



Nachlass Robert Koch

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14/7 85

Officer of Health's Department,  
Town Hall,  
Bradford, 18[85]

Hochgeehrter Herr Professor! [Robert Koch]

Hiermit habe ich die Ehre einige Exemplare der „British Medical Journal“ an Sie zu schicken, in welchen Sie Briefe über den Cholera-Bacillus vorfinden, die Sie interessieren dürften.

Wie es Ihnen wohl bekannt ist hat die Englische Regierung Herrn Dr. Med. Klein voriges Jahr nach Indien geschickt um die Cholera dort zu studiren. Dieses fand bald nach der Bekanntmachung Ihrer merkwürdigen Beobachtungen und Erfindungen über dieselbe Frage statt.

Herr Dr. Klein ist nicht Ihrer Meinung dass der Komma Bacillus ein specifischer ist, und ist vor kurzem so weit gegangen um zu sagen dass ein Bacillus, identisch mit Ihrem Komma (Cholera-) Bacillus im Darne von gesunden Meerschweinchen normal existirt. Ich habe gar kein Vertrauen in seinen Experimenten. Doch das seine Stellung als Regierungs Rath, und die Frage selbst von grosser Wichtigkeit ist, kann man ihn nicht vernachlässigen übersehen. Herr Dr. Cheyne in London, einer der Scharfsinnigsten und gelehrtesten Pathologen Englands hat die Beobachtungen und Resultate von Klein in den „British Medical Journal“ vor einigen Wochen widerlegt.

Indem die Frage so höchst wichtig ist habe ich im Interesse der öffentlichen Gesundheit und der Wissenschaft die genannten Herrn in einen Briefe [Brit. Med. Journal ] eingeladen ihre Experimente vor einer Commission von Fachgelehrten zu wiederholen – wie es Pasteur mehrmals gethan hat; und wie Sie es vor Ihren Klassen zu thun gewöhnt sind. Wenn Dr. Klein wirklich in der Lage ist, wie er annim[m]t, zu demonstrieren dass Sie und wir Alle getäuscht worden sind, und dass ein Komma-Bacillus [unleserlich] normal Meerschweinchen-Darm existirt, so glaubte ich er würde sehr gern diese Einladung annehmen, und grosse Kudos erwerben. Dr. Cheyne schrieb sogleich an die British Medical Journal dass er gern bereit ist die Aufforderung anzunehmen, und die Existenz Klein's Bacillen zu prüfen. Aber Dr. Klein sagt [Brit. Med. Journal ] wenn Prof. Koch selbst nach London kommen will, so will ich meine Experimente mit ihm vor einer solchen Commission wiederholen! Dies scheint einer verdeckten Verweigerung sehr aehnlich. Ihre Anwesenheit kann in keinem Sinne verlangt werden oder nothwendig sein.

Doch habe ich es für richtig gehalten Sie wissen zu lassen wie die Sache jetzt steht so dass Sie selber eine Antwort darüber schreiben können.

Falls Sie nach London nicht kommen können oder wollen so scheint es mir dass Sie der Sache einen grossen Verdienst thun würden wenn Sie erklären würden dass Sie Herrn Cheyne für einen competenten Vertreter Ihrer Ansichten halten, und als einen Experimentator welcher die Experimente von Dr. Klein vor einer Commission zu überwachen vermag.



Wenn es Ihnen gefällt sich in diesem Sinne auszusprechen so wäre Dr. Klein gezwungen entweder vor der Commission seine Experimente zu demonstrieren oder zuzugeben dass er es nicht wagt.

Wenn Sie mir auf meine Aufforderung eine bejahende Antwort geben wollen so bin ich sicher dass Sie alle Leute hier im Lande die sich ~~mit~~ für diese sehr wichtige Frage interessiren sehn verpflichtet werden; und möchte ich Sie bitte Ihre Antwort so z abzufassen dass ich dieselbe im British Medical Journal eindrucken lassen kann.

