiction." The writer sums up these in three propositions: "(1) that the 'comma-shaped' bacillus is the cause of cholera; (2) that the alimentary canal is the exclusive organ of entrance of the cholera-virus; and (3) that the 'comma-shaped' bacillus is neutralised and killed by acid." He then proceeds to remark that "the first two propositions are assumptions, the third is based on direct experiment, and is perfectly in harmony with other observations. If, then, this third proposition be true, the other two cannot be true; that is to say, if it is true—and there can be no doubt about it—that the 'comma-shaped' bacillus succumbs to the action of acid, then it cannot be true that the 'comma-shaped' bacillus is the cholera-virus, nor that the alimentary canal is the sole entrance of the cholera-virus. How, we may ask, can the 'comma-shaped' bacillus pass unscathed the acid contents and the acid secretion of the stomach? To maintain, as Koch is reported to have done, that in all persons attacked by cholera the stomach must have been previously so deranged that its contents and secretions are not acid, must appear to everyone who has had any experience during a cholera-epidemic an untenable proposition. On the one hand, it is known that such a serious disorder of the gastric mucous membrane as the total absence of acidity is of comparatively rare occurrence, while, on the other, in every cholera-epidemic, numbers of persons become affected with the disease in whom such a gastric condition, antecedent to the affection, can with certainty be excluded." After reading E. K.'s communication, I at once wrote a short letter to the editor of *Nature*, which I desired him to publish; but, for some reason best known to the editor, the letter never appeared. The object of the letter was to point out that E. K. and Koch were wrong in assuming that the reaction of the gastric secretion is in the healthy person constantly and at all times acid. The recent publication in this JOURNAL of a translation of Koch's opening address at the Conference on Cholera, held last year in Berlin, has reawakened my interest in the subject, more especially as I observe that he adverts to the gastric reaction. I, therefore, now propose to give my experience regarding this physiological question, and in much the same form as I did in my unpublished letter to *Nature*, though in greater detail. My observations were made almost entirely on cats.

In the course of a large number of experiments on the action of saline cathartics (*The Physiological Action of Saline Cathartics*, Edinburgh, 1884), I often found it necessary to kill the animal, eighteen to twenty-four hours, or more, after it had last been fed. In several instances, I took the opportunity of testing the reaction of the contents of the stomach; and I found it almost invariably to be alkaline, provided that the stomach contained no remnant of undigested food, and that the stomach had been for some time, an hour or two, empty, as was proved by the upper half, if not the whole, of the small intestine being likewise empty. The simple explanation of this is, that the digestive acid gastric secretion is, like other digestive secretions, poured out into the alimentary canal only when food, or food-like substances, are present. When these are not present, the mucous membrane of the stomach is bathed merely with the glairy alkaline mucous secretion which every mucous membrane appears to produce.

I next ascertained the effect of water on the reaction of the previously empty and alkaline stomach, by injecting water into the stomach of the living animal, and placing ligatures round the cardiac and pyloric orifices. The water was found, even after half an hour, to remain alkaline; that is, water—at least under the conditions of the experiment—has no excitant action on the glands which secrete the acid gastric juice.

Further, I observed, by a similar mode of experimenting, the effect of the solution of a neutral salt of sodium, as chloride of sodium and sulphate of sodium. In both cases, the saline solution acquired a well-marked alkaline reaction. Several experiments were made with the sulphate, some with a 10 per cent. solution, others with a 20 per cent. solution; but the result was always the same, even though the time which was allowed to elapse before killing the animal varied from one hour to three hours and a half. To give some notion of the degree of the alkalinity of the fluid, it was found in one case, where 50 cubic centimetres (under 2 ounces) of a 10 per cent. solution of sulphate of sodium were used, and the animal killed after one hour, that 0.77 cubic centimetre (11.88 minims) of the usual standard solution of oxalic acid (*vide British Pharmacopoeia* appendix) were required to exactly neutralise the 55 cubic centimetres of fluid which were recovered from the stomach. One of the experiments with the sulphate of sodium solution was made on a rabbit, with a like result; though these animals are less suited for such experiments, since, even after prolonged fasting, their stomach does not become wholly emptied of digesta, which, of course, have an acid reaction. The food was, previously to the experiment, removed from the stomach, which was then well washed by gently syringing it with luke-warm water. But, as this involves a greater disturbance of natural conditions than in the case of the experiments on cats, the result is of less value. It may be of interest to mention that in the case of one cat, where a 10 per cent. solution of sulphate of soda had been injected, and the animal killed after three hours and a half, the fluid, which was removed from the stomach, though alkaline, digested fibrine with great facility after it was acidified. This appears to show that, in certain conditions of the stomach, the peptic ferment may be secreted without its being accompanied by acid. It is of still greater interest, because of greater importance in connection with my present object, to state that the fluids contained but the faintest trace of albumen, and gave no peptone reaction. Further, the gastric mucous membrane showed no signs of inflammatory reddening, unless when a moderately strong solution of chloride of sodium, or a highly concentrated solution of sulphate of sodium had been employed. The secretion which is poured out under the stimulus of the saline solution cannot, therefore, be regarded as merely an inflammatory effusion.

In some other experiments on cats, the saline solution was administered through the mouth by means of a thick flexible catheter; and no ligatures were applied to the alimentary canal. The animals were, therefore, in a perfectly normal condition. As the saline solution passes very quickly from the stomach into the small intestine, the cats were killed at periods within half an hour after the administration of the salt. In every case except one, the reaction of the portion of the saline solution found in the stomach was unequivocally alkaline.

All these experiments were, for the most part, made with another object in view than the mere determination of the reaction of the gastric secretion; but, after reading some of Koch's earlier reports on the cholera-bacillus, I made one or two experiments of a very simple kind, for the purpose of obtaining further confirmation of my previous results. I starved a cat for about eighteen to twenty hours, allowing it a little water, but no ordinary food, in order that the stomach might become quite free from food; I then killed it, and at once removed the stomach, taking care that by the method of killing it, and by the manner in which the stomach was removed, regurgitation of the alkaline contents of the duodenum through the pylorus was, as far as possible, prevented. The stomach was cut open lengthwise, and spread out, with its mucous surface uppermost, on a table. Care was taken that no blood was allowed to touch the surface of the mucous membrane, since the alkalinity of the blood might have masked the reaction of the secretion. In each case, the stomach was quite empty, except for the mucus which coated its walls. Thus spread out, each part of the mucous membrane, from the cardiac orifice to the pylorus, was carefully tested by means of sensitive litmus-paper, and everywhere it was found to possess a distinct alkaline reaction. It cannot, therefore, be doubted that, in the cat, the reaction of the stomach, when empty, or when filled only with water or with a saline solution, is alkaline; and that the reaction is the same in other animals, including man, under the same circumstances, appears to be a fairly reasonable induction. This was decidedly the conclusion at which I had arrived when I wrote to *Nature*, and which I then