

Cholera Readings

5/27/2020

As we discussed last week, knowledge of disease needs to fit into a way of understanding the world – a “framework” through which suffering makes sense. The following readings present different views of cholera, each with different understandings of disease and society.

As you read, consider the following questions:

What frameworks might we infer lead to these different understandings? How does disease “work” in each of these cases? What appears to be said and left unsaid in these different explanations? (That is, what does each author assume their audience knows already, and what needs to be explained?)

“Cholera Morbus Prescription,” 18 --. (Wellcome Collection)

A “prescription” describing (one view) of the steps that one should take to prevent and cure cholera around the middle of the nineteenth century.

Enos T. Throop, “Governor’s Message, 21 June 1832” in *Messages from the Governors*, (J. B. Lyons: Albany, 1909) 393-395.

An address from Governor Enos T. Throop of New York, to the New York State assembly, discussing the presence of cholera in the state. Note his opinion as to the causes of the spread of cholera, and his implied remedy.

“Cholera at the Fair,” *Chicago Daily Tribune* (09 Feb. 1893) 4.

A brief article describing the steps taken to ameliorate the risk of cholera at the 1893 Columbian Exposition, held in Chicago. Note especially how the author thinks about the matter of municipal authority in controlling cholera.

(OPTIONAL)

These optional readings give a little bit of the scientific background of debates about the cause, or “etiology” of cholera. As with the previous readings, it is instructional to note what the authors think is necessary to emphasize, and what they leave out.

Henry Koplik, “The Present Aspect of the Cholera Question, by Max von Pettenkofer,” *Lancet*, 10 July, 1886, pg 89.

Pettenkofer was a physician, a medical scientist, and an “anticontagionist” – that is, he didn’t believe that cholera was contagious. How could this be so? This document provides some clues as to his reasoning.

Douglas Cunningham and Timothy Lewis, “Scientific Investigation into the Causes of Cholera,” *Lancet*, Jan 2, 1869, pg. 3.

An earlier interview with Pettenkofer, in which he more quickly outlines his belief that cholera is not contagious.

Edgar Crookshank, “Remarks upon the Cholera Bacillus of Koch,” *Lancet*, 13 Jun. 1885, pg. 1072

Robert Koch was Pettenkofer's opponent – a “contagionist” and advocate for the theory that cholera was caused by a bacteria. Of course, today this fact appears to be obviously true, but pay attention to how carefully Crookshank builds his argument in favor of the contagionist position, and how carefully he thinks about possible objections.

CHOLERA MORBUS.

The Following Prescription

FOR

THE CURE OF THE CHOLERA MORBUS,
Has proved greatly beneficial in numerous instances in FRANKFORT, and is strongly recommended by those who have had opportunity of witnessing its efficacious tendency.

One Pint of Strong Spirits of Wine.

One Pint of good White Wine Vinegar.

One Ounce of Powdered Camphor.

One Ounce of Flour of Mustard, or bruised Mustard Seed.

Quarter of an Ounce of Ground Pepper.

One full Tea-spoonful of Bruised Garlic.

Half an Ounce of Powdered Cartharides.

Mix the above well together in a Bottle, and expose it twelve hours to the sun, or in some warm place, taking care to shake it repeatedly.

Let the patient be instantly put to bed, under warm Coverlids, and let his Hands and Feet be rubbed powerfully and uninterruptedly with the Mixture warmed. During this operation let the Patient take a Tumbler full of strong drink, composed of two parts of Camomile Flowers, and one part of Balm Mint. Persevere in this course, and at the end of Fifteen Minutes, at the utmost, the Patient will break out into a profuse perspiration, his Head and Body being well covered beneath the Bed-clothes. The Patient must be kept in this state between Two and Three Hours; he must not fall asleep. After this, remove the extra covering from off the Bed, and the Patient will drop into a slumber, which will last Six or Eight Hours, accompanied by a gentle perspiration. When the Patient awakes, he will find himself weak, but the disease will have entirely left him; he will require nothing but rest and a moderate diet to restore him to perfect health.

After having been rubbed, the Bed-clothes must COMPLETELY cover the body and arms, as the slightest chill would occasion his death. When the Cramps in the Stomach come on, apply very hot dry Bandages of Bran and Ashes to the Pit of the Stomach, and when necessary, a Bladder of Hot Water to the region of the Navel.

The great object is to produce strong perspiration, and to restore the circulation of the blood.

In all cases where ~~RE~~ ^{RECOUSE} IMMEDIATE ATTENTION to the above Recipe has been had ~~course to~~, the Patient has, under the Divine blessing, recovered.

The disease has been proved NOT TO BE CONTAGIOUS.

STATE OF NEW YORK

MESSAGES FROM THE GOVERNORS

COMPRISING

Executive Communications to the Legislature and Other
Papers Relating to Legislation from the Organization
of the First Colonial Assembly in 1683 to
and Including the Year 1906

WITH NOTES

EDITED BY CHARLES Z. LINCOLN

PUBLISHED BY AUTHORITY OF THE STATE

VOLUME III

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ALBANY

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1909

State to convene, as a Legislature, at the Capitol in the city of Albany, on Thursday the 21st day of June next, at twelve o'clock, at noon.

Given under my hand and the privy seal of this State, at the city of Albany, the twenty-sixth day of May, in the year of our Lord, one thousand eight hundred and thirty-two.

E. T. THROOP.

GOVERNOR'S MESSAGE.

FELLOW-CITIZENS OF THE SENATE AND OF THE ASSEMBLY.—
At the opening of your last session, it was expected that the National Legislature would apportion among the States the representation in Congress, at a day so early as to enable you before your adjournment to divide the State into suitable election districts. Our hopes in this respect were not realized; and after a very laborious session, you adjourned, at the usual time, having previously provided by law for a contingent postponement of the next election, in case an apportionment should not be made. Since your adjournment, Congress has acted upon this subject, and fixed a ratio of representation in the popular branch of that body, by which our State is entitled to forty members, whereby the law postponing the election is annulled.