In der Hoffnung recht bald von Ihnen zu hören habe ich die Ehre mich zu zeichnen mit vorzüglicher Hochachtung ergebenster

Thos. Whiteside Hime

D. med. Sanitätsrath [unleserlich]

Kurz - was mir genügend und wünschenswerth ist dass, wenn Sie Herrn Cheyne als competent Klein's Experimente vor einer Commission zu überwachen, Sie werden so gut sein das zu schreiben, und zu erklären dass (falls Sie nach London nicht kommen) Sie werden mit Cheyne, a[ll]s Vertreter, zufrieden sein.

Anlage: Zeitungsartikel

14/7 85

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[Reprinted for the Author from the BRITISH MEDICAL JOURNAL, March 15th, 1884.]

## A LECTURE ON ASIATIC CHOLERA.

*Delivered at the Westminster Hospital, March 4th, 1884.*

By C. MACNAMARA, F.R.C.S.,  
Surgeon to the Hospital.

GENTLEMEN,—My colleague, Dr. Allchin, before completing that part of his course of lectures on medicine which refer to Asiatic cholera, has asked me to address you on this subject. I am glad to accept Dr. Allchin's offer, because I thoroughly appreciate the spirit in which it has been made; and, further, I think the circumstances of the late epidemic in Egypt should be carefully considered. I have never, so far as I can remember, spoken to Dr. Allchin on the subject of cholera; the idea of asking me to address you was entirely his own, arising from a desire on his part that you should hear the impressions formed regarding this disease by one who lived for nineteen years in the "home of cholera," and who, when there, certainly tried to learn something of its nature and properties. I do not purpose referring to the pathology or the treatment of cholera; for in Dr. Quain's *Dictionary of Medicine*, and elsewhere, I have written fully on these subjects.

During the year 1866, with the consent of the British and other governments of Europe, an international conference on the subject of cholera was held at Constantinople. The objects of this meeting were to investigate the cause, and to devise means for preventing this disease from so frequently spreading from India to Europe, especially through the Suez Canal. One of the results of the conference was the appointment of an International Sanitary Board, having its headquarters at Constantinople, with medical officers under it stationed in Jeddha, Suez, Alexandria, and, in seasons of cholera, at Mecca, and other important points. The duty of these officers was, through the aid of constituted authorities, to prevent infected persons or things from carrying the disease into distant parts. Within thirty-three years—that is, from 1830 to 1864—epidemic cholera had extended from India to Europe and America on four different occasions, each invasion of the disease having been attended with a terrible loss of life; but since the establishment of the international sanitary service in 1866, no epidemic outburst of cholera such as those which had previously occurred has visited this Continent. It is true that epidemic cholera of a virulent type spread through Lower Egypt last year, but our military operations in that country in 1882 had practically, for the time being, suspended the working of the sanitary service so far as the Suez Canal was concerned.

In 1874, another international conference on cholera was held at Vienna, with the object of defining more clearly the quarantine regulations which were to apply to vessels arriving in Egypt or Europe from ports affected with cholera. The regulations then drawn up virtually formed a contract between the consenting parties, of which England was one; and these regulations were based unquestionably on the understanding that Asiatic cholera was a communicable disease. This fact had, however, been admitted by our Government previously to the Vienna conference; for, in an Order of Her Majesty's Privy Council, dated August 1871, it is stated that, as Asiatic cholera was "prevailing in foreign ports within a few weeks' voyage of this country, and may probably extend to others which have still quicker communication with England, it is not unlikely that, within the next month or two, cases of the disease may be brought to the ports of this country." This order lays down the principles which guided the Government in its action, as follows:

"Asiatic cholera has a peculiar infectiousness of its own which, when local conditions assist, can operate with terrible force, and at considerable distance from the sick. All matters which the patient discharges from his stomach and bowels are infective; probably the patient has no power of infecting other persons except by means of these discharges, nor any power of infecting even, except in so far as particles of these discharges are enabled to taint food, water, or air which people consume."

These principles not only expressed the opinions of Her Majesty's Privy Council in 1871, but, outside the immediate influence of the Government of India, they accurately expressed the opinions of all the leading medical and sanitary authorities of Europe and America.

We must clearly bear in mind the fact that the above-quoted Order of Her Majesty's Privy Council was issued, because cholera had appeared in places within an appreciable distance of Europe. Nevertheless, in July 1883, Earl Granville (Secretary of State for Foreign Affairs), in a despatch sent to the French Government, complaining of the tone adopted by the press of that country towards England, for our supposed inaction with reference to the cholera in Egypt, remarks that the principles which have guided Her Majesty's Government are based upon the following considerations (the italics are mine). "*Every time there has been an outbreak of cholera, people have invented some story, more or less credible, to demonstrate in what manner the disease was imported. These stories have been carefully examined in their time and place, and Her Majesty's Government experience no hesitation in affirming that no outbreak of cholera in Egypt, or even in Europe, can be considered as having occurred by the importation of the disease on board ships arriving from the Indies. Supported by this information,*



Her Majesty's Government offer a serious and well founded objection to the theory generally admitted, and the custom of quarantine." "Sanitary measures have proved to be the only efficacious means of impeding the march of an epidemic." "In spite of some divergence of opinion with regard to the contagium, the avowal that no theory concerning the origin or propagation of cholera should be accepted as true, and the history, the cause, and the nature of the disease, both in its endemic and epidemic forms, are yet to be discovered." We read with infinite regret a statement of this kind, which seems to throw discredit on the work done by Simon, Parkes, N. Radcliffe, and many other able observers, to elucidate the circumstances of this fearful disease. The question, however, arises, How is it that our Government has abandoned the teaching of these authorities, admitting, in 1871, the communicability of cholera with all its consequences, and in 1883 declining to entertain this doctrine? Earl Granville, though responsible for the despatch above quoted, must have been guided by the opinion of the medical men he consulted on the subject. So long as Mr. Simon held office, it would appear that the Government referred to him on these matters. Dr. G. Buchanan succeeded Mr. Simon, but not quite to his office; that was created by statute, and it lapsed when the Local Government Board was constituted by a later statute. Nevertheless, I believe that Dr. Buchanan, and the department over which he presides, helped to work out Mr. Simon's reports, and that the opinions they hold on the subject of Asiatic cholera are in accord with the views so admirably expressed in the report of the Local Government Board, dated 1875. It seems hardly possible, therefore, that Earl Granville's despatch emanated from the health-authorities of the Government, and it is generally assumed that Sir J. Fayrer was authorised by Government to select some one to go out to Egypt, and report upon the cholera prevalent there in 1883. Dr. Hunter, now Sir W. Guyer Hunter, was chosen for this office. If I am not mistaken, Dr. Cunningham, the Sanitary Commissioner with the Government of India, was also in London in the spring of 1883. On referring to the published opinions held by these three physicians upon the subject of cholera, we find that they as closely coincide with the ideas expressed in Earl Granville's despatch as they differ from those of Mr. Simon and the Local Government Board. Sir J. Fayrer states "most emphatically that he does not believe in the contagiousness or communicability of Asiatic cholera" (*Lancet*, p. 296, February 16th, 1884); that he does not believe that cholera ever has been, or ever can be, carried from one place to another by means of human beings, or by merchandise travelling in vessels or in any other way. Dr. Cunningham writes: "Fairs and other gatherings in this country (India) have again and again testified to the truth of the conclusion that cholera is not carried by persons from one locality to another" (*Eighteenth Annual Report of Sanitary Commissioner with Government of India*). Lastly, Sir W. G. Hunter asserts that, "being one of those who do not believe in the contagiousness of cholera, and who, therefore, do not look for a specific entity or germ, and its importation into a country to account for the development and spread of the disease, I turned my attention to the country itself in the search for a cause of the epidemic" (*BRITISH MEDICAL JOURNAL*, p. 93, January 1884). I wish here to assert that these opinions are at variance with those held on this subject by many of the best known officers in the Indian Medical Service. I have only to mention the names of Mr. Cornish, Surgeon-General, Madras; Dr. Furnell, Sanitary Commissioner, Madras; Dr. Payne, head of the Medical Department, Bengal; and Mr. De Renzy and Mr. Scriven, of the Punjab; Dr. J. Murray and Mr. C. W. Deakin of the North-West Provinces; as also Professors Maclean and Aitken of Netley. Unfortunately, the action taken by our Home Government has extended to the Government of India; and, in almost identical terms to those used in Earl Granville's despatch, they decline to discuss, and evince impatience at receiving, reports from either civil or medical officers referring to cholera as being a communicable disease; because they fear the action of the International Sanitary Board at Constantinople—a board which, as we have seen, was constituted with the consent of our own Government. It is the fear lest this board should put in force regulations such as those decided on in the conference of 1874, to the supposed disadvantage of the mercantile community of India, that constitutes the real difficulty. Conduct of this kind, however, is eminently short-sighted; for, let the practice of our Government be what it may, if they repudiate the principles which govern the practice of all the other civilised countries in the world with respect to the communicability of cholera, the time must speedily arrive when the ports of Europe will be closed to our vessels directly there is a rumour of cholera in the Red Sea or in Egypt. This consideration alone should lead our Government to work with, and not against, public feeling. But over and above this consideration, suppose that, after all, Mr. Simon, Dr. Parkes, and the greater number of men who have carefully examined this subject, are right, and that cholera is a communicable disease, how great is the responsibility of the Government in declining to receive and act upon the opinion of such men!