On the occurrence of this event, I have, in obedience to my constitutional duty, as well as in compliance with your expressed wishes, convened you at this time, that you might provide for the exercise by this State of its full voice in the councils of the nation. As this was my motive for calling you together at this unusual and uncomfortable season of the year, so it is the only business pertaining to your duties within my knowledge, now so urgent as to justify me in the exercise of this power. I am happy to be enabled to say this, and to express the belief that the public wants will not require of you to sacrifice your personal comforts and private interests to the duties of ordinary legislation, through a protracted session. [See note 12.]

There is one other subject, however, which I feel bound to present to your consideration, and which may be disposed of without materially interfering with your special business, or prolonging your session. I allude to the supposed approach, at a time of unusual health within our State, of the Asiatic Cholera; a direful disease, which, having spread desolation over a great part of Asia, has advanced with rapid strides through Europe, and is now supposed to have crossed the Atlantic and to be frightfully busied in the work of death among our Canadian neighbors. It is certain that a very malignant disease, in its type resembling the much dreaded cholera, is ravaging the hordes of squalid emigrants which have been recently disgorged from transport ships near our borders, and has been communicated from them to a portion of the population of the cities of Quebec and Montreal.

It has not been satisfactorily ascertained that the disorder has been communicated within our State, although a few cases of sudden death have occurred, which would not have attracted particular notice in ordinary times. Whether the disease which has proved so fatal in Canada is the cholera, or has been produced by the very unfavorable circumstances under which an unusual number of the most destitute class of emigrants have been landed during warm weather, in a strange climate, cannot be known without a more minute and scientific examination of facts. But so far as facts have been ascertained, there is reason to believe that the two disorders, if of different character in other respects, resemble each other in these particulars, that they are caused by inattention to cleanliness, and by enfeebling dissipations and excesses, and may be communicated from one person to another in a tainted atmosphere. Cleanliness in and about houses and neighborhoods is believed to be the most effectual barrier against the spreading of this and other similar diseases.

Most of the emigrants who land in Canada direct their course toward the United States, with a view of settling there, and thus expose our fellow citizens to the contagion of diseases which they may bring with them. Heretofore it has not been deemed necessary to guard any avenue for the approach of infectious diseases, except the city of New York, and our quarantine and health regulations relate chiefly to that city. No power now exists in any public officers or municipal authority elsewhere to interfere to prevent the introduction of disease into other parts of the State. We shall be annually exposed to similar incursions, and I respectfully recommend to you to pass such laws as may be necessary to enforce a sanative quarantine, at or near the other ordinary points of ingress to our State, and to invest the proper officers with sufficient power to act promptly and efficiently in times of alarm and emergency. An infinitely wise and just God has seen fit to employ pestilence as one means of scourging the human race for their sins, and it seems to be an appropriate one for the sins of uncleanness and intemperance; and when we can trace its existence and propagation under any circumstances to certain causes, it is our duty to employ suitable means to remove those causes and to arrest its progress.¹⁶

Having thus discharged our duty, we may repose with confidence upon a power, wisdom and mercy beyond our control or comprehension, to bless our exertions and prayers, by removing from us in due time existing calamities, or by averting those which may threaten.

E. T. THROOP.

Albany, June 21, 1832.

July 2. The Legislature adjourned without day.

¹⁶ By chapter 333, passed June 22, "for the preservation of the public health," quarantine regulations were to be imposed on vessels coming to New York, from any place in which Asiatic or malignant cholera or any other malignant disease was known to exist. The act also contained provisions relative to the organization of boards of health in different parts of the state, and prescribed details of administration.

CHOLERA AND THE FAIR.

Director-General Davis has submitted a report on the possibility of cholera germs being introduced into this country with exhibits sent to the World's Fair. He regards the risk as small, and believes there will not be the slightest danger if a strict quarantine be maintained under government control at New York and other ports, and if Chicago be kept thoroughly clean. These two things are essential to public confidence, and would insure it. He holds it to be the duty of the municipal authorities to make this city "conspicuously clean," and to keep it so till next November, when the public sentiment will demand that there be no relaxation of the system. For the Exposition he recommends a special sanitary corps, to be organized under the medical director, with a force of perhaps twenty to thirty inspectors. The duty of these people should be to make a minute inspection of the entire Exposition daily, or twice a day should the weather require it. He thinks such a sanitary service is an absolute necessity, but nothing beyond that is advisable, unless in case of emergency and then upon the judgment of the medical officers.

The Director-General says there are but two sources from which the introduction of cholera to our country can be expected. The first includes immigrants and visitors from places where cholera prevailed last season, and where it is not entirely eradicated. The second is bales of merchandise from those ports. But rags constitute the class of merchandise which forms almost the exclusive vehicle for conveyance of disease, and the bulk of the rags received by us are shipped from the ports of Southern Europe, which were not visited by the disease last year. Only a small portion of the rags thus imported ever reach Chicago, most of them being used in the paper and cloth mills of the Eastern seaboard States. New merchandise, such as will be sent to the Exposition, is not recognized by the medical authorities as in any sense dangerous. The question of personal importation of cholera is more serious, but the danger is slight. Last year "the disease was absolutely excluded under the most unfavorable conditions," such as will probably never again, and certainly, not this year, be permitted to occur if human vigilance can prevent.

The report also calls attention to the fact that in the cholera-infected districts of continental Europe the winter has been one of unusual, even phenomenal, severity. While it is admitted that severe cold does not kill the cholera germ it is equally admitted that severe cold absolutely checks the spread of infection and stops the reproduction of the germ, except in the already infected human subject. The goods which will be received in Chicago this year for the Exposition will have been handled, finished, packed, and shipped by healthy workmen during the prevalence of a temperature wholly unfavorable to the conveying of the infection, if not actually prohibitive of it.