But, it is argued, cholera did not extend to Europe in 1883. Cer-



tainly not ; but, so far as we know, small credit is due to England for this ; rather the various countries of Europe refused to allow the disease to be imported into their midst. I say there are no thanks due to England, because, as we have seen above, the latest principles enunciated by our Government are that it is useless to attempt to stop cholera from extending by quarantine-regulations ; "sanitary measures," in their opinion, being the only means capable of impeding the march of an epidemic of this disease. On the other hand, there can be no question that, from 1866 to 1883, the Sanitary Board of Constantinople being in charge of Arabia, Egypt, and the Red Sea, cholera has not extended in this direction to Europe. But during this time it has spread from India westward, where no such means of protection were furnished ; and further, the very year after the operation of the Sanitary Board had been interfered with, and a large body of native troops had been sent from India to Egypt, Asiatic cholera broke out in that country.

Sir W. G. Hunter having gone out to Egypt disbelieving in the communicability of cholera, arrived in that country when the epidemic was at its height, and the people were demoralised by the effects of the disease. Those of you who study the reports issued by the Local Government Board, must be aware of the extreme difficulty there is in tracing an epidemic or *contagium* to its source, even when the inquiry is conducted by trained experts and in a country like England. Such being the case, it was hardly possible for Sir W. G. Hunter to have arrived at ideas respecting the cause of this outbreak of cholera other than those contained in his report. In the circumstances, however, his conclusions can hardly help the profession one way or the other. Having determined in his own mind that cholera was not imported into Egypt in 1882-83, Sir W. G. Hunter proceeds to discuss how the disease commenced in that country. He believes that the epidemic of cholera may have originated in the unsanitary state of Egypt. I can only repeat, through all its long history, the unsanitary condition of that country had never yet been known to give birth to epidemic cholera. Beyond this, such conditions never have produced an epidemic of this disease in any other part of the world. Failing this cause, Sir W. G. Hunter thinks the epidemic of 1883 might have been a revival of the epidemic of 1865. Supposing this had been the case, would not the dreaded Sanitary Board at Constantinople have known of the continued existence of the disease in Egypt ? And surely it is a remarkable circumstance that, during our military operations in the country in 1882, nothing was heard of the disease among our troops. But Sir W. G. Hunter assumes that, if neither of these causes produced the cholera of 1883, then it might have developed out of the diarrhoea common in that country. We are left in the dark, however, as to why diarrhoea has not often before given rise to epidemic cholera, either in Egypt or in any other part of the world. The idea is at variance with all that we know of the history of the disease.