All this is true, and yet the Director-General may not be warranted in denouncing the anxiety about a possible outbreak of cholera this year as the result of a professional fad among medical men. It is only by warning people against a possible and maybe a pending danger that they can be induced to take the preventive measures necessary to ward off the mischief in case it makes its appearance, and it is better to be prematurely warned a dozen times than not to be warned at all when "the wolf" is actually at the door. While undue alarm is to be deprecated, some degree of fear for the possible consequences of neglect is necessary to stimulate to the adoption of the precautionary measures recommended in the report; and without it the authorities would not be justified in incurring the expenditures which such precautionary measures will entail.

THE PRESENT ASPECT OF THE CHOLERA QUESTION.

BY PROF. DR. MAX VON PETTENKOFER,
OF MUNICH.

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HENRY KOPLIK,
OF NEW YORK.

(Concluded from page 30.)

HIRSCH, in his work on Historico-Geographic Pathology, has collected a number of facts concerning the Indian epidemics which speak as much in favour of the influence of human intercourse as they do for that of *time* and *locality*. The epidemiological facts connected with the course of cholera through long intervals of time and over wide tracts of land show that the spread of the specific cause of cholera through Europe is clearly connected with human intercourse, for in no other way could we explain how the disease has wandered in different directions from India step by step, and why so great a time was included in this spread of the disease. Yet human intercourse does not account for everything in this connexion; time and locality also play an important rôle in this question. The author lays especial stress on the two latter conditions, and endeavours to explain through them the diversity of epidemics in time and place of occurrence. The Contagionists, in rejecting the influence of time and locality, are as one-sided as the Autochthonists, who reject entirely the facts speaking for the influence of human intercourse.

1. *Infection of the healthy by the sick.*—Contrary to what is found to be true in the case of entogenic infectious diseases (small-pox, typhus, and scarlet fever), physicians in constant attendance on cholera patients are not attacked by the disease more frequently than others who are not. This same phenomenon has been observed among the nursing staff. If cholera were contagious in an entogenic sense, these persons should show a greater mortality than others. The researches of Günther, Bouchardt, Kopp, and Ricord support the above fact. Prof. Pettenkofer has found that in the epidemic in Munich the nurses also were as little subject to the disease as the physicians, though they came in much closer contact with the patients. The nurses, or other persons who are in the vicinity of the patients, become sick, not from an entogenic infectious matter, but from an ectogenic matter, which either develops in the locality itself or is brought by the patient on his person in sufficient quantity from the cholera district. That intercourse with the patient cannot be regarded as a source of infection shows itself most plainly in India among the nursing *personnel*. The explanation offered in the Berlin Conference of the fact that the nurses had perhaps acquired a certain immunity and protection against the disease from their being natives is inadmissible, inasmuch as natives outside the hospitals not in contact with patients fell victims to the disease. If the nurses became the victims of the disease (which occasionally happened), its spread among them was checked, not by disinfection, but by *changing the locality* of the hospital. Virchow (as contagionist) cited a case at the Berlin Congress which showed that a patient suffering from cholera infected also his three nurses. These nurses, however, might have been infected from ectogenic matter adhering to the body of the patient; or, again, the above facts do not exclude also the probability that the nurses may have been subjected to the same influences to which their patient was, and thus have contracted the disease. The vast number of facts given us by the history of epidemics in Europe and India (Cunningham) cannot be put aside by exceptional picked cases. If in most cases the physicians and nurses are not attacked by the disease, it must be because the cholera patient produces nothing in itself infectious (entogen), and the exceptional cases of infection can be explained by assuming that the patients in these instances have brought from the cholera district an ectogenic matter on their persons, which, developing in the new locality, spreads the disease.

2. *The excrement of the patients as the seat of infectious matter.*—If the cholera patient himself cannot in an entogenic sense produce anything to convey the disease, his excretions

also certainly cannot. It is to be expected that, as the intestinal tract is the seat of the principal phenomena of the disease, the stools should be found to contain infectious matter. Professor Pettenkofer himself once believed that the stools, if not immediately the seat of infectious matter, could become so by subsequent development in waterclosets and drains. The experiments of Burdon-Sanderson and Thiersch seemed to support this view. Koch also has recently discovered that a constant element in the stools of cholera patients was a bacterium (*comma bacillus*). But if we go over the history of epidemics in former times, we see, by the light of present bacteriological researches, that the careful disinfection with iron sulphate and carbolic acid was powerless to carry out its professed object; the vitality of certain micro-organisms is not annulled by these agents. The vaunted control of certain epidemics by these agents must have been illusory. The limitation of the epidemics must have been due, in the light of present knowledge, to other more potent agencies not at the time understood. Moreover, in draining cholera localities the excrements of cholera patients must necessarily have passed through large tracts of country, yet no infection has been seen to take place along the direction of such drainage (India). Patients passing through towns have, though suffering from cholera, stopped at hotels and inns, and no cholera has been found to exist along their route. The author, therefore, though accepting the existence of a bacillus, *x*, entogenic in its nature, as the exciting cause of infection, does not admit the entogenic doctrine of Koch concerning the cholera bacillus. The ectogenic nature of the bacterium, *x*, is proved by its disposition to develop under favourable conditions of time and locality. Professor Pettenkofer does not accept the diblastic theory of Naegeli, which supposes that infectious diseases dependent on the circumstance of time and locality are due to the action of two micro-organisms, *x* and *y*, *x* being that emanating from the patient, and *y* being produced by the locality. The combination of the two in the human economy is the cause of the disease. By having lived in a locality containing *y*, a disposed subject can be infected by *x*, and *vice versa*. The author is rather monoblastic in his belief, and believes that the bacterium *x*, of an ectogenic nature in cases of cholera, is spread by human intercourse.

3. *Hospital and barrack epidemics as a proof of the contagiousness of cholera.*—In spite of the universally observed fact that comparatively few physicians and nurses, in their attendance on the sick, contract cholera, the probability of the disease being contracted through this avenue has become more and more feared. This has led to the construction of separate hospitals for cholera patients. True it is that Griesinger's statistics show in Moscow a mortality of from 30 to 40 per cent. among the nursing staff in 1830, whereas in the city the mortality was 3 per cent. of the population. Romberg's hospital in Berlin shows in 1831 a mortality of almost 50 per cent. among the nursing staff. In the Paris Charité, in 1849, the deaths amounted to 16 $\frac{2}{3}$ per cent., whereas 40 per cent. of the population died. The hospitals in Milan, Toulon, and Vienna also show an increased rate of mortality among the nurses. In Munich certain hospitals in particular localities show an increased death-rate among the nurses. In the case of the latter the author shows that these hospitals were situated in those parts of the city where cholera was also epidemic at the time. Other hospitals in Munich situated outside of these localities showed no mortality at all among the nurses, though they were in constant attendance on cholera patients. In both sets of hospitals the same variety of patients existed, also the same conditions of cleanliness and disinfection. The same order of nuns officiated as nurses in both. Porta supports the author in the above facts. If in Moscow and other cities the hospital nurses show such a rate of mortality, the cause must be sought, not in the patients, but in the *situation* of the hospital. The hospital situated in a cholera locality becomes, with all its inmates, subject to the same influences as other houses and their inmates in these localities. If the hospital is not a "cholera house," the patients will not be a menace to the nursing staff. Bryden has proved this to be the case with India. By studying the history of the spread of cholera in barracks in Munich, Professor Pettenkofer has found the same conditions to hold true as in the case of hospitals. Barracks and hospitals, moreover, situated in cholera districts showed the same course of the disease among themselves as was the case with houses outside the hospitals, but also situated in these districts.

4. *The linen of cholera patients as a source of infection.*—If we believe that the excrement of cholera patients is a source of infection, we must also believe that clothing soiled with the same is a source of infection. Professor Pettenkofer, in 1854, himself called attention to facts which would seem to strengthen the belief that soiled clothing is an entogenic source of infection. Koch, in explaining the striking immunity of Lyons from cholera, attributes this to the fact that the clothing is not washed in the city, but in booths situated on the banks of the Seine and Rhone. But in Zurich, Vienna, and other places, where similar conditions exist, no such immunity from cholera has been observed. Koch also traces the cholera epidemic in the washing village of Capronne to soiled clothing of cholera patients—refugees from Marseilles to Lyons. But at this time there were about 1000 cases of cholera in the city of Lyons; and it is possible also that the epidemic in Capronne was due, not to the stained linen in an entogenic sense, but to a something adhering to the linen which, ectogenic in its nature, was thus conveyed from Lyons to Capronne. Moreover, such carriers of germs may be other articles than linen. It is not an absolute condition that such clothes be washed by others to convey the disease. Cases exist where clothes simply handled in localities and at times favourably disposed have conveyed the disease. If sick persons coming from cholera districts have proved more infecting than healthy persons coming from the same places, it must be because the sick person has come into closer contact with the local ectogenic infectious matter. But healthy persons coming from cholera districts may also be carriers of this infectious matter. Drasche, more a clinician than an epidemiologist, cites cases where epidemics resulted from washing infected clothing, but which seem to be only hospital epidemics not connected with infected linen. The author, while admitting that clothes from cholera districts can cause "sporadic infections," denies that epidemics can result from such cause alone. In this connexion the author cannot agree with those—as Drasche and Monad—who attribute such a short period of incubation to cholera as half an hour. In 1854 this question, considered in Munich epidemiologically, was divided into two varieties of cases: (a) A person coming from a locality free from cholera to an infected spot was attacked within from two and a half to five days. (b) A person coming from a cholera district to one free from it, the next case resulting appeared within six or seven days.

5. *Cholera on ships.*—It has been proved that epidemics do not result where one or two isolated cases come from a cholera district to a locality free from the disease. We may suppose in such cases that the patients bring from the infected localities sufficient germs to communicate the disease to two or three others in the new locality, but the infectious matter (ectogen), not finding a suitable soil, does not thrive, and dies out, hence no general infection. Therefore it follows that the cases of cholera in the new locality were not infected by anything produced by the cholera patients there, but by the original infectious material brought from the cholera district. Such protected spots are naturally ships, and the author has always classed these among the localities unfavourably disposed to the development of the cholera germs. Ships in cholera districts have cholera on board, both where persons have come on board from the infected districts, and also where the patients have not been in such districts. Bryson shows that where ships have for a time been in the cholera district the disease has ceased to spread on board as soon as the ships have left these localities. Therefore when ships with cholera on board set sail for open sea the disease does not become epidemic, but remains limited to one or two cases. If such ships return to the cholera localities the disease also reappears (*Caledonia, Queen, Bellerophon*). According to Macpherson, ships sailing up the Hooghly and Ganges frequently have cholera on board when passing the cholera banks (one or more cases), but as soon as the ship reaches open sea the disease disappears. That ships are bad soil for the development of cholera is shown by the fact that only 11 per cent. of the ships leaving cholera ports have cases of the disease on board subsequently. The mortality on shipboard is only 1.22 per cent. (Robert Lawson). Cunningham and Lawson explain the occurrence of cholera on ships, not by the infection of the healthy through the sick, but by the supposition that something is brought from the land on shipboard, and here develops or comes in contact with those on board who have not been in the cholera districts. In