It is beyond my power to say what kind of evidence would be necessary to convince those who now disbelieve in the communicability of cholera, that it is a disease which spreads in the manner indicated by Mr. Simon. But I may refer to the reasons which caused me to arrive, as far back as 1866, at a most positive conviction that the disease is a communicable one. At one time, although not denying, I could not subscribe to the opinion that cholera was a communicable disease ; this fact may be substantiated by reference to p. 121, vol. iii, of the *Indian Medical Gazette*. At that time I had proof that the dejecta passed by patients suffering from cholera might be taken into the stomach of human beings and not cause cholera, and that the medical or other attendants on cholera-patients did not, except under well defined conditions, suffer from the disease ; but, as I studied with greater care the histories of the various epidemics, and filled in these histories from the old MS. records of the Calcutta Medical Board, I became convinced that Asiatic cholera had invariably extended from India over the world, along the lines of human intercourse, and that the disease was therefore a communicable one. At this time, as stated in Dr. Quain's *Dictionary of Medicine*, the following circumstance was brought to my notice. "A small quantity of a fresh rice-water stool, passed by a patient suffering from cholera, was accidentally mixed with four gallons of dirty water, and the mixture exposed to the rays of the tropical sun for twelve hours. Early the following morning, nineteen people each swallowed about an ounce of the contaminated water ; they only partook of it once, but within thirty-six hours five of these nineteen persons were seized with cholera. In this case, the cholera-evacuation did not touch the soil ; as it was passed so it was swallowed, but (and this is the most important to remember) it had been largely diluted with impure water, and the mixture had been exposed to the light and heat of a tropical sun for twelve hours." In the year 1872 I was engaged in making some experiments on monkeys. For this purpose I exposed to the sun some fresh cholera-dejecta in a pail full of dirty water. The following morning, at about 6.30 A.M., I and my assistant were shut up in a close room with these monkeys, experimenting upon them with this diluted cholera-stuff. By two o'clock the same day I was suffering from a severe attack of cholera, and the same evening my assistant was seized with the disease ; we were both of us very dangerously ill, passing into a state of collapse in a few hours after the commencement of the symptoms. The monkeys escaped without the slightest ill effects from our endeavour to give them cholera.

With convictions, therefore, formed not only from the evidence afforded me by the history of the disease, but also from an experience of the communicability of Asiatic cholera such as I have referred to, you will agree with me that I was justified in publishing the following statement regarding the epidemic in Egypt of 1883. "Asiatic cholera has never yet visited Egypt unless it has first broken out in Turkey, Arabia, or Europe, whence it could be traced back to India. Last year, for the first time in the history of Egypt, a large body of native troops and camp-followers were brought by steamers and landed



in that country from India; and the following year (at its usual season for development) Asiatic cholera appeared among the Egyptian people. It is worthy of note also that, in consequence of the war, the international sanitary arrangements of Egypt and the Red Sea were suspended during the latter half of 1882-83. With facts of this kind before us, the question of exactly how or when the cholera of 1883 originated in Egypt is a matter of secondary importance."

I have explained in the *BRITISH MEDICAL JOURNAL* (October 27th, 1883), that in the previous February, before cholera had been heard of in Egypt, I had applied to the Government of India for the services of a young medical officer, then in England, to study, in conjunction with myself, the subject of micro-organisms in connection with cholera. My idea was that, by working together, I should save this gentleman the unnecessary labour of going over much ground that had been explored; and, subsequently, he might have carried on his investigations in Bengal; but the Secretary of State for India declined to accede to my proposal. I expressed my own opinion very clearly on the contagium of cholera as far back as 1867, holding that we had then no proof of the existence of a specific form of bacteria in cholera, but that the disease depended on the introduction of specific organic matter into the intestinal canal. The Egyptian cholera, however, of 1883 will certainly be connected in the minds of scientific men with the admirable researches commenced by Dr. Koch into the micro-organisms of cholera. This work Dr. Koch has subsequently carried on in Calcutta; and, although no one can receive his admirable reports with greater satisfaction than I do, it is impossible to ignore the fact that this most important work is being effected in our Indian possessions, it is true, by brethren, but nevertheless by rivals. Dr. Koch has found bacteria in the walls, epithelium, and glands of the intestinal canals of persons dying from cholera. He has separated and cultivated these bacteria, and finds they differ from those discovered in cases of diarrhoea and dysentery. The presence of these bacteria, Dr. Koch believes, excites sufficient irritation in the mucous membrane of the intestines to cause the symptoms of cholera. But it is said that heaps of micro-organisms exist in the contents of the intestinal canal and in decaying epithelium in all parts of the body; but this is no reason why some form of specific bacteria may not exist in cases of cholera. We might as well say that no form of pus contains specific qualities because, under the microscope, all pus looks very much alike. By its effects we know that pus differs in its properties in a very remarkable way. Then, again, it is said the micro-organisms are the effect of disease, not the cause. But how can disease produce a living micro-organism? If, on the other hand, you find the mucous membrane of the intestine perforated, and, in fact, its tissues swarming with micro-organisms, which appear to be living at the expense of these structures, surely it is not difficult to imagine that we have in such organisms a sufficient cause of irritation to account for the symptoms of cholera. It is satisfactory, however, to find that, while prosecuting his researches into the micro-organisms found in the walls of the intestines in cases of cholera, Dr. Koch has been led to inquire into the cause of the remarkable diminution in the death-rate from cholera in Calcutta since the introduction of the new water-supply into that city. In my work on the *History of Cholera*, published in 1876, p. 423, I have gone fully into this subject, and the still more remarkable instance of the condition of our troops in Fort William.

In conclusion, I hold with the views entertained by the majority of men who have most thoroughly studied the subject, that it is in the power of England to do very much towards preventing the extension of cholera from India over the world by means of well advised quarantine-regulations applied to vessels arriving from infected ports. I also believe that more might have been effected by our Government in India to lessen the death-rate among our European troops and the natives of that country from cholera; but that, when cholera exists in an epidemic or any other form in a country, there is one, and only one, way of averting its fatal effects, and that is by preventing the water and food which people consume from being contaminated by the organic matter passed by patients suffering from the disease. This infecting matter may retain its deadly influences if kept in a dried condition (as on soiled clothes) for years; but, if it pass into water containing elements which are probably essential to its development, it bursts forth into rapid growth. The life of this organic matter (it very possibly is a micro-organism) is, fortunately, of short duration; so that, if it pass into a river or well containing elements favourable to its growth, it springs into active life, but dies in a few days. Unless such a source of water-supply be again contaminated, it may be consumed with impunity in the course of a few days. The rapidity with which the organic matter passes through its stages of development depends largely upon the conditions of the atmosphere and the amount of sunlight it receives. If the organic infecting matter of cholera pass into water which does not contain elements essential to its development, it may remain sterile. My firm conviction is, that no amount of cleanliness, drainage, or any other means, will preserve a community from epidemic cholera if the food or water people consume be contaminated by the dejecta of cholera-patients. On the other hand, no amount of filth, overcrowding, or other unsanitary conditions, can produce epidemic cholera, if the organic infecting matter be kept out of the food and water. Unsanitary conditions predispose people to cholera. The majority of the world's populations, however, especially the lower class, live in such conditions; and therefore a greater number of the poor die from epidemic cholera than those who are in better circumstances.



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## THE CHOLERA-BACILLUS FROM A PUBLIC HEALTH POINT OF VIEW.

SIR,—As one specially interested in the subject of public hygiene, I have made it my business to follow the researches as to the nature and diagnostic value of the comma-, or so-called cholera-bacillus.

The subject is not merely of local or even national importance. The rapidity with which cholera can travel, the frightful ravages due to its epidemic violence, and the great facilities offered for its spread by the increased means of communication between different countries at present, render this subject one of universal importance. Up to the present time, there has existed the greatest difficulty in diagnosing cases of severe English cholera (*cholera nostras*) from true Asiatic cholera.