this connexion it would be interesting to call attention to the fact that Cunningham records that from 1861 to 1869, of 129 ships sailing from Calcutta, carrying 50,604 natives to America, in only twenty of them (16 per cent.) did cholera appear, and on only two ships were there more than five cases of cholera during the voyage. In the year 1873, which was a cholera year, not merely for India but also for Europe and America, we have the following statistics. In this year about 400 ships brought 152,135 persons from cholera-infected districts and localities of Europe to New York. Only four of these ships had cholera on board. An interesting instance of these four is the *Westphalia* from Hamburg to New York. On her journey two persons died with cholera. On her arrival in New York only nine cases of cholera were found on board; of these eight recovered subsequently in the quarantine hospital. All the eleven cases of this ship belonged to one family. Another interesting case, for a long time unexplained, is that of the war-ship *Britannia*. The war fleets of the United Powers—England, France, Italy, Turkey—lying in the vicinity of Varna (1854) were visited by cholera. The French frigate *Ville de Paris* and the *Britannia* had a large number of cases on board. The English ship, wishing to check the spread of the disease, sailed for open sea; but the disease continued its severity, and in one night fifty cases of death were recorded. It was only after the ship had returned to Varna (where the cholera was at the time) and had transferred the troops and men to other ships that the disease seemed to become less severe, and then ceased without spreading further. The officers of the *Britannia* remained free from the disease. The immense mortality in one night was for a long time attributed to the fact that during the night the companion-ways were all closed, and, the air being very close, the disease was thought to have a favourable soil for its spread among the crew. But investigation has shown that the crew of this ship had probably most of them visited the sailor resorts in Varna, there came in contact with the infectious matter, conveyed it on board, and then became sick with cholera, as the fugitives from other cholera districts are on land. The officers of the ship, not having frequented the resorts or cholera localities, remained free from the disease. That cholera finds an unfavourable soil on shipboard can scarcely be denied. The cargo of the ship also might be a means by which infectious matter is conveyed on board. Bad ventilation, uncleanliness, soiled linen, and cholera stools are not sufficient to explain the spread of cholera on shipboard, for cholera exists on ships where, as in the *Windsor Castle*, the hygienic conditions were all that could be desired. Ballast in the form of soil and sand may be regarded as the carriers of harmful matter, but ships themselves cannot be looked on as the carriers of infection. Koch and Virchow do not agree with the above views. The former, in support of the entogenic theory, shows that cholera is more frequent on transport ships than on merchantmen. But Professor Pettenkofer explains this apparent frequency by the fact that on merchantmen we are less likely to find the disease, in itself not frequent on shipboard, inasmuch as we have here a vastly smaller number of persons than on the transport ships; hence in the latter we are more likely to meet with instances of the disease. Cholera epidemics on shipboard are the exception.

6. *Spread of cholera through human intercourse on land.*—The Autochthonists do not believe in or admit the agency of human intercourse in the spread of cholera on land. Cunningham records that in India pilgrims very often travel in different directions from cholera-infected spots, and some die on their journey. But, independently of this fact, the epidemics always take a certain route. In places lying outside the line of route the cholera patients cause no epidemics, but in the epidemic direction they appear to be infectious. In studying the map of Europe, we find that the cholera epidemics do not in the least spread in the lines of travel. If we were to make a topographical map of the cholera in Europe from 1830-85, as Rheinhard and Günther have done for Saxony, we should see that cholera only favours special localities and avoids others. Railroads have changed nothing in the direction of the spread of cholera either in India or Europe. Nor does the cholera spread any more rapidly to-day than in the days previous to the introduction of railroads. Cunningham in India and Günther and Rheinhard in Saxony have shown that cholera does not spread in the direction of the railway network. If a person comes from a cholera district to one not infected, and cases

follow his arrival in the new locality, or even when epidemics result, it does not follow that this original case is the cause. The arrival of the first case and the outbreak of cholera in the new locality may be simply accidental, or be due to germs carried into this locality at another time and until now latent. The mobilisation of troops in a cholera locality, combined with the want which they bring with them, and also the consequent concentration of masses of people, tend to raise the death-rate, but the spread of cholera is not due wholly to this cause. In 1866 countries harbouring no troops were the seat of cholera, more so in some cases than localities in which cholera-affected troops were quartered. Moreover, following the line of march of cholera-affected troops through a series of forty towns, it was found that only four of these subsequently became the seat of epidemics; in others the disease appeared sporadically, and in twenty-three not at all. It is only where the cholera germ finds a favourable soil that it develops and causes an epidemic. That intercourse has some influence in the spread of cholera is not denied by the author. It has not the great influence attributed to it by the Contagionists.

A D D R E S S

DELIVERED AT

S T. M A R Y ' S H O S P I T A L,

AT THE

Presentation of Prizes, June 28th, 1886,

BY THE VERY REV. THE DEAN OF GLOUCESTER, D.D.
(DR. BUTLER.)

GENTLEMEN,—It must be the desire of any man who is honoured by the commission which I hold to day to say something which may be useful as well as acceptable to his audience. If you were listening to some member of your own great profession, you would doubtless expect some addition to your knowledge, derived from his insight and experience. You will now of course expect no such benefit. I have nothing to tell you that you do not know already. If I am able in any way to interest you, it will be because sympathy and respect can never be wholly disagreeable to those who receive them. Let me, then, at once try to find out at least one topic common to us all. My experience of life has lain chiefly with the young. I love to think of young men at the beginning of their career, while still a little "sicklied o'er" with the pallor of "hope deferred," and before they become too rudely embrowned by the sun of assured success. They are less interesting to me in what Mr. Canning calls "the brightness of meridian glory" than when for the time a cloud seems to brood on their imaginations and their hopes.