In consequence of erroneous diagnosis, unnecessary panic and great commercial loss have been many times caused by the announcement of the occurrence of a case of Asiatic cholera, which was really only English cholera. And, what is still worse, epidemics of Asiatic cholera have been allowed to grow to formidable proportions before their real nature was discovered. Hence the great value which Koch's discovery assumes, if it offers, as he says, a certain and easy means of diagnosing between two diseases very different in their fatality, but not dissimilar in their symptomatology.

The German government at once recognised the value of Koch's

discovery from this point of view. It has arranged that medical men from various localities shall have the opportunity of studying in Berlin, under Koch's personal superintendence, the nature and mode of artificial culture of the cholera-bacillus. Already 150 medical men have gone through the bacteriological course in the Imperial Sanitary Institute, and are now in a position, within forty-eight hours, to diagnose whether a suspicious case is one of Asiatic cholera or not. The apathy hitherto displayed in this country, although certainly not less interested in the question than the German Government, is not a little surprising. So far as I am aware, there is no public institution in Great Britain where a person anxious to study this most important and recent branch of experimental pathology can do so. I have had to avail myself of the resources of a private one, and I believe that our Government possesses no laboratory where researches of this nature can be carried on, but is obliged to depend on the resources of private individuals, or of the Brown Institute.

Dr. Klein, who was sent to India, after Koch's return, to investigate the matter in the light of recent discoveries, does not admit the specific character of the comma- (*cholera*-) bacillus, and has nowhere expressed an opinion in favour of its diagnostic value. It is most perplexing, indeed, that he has not expressly stated his opinion on this capital point. In a recent number of the JOURNAL, he states that "they [the comma-bacilli] are therefore the result, and not the cause, of the disease." Now, if they are an invariable result of Asiatic cholera, and are the result of no similar disease, their presence as a diagnostic sign is quite as important as if they were the cause, instead of the result. The discovery of a scarlatinal rash, the result of the specific infection, is of quite as great value as a diagnostic sign as would be the discovery of the microbe to which the disease is due. This being the case, it is most surprising that, in view of the possible recrudescence of cholera on the Continent, and of its invasion of this country, Dr. Klein should not advise the Government to take steps to utilise Koch's discovery. If, however, his silence on this point is to be taken as a denial of the diagnostic significance of the comma- (*cholera*-) bacillus, then we are landed in the difficulty, that the official adviser of the English Government stands in direct opposition to the unanimous opinion of the great majority of those who are capable of giving an authoritative opinion on the subject. In this country, one of the ablest bacteriologists, Mr. Watson Cheyne, is publishing a series of papers in the JOURNAL, in which he very ably maintains the accuracy of Koch's results. Surely the point is deserving of immediate decision, so that the Government and the medical officers of health throughout the country may be relieved from any doubt.

I would suggest, as the only way of deciding the contradictory experiments of Dr. Klein and Mr. Cheyne, that they be repeated before a competent commission. Neither of the gentlemen named need consider such a reference to arbitration as in the least derogatory, and a decision of the disputed points would be of the utmost importance.

It is well known that on many occasions the novel and surprising discoveries of Pasteur were received with open doubt, and even disbelief, at the Academy of Sciences in Paris. Confident in the accuracy of his methods and the correctness of his conclusions, he several times claimed the appointment of a commission of experts, before whom he repeated his experiments. Koch has, apparently, not hesitated to act in a similar way, in calling together a commission of scientific men to see and discuss his experiments. There are a number of men among ourselves well able to supervise and test experiments of this kind. I need only mention the names of Tyndall, Lister, and Burdon Sanderson. I believe that the appointment of such a commission would be highly satisfactory. May I hope that the Association, on whose behalf Mr. Cheyne is reporting on the subject, will take the initiative in this matter? The Association has specially involved itself in the investigation, and ought at once to justify its selection of Mr. Cheyne as its research scholar, by having his experiments tested, and his conclusions proved to be warranted or not.—I am, sir, yours, etc.,

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