The keen interest of biographies lies not so much in tracing how men of genius find themselves gradually forming an "environment" exactly fitted to their gifts, as in noting how they develop gifts which were not originally conspicuous; how a sense of duty, or of ambition, or of sheer professional perseverance brings out powers of which they were almost unconscious. No doubt there is a pleasure in seeing genius move calmly and swiftly to its recognised goal. We like to read of the young Correggio standing before a masterpiece of Raphael and muttering to himself, with the consciousness of power, "I, too, am a painter." We like to read of Sheridan, after one of his early failures in the House of Commons, striking his head and saying, "I have got it in me, and, by Heaven! it shall come out." But I confess I find something even more stimulating in reading—first, the humorous praise given by a surgeon of St. Thomas's Hospital to Sir Astley Cooper: "Sir, it is of no consequence what instrument Mr. Cooper uses—they are all alike to him; and I verily believe he could operate as easily with an oyster-knife as the best bit of cutlery in Laundry's shop." I like to compare this fervid eulogy—not perhaps to be taken quite literally—with what Sir Astley wrote of himself and left among his private papers: "Sir Astley Cooper was a good anatomist, but never was a good operator where delicacy was required. He felt too much before he began ever to make a perfect operator." Have we here simply the unconsciousness of genius, the self-ignorance

of an artist whose ideal of excellence is surpassingly high and exacting? or was this great surgeon really right in his estimate of his own natural powers; as right and as instructive as Brodie afterwards, who in his Autobiography—a twenty years' favourite of mine—has told us this strange story: "A friend of mine, a craniologist, told me that he saw I had the organ of constructiveness much developed, and that this explained how I excelled in the use of my hands, and was an excellent dissector. There was never," says Brodie, "a greater mistake. I was naturally very clumsy in the use of my hands, and it was only by taking great pains with myself that I became at all otherwise."

Gentlemen, I a little hesitate to draw the moral from this candid avowal. I hardly like to conceive of it as even possible that anyone here may say, "I am naturally clumsy in the use of my hands"; but if there be such a man I would dare to say to him, Let not his diffidence be too much for his ambition. Let him not exaggerate the sentiment of pity for those unknown sufferers whom in self-accusing moments he would call his victims. Nay, let not the very irksomeness of the pursuit have too depressing an effect on his spirits, for has not Brodie said, again, that those who succeed best in a profession are those who have embarked in it not from irresistible prepossession, but perhaps for some accidental circumstance, and persevere as a matter of duty, or because they have nothing better to do? "They often," he adds, "feel their new pursuit to be unattractive enough in the beginning; but as they go on and acquire knowledge, and find that they obtain some degree of credit, they become every day more interested in what they are about." But enough upon this head. Let me carry you away, gentlemen, however suddenly, from the reverie of early depression to the heyday of recognised fame and usefulness, and let me bring the physician before you in another aspect.

What rich opportunities does the physician enjoy for the study of his fellow-creatures! I am thinking, of course, of the confidential relations between doctors and the families of their patients. The ear of the physician is the modern confessional. Who is afraid to trust him, if he be worthy of trust? What insight he gains into family histories, into the causes which prevent men from being of one mind in a house, of the secret humiliations concealed beneath a bold front, of the patient heroisms, of the unsuspected sins and even crimes, of the daily tragedies under roofs which to the world at large seem a very temple of prosperity. The thought that I would endeavour to leave with you, my younger friends, is this: If such is to be your position hereafter, what manner of persons ought you to be now in preparing yourselves for so high a trust? The qualifications for such an office are clearly not professional alone, unless, indeed, you are bent on demanding for "professional" training a range and a latitude ideal rather than conventional; unless, just as Cicero claimed for his ideal orator every branch of learning and every intellectual accomplishment, you are prepared to claim for the ideal physician not only all that gives richness to the intellect, but all that gives grace and beauty to moral worth. For, consider how much is required by one who is to be the confidential friend and almost confessor of hundreds of families. He must have in a high degree the gift of sympathy first implanted by nature and then systematically trained. He must have, further, wide knowledge of the world, a clear and sober judgment, an acute moral sense, and an absolute rectitude of purpose. Further, he must have the gifts which attract and inspire affection. Much of the power which doctors so happily wield in families is, I cannot doubt, to be set down to the gentleness of their manners. I suppose, my friends, we are all more or less rough at some part of our lives, and it would be strange if this moral hirsuteness, often found among the young in all departments of study, were conspicuous by its total absence in the student of medicine. The Lord Chesterfield of the hospital is perhaps seldom matured before twenty-two. Unlike the poet, "fit, non nascitur." But it is rare, I think, to find other than gentleness in the adult physician and surgeon, though there have been, and may again be, Abernethys and Jephsons. Gentleness may fairly be termed a characteristic—if you like, an acquired instinct—of your profession. I read of one whom you regard as one of the chief ornaments of your profession. "His manner in ordinary society was sometimes complained of as harsh or abrupt, and he certainly was occasionally neglectful of the mere trifling courtesies of life, and sometimes careless of refinement of punctilio. . . . But"—and this is what I love to read of so

than in the other, will produce anæsthesia in both sides of the body, but in a greater degree in the side opposite to that side of the cord where the lesion is most extensive.

Reading by the light of this conclusion the cases I have reported in the preceding lecture, it will be seen that in Cases 1, 2, 4, 5, 11, 12, and 13 (see THE LANCET, November and December, 1868), there was a lesion, almost entirely limited to one-half of the spinal cord, in its cervical region. On the contrary, there was a lesion extending to both sides in Cases 3, 6, 7, 8, 9, and 10.

3rd. *A lesion in one side of the spinal cord produces a paralysis of the muscular sense in the corresponding, and not in the opposite, side.*—We are not much advanced yet, as regards the physiological and pathological history of the *muscular sense*. We do know, however, that the conductors serving to give us an accurate notion of the state of contraction of our muscles are absolutely distinct from the conductors which give us the variety of painful sensations we may receive from these contractile organs. We know, also, that in cases in which an autopsy has been made, and the spinal cord found injured or diseased in one of its lateral halves, there was no alteration of the muscular sense in the parts that were not paralysed; and as the paralysis was on the side of the lesion in the spinal marrow, it is clear that the conductors serving to the muscular sense do not decussate in that nervous centre, or, in other words, that they remain, up to the brain, in the same side of the cord to which belong the muscles from which they come.

There is, therefore, a radical difference between the conductors which serve to give the peculiar kinds of sensation that belong to the muscular sense and those nerve-fibres which serve for all other kinds of sensation arising either from muscles or from joints, from the skin, or from other parts of the trunk and limbs. The conductors serving for the muscular sense behave just like the voluntary motor conductors, and seem to follow exactly the same course in the spinal marrow. These two sets of conductors, I repeat, do not decussate in that organ; while, on the contrary, the conductors of impressions of tickling, of touch, of pain, and of temperature, all decussate before reaching the base of the brain.

I can conclude, therefore, that in the cases I have related in which the muscular sense was lost or diminished, in one side of the body (Cases 1, 2, 3, 4, and 7, in THE LANCET, Nov. and Dec. 1868), there was a lesion in the corresponding side of the spinal cord.

SCIENTIFIC INVESTIGATION INTO THE CAUSES OF CHOLERA.

I. A REPORT OF INTERVIEWS WITH PROF. MAX VON PETTENKOFER AT MUNICH, Nov. 1868.

BY
DR. D. DOUGLAS CUNNINGHAM
AND
DR. TIMOTHY LEWIS.

[THIS and the two subsequent reports on interviews with Professors De Bary and Hallier were kindly placed at our disposal by the Director-General of the Army Medical Department. The writers, Dr. Cunningham, of the Indian Medical Service, and Dr. Lewis, of the British Medical Service, having passed through the Army Medical School with great distinction, were selected by the Senate of the School for special service in India, in connexion with a thorough investigation of cholera which has been lately ordered by the Government. Before proceeding to India it was thought desirable that they should receive special instruction on the methods of investigating the forms of fungi, (as so much importance has been attached to this point,) and they were accordingly directed to go to Halle and Jena to see Professors De Bary and Hallier. Subsequently they proceeded to Munich to talk over the best methods of investigating cholera with Professor Von Pettenkofer. On returning home they presented short reports of their interviews, which, as expressing the latest views on the subject

of cholera and of the fungoid theory of these eminent German Professors, will doubtless be interesting to our readers. We publish the Report on the interviews with Professor Pettenkofer this week, and those with Professors De Bary and Hallier shall follow. The Director-General informs us that nothing could exceed the interest the German Professors took in the inquiry, and they showed the greatest kindness in discussing the subject with the two gentlemen. The same may be said also of the Rev. Mr. Berkeley, Mr. Huxley, Mr. Simon, Dr. Thomas Thomson, Dr. Burdon-Sanderson, and others in this country, who all most kindly gave Drs. Cunningham and Lewis the benefit of their suggestions.—
ED. L.]

During our stay at Munich Professor Pettenkofer went over the subject of cholera with great care, and at the close of our interviews with him made the following statements as a summary of his views:—

In my opinion four conditions (*momente*) are essential in order to bring about an epidemic of cholera:—

1. A specific germ.
2. Certain local conditions.
3. Certain seasonal conditions.
4. Certain individual conditions.

I have not investigated the nature of the cholera germ as disseminated by human intercourse. I have only taken for granted that it exists in the intestinal discharges of persons coming from infected places. (*Vide* "Untersuchungen über die Verbreitungsart der Cholera," München, 1855.) My own investigations have been chiefly confined to the second and third before-mentioned conditions. Hitherto I have considered the human subject only so far as he is the bearer of the infecting matter of cholera, or of the germ of this matter; and have with facts contended against the pure contagionists, who declare that the infecting matter is produced by a process of multiplication within the bodies of those affected by the disease. My chief proofs of this have always lain in simple *facts* (independent of any theory) as to the spread of cholera over large districts. (See, for example, maps No. 8 and 11 in the Bavarian Cholera Report.) There are certainly *places* enjoying complete immunity from cholera, also *periods* of immunity. (Refer to my article on the Immunity of Lyons from Cholera, and the occurrence of Cholera on board ships, "Zeitschrift für Biologie," Bd. iv., pp. 400-414.)

The development of epidemics, and the immunity of many places, is totally inexplicable by the simple assumption of contagion from person to person. Observe the spread of epidemics along the course of railways and other ways of intercommunication indicated in the above-named maps. Nor are they to be explained by certain individual disposition of person (food, drinks, domestic arrangements, age, position, &c.); but the circumstances require, besides these, the existence of local and seasonal aiding causes, which have to be assumed.

Are these in immediate relation to the cholera germ itself, or to the individual disposition? Facts speak in favour of the first opinion only. 1. Persons from an unaffected place going to an affected one, are attacked quite as numerously and as soon as the persons who constantly reside in these places. 2. Cases are on record where a person from an infected district conveys (in a way not yet clearly ascertained) infecting matter into a place enjoying complete immunity from cholera; and there, by means of this limited amount of infecting matter, infects a few persons who themselves had never been subject to the local conditions of an infected place, and therefore could not have had their individual disposition altered by it. (See the article on Lyons, where the enigmatical example of cases of cholera in Stuttgart in connexion with the cholera in Munich are recorded, pp. 424 to 426; see also cases recorded of persons infected on board ship who had not been on land, p. 428.) Facts imperiously demand that we should consider that the "seasonal" and "local" conditions are intimately connected with the cholera germ, although they may in addition be in a condition to act on the individual predisposition also.

The infecting matter, in my opinion, is not a product of the human intestines, but of the soil. In so far as we consider the cholera germ of an organised nature, and capable of various degrees of development, it is possible—nay, very

probable—that there may be various degrees of infection. The distinction between cholera “germ” and cholera “infecting matter” must be noted. The cholera germ stands in the same relation to the infecting matter as the seed does to the fully developed plant.

In order to carry out a rational investigation of the specific infecting matter an exact study of the local and seasonal conditions of cholera epidemics is absolutely necessary, for it is only thus that one can find where and when this infecting matter is to be sought with any prospect of success. The differences in the local and seasonal occurrences of cholera are at present the only substantial grounds for inferring the existence of a specific cholera germ and infecting matter. Exact statistics of the local and seasonal movement of cholera are indispensable. (Vide the tables concerning Bavaria in the Bavarian Report, p. 50.)

Closer examinations of isolated cholera places disclose that even in these more limited areas as great differences exist in the local conditions (proportionately) as in entire countries and provinces. (Examples of such are given in his article on Lyons, p. 464, and in Pfeiffer’s article on Weimar in “*Zeitschrift für Biologie*,” part iii., p. 189.)

Whatever has hitherto been advanced against the view that the soil exercises an essential influence on the spread of cholera epidemics has proved untenable on closer examination; in illustration of which refer to pages 91 and 98 in the Bavarian Report on Kienberg, and to the remarks of Dr. Sutherland on the condition of the soil at Gibraltar and Malta in the Sanitary Reports.

The seasonal disposition to cholera consists, in my opinion, in a certain degree of humidity combined with warmth of the soil. Refer to the article on Lyons, pp. 465–468. Note the definition of “ground water” in pp. 468, 469; also a description of the influence of the rain season in Calcutta, Bombay, and Agra, in pp. 469–477, as well as the remarks on the influence of the Rhone on that portion of Lyons which is situated on alluvium, p. 484.

Here I may also allude to the evidence establishing the connexion which existed between the ground water and ileo-typhus in Munich. When the former was at its lowest level, the latter raged the most. (Vide article on the subject, with diagram.)

Individual predisposition consists, in my opinion, as a rule, in a proportionately excessive amount of water in the tissue of organs, and a deficiency of albumen. This subject is treated at length in the “*Zeitschrift für Biologie*,” part ii., pp. 92–97.)

In illustrating these views, the Professor entered at some length into a demonstration as to the grounds on which they are founded. In doing this he showed practically the method of estimating the percentage of the porosity of gravel, clay, and solid stone. He also showed the methods which he employs in ascertaining the temperature of the soil, and the level of the ground water.

ON

THE NUTRITIVE VALUE OF DIFFERENT SORTS OF FOOD.

BY BARON LIEBIG.

It has been said that if man could live on air and water, there would be an end at once of the notions master and servant, sovereign and subject, friend and foe, hatred and affection, virtue and vice, right and wrong, &c., and that our political commonwealth, social and family life, our inter-communication, trade, commerce, and industry, art and science,—in short, all that makes man what he is, would not be if he had not a stomach, and were not subjected to a natural law which obliges him daily to take a certain quantum of nourishment. It is therefore worth while to answer the question why in reality man eats and drinks, and what the substances are which, received in the body during a succession of years, have an influence on the duration of his life.

If eating had no other end but to satisfy appetite, one

might perhaps believe that by perseverance the habit could be got rid of. But hunger is the inner admonisher that tells us there is something wanting within the body, and that we must supply the deficiency. That we grow thinner from privation of food, that we are cold and our forces diminish, while on the other hand our weight and bodily powers are kept up, and even increased, by taking nourishment, are facts which everyone can observe in his own person. But what we do not comprehend is, that in spite of this renovation, our body does not remain as it was, but gradually dies away.

What we know of life does not help us to comprehend death, which still remains an inscrutable mystery. The animal body is a warm body, which emits warmth continually, and the heat thus lost must continually be replaced. It is, moreover, like a machine which daily performs a certain amount of work. The heart, or the muscles of the heart, work on uninterruptedly to keep up the circulation of the blood; the muscles of the chest to keep going the respiratory organs; and those of the entrails to maintain the worm-like motion of the organs of digestion. These are daily performances with which our will has nothing to do, while the mastication of our food, the motion of our limbs in walking and running, or otherwise working with them, are all dependent on our volition. The animal body has often been compared to a locomotive, in which, by air, water, and fuel working together, warmth and force are generated. Indeed, air and water are necessary conditions for the generation of heat and strength in the animal body, and food therefore may in a sense be considered as fuel: it has, however, other purposes also.

We all know how rapidly wear and tear ruins an engine; and that to put it in repair materials and work are necessary. The iron or copper which has to be renewed will not be replaced by giving the machine more fuel, and therefore an exterior human power—the power of many workmen—must co-operate and must be consumed, to make up the deficiencies of the machine, and set all right again.

Food does not serve to generate warmth and force only, as is the case with the steam-engine, but also to form and to increase the quickened parts of the animal body, and to reproduce those which are worn out.

If we keep in view these different purposes which food serves, it will be easy for us to form a decided notion of the nutritive value of different sorts of food. Physiology teaches us that all animated and plastic parts of the body arise from the blood, and that the component part of blood, which becomes fluid when heated, and is called albumen, is the material which serves for their construction.

All the different sorts of food of man, as well as that of animals, contain, without exception, substances which are identical, or nearly identical, with the albumen of blood. This constitutes an entire group of substances found in plants, partly in solution in the juices, partly deposited in the seeds, and which are found in greatest quantity in the cereals. The name given them is albuminate. In the nutritive process, albumen of the blood is formed from them; they are also called constituent nutritive substances, because they furnish the material for the formation of all the plastic parts of the body. From other organic substances they are materially distinguished, having an abundance of nitrogen, and by containing a certain amount of sulphur. The cheesy substance (casein) of milk; syntonin, the principal component part of the muscles; albumen, or that part of vegetable and animal juices which is soluble in heat; gluten of the cereals; vegetable casein in peas, beans, and lentils (legumin), all belong to the group of albuminates.

The component parts of human food and of the fodder of animals which are free of nitrogen, such as fat, starch, sugar, sugar of milk, &c., are applied in the vital process, principally—and in part exclusively—for the generation of warmth. They have been named respiratory substances, or warmth-generating substances. The food of men and animals contains, besides, a third class of nutritive substances, known as nutritive salts. These are the substances which remain in the form of ashes when the articles of food are burned: phosphoric acid, potash, lime, magnesia, iron, common salt, are their chief elements.

The albuminates and heat-giving substances are quite incapable of nourishing and of sustaining life if the nutritive salts are not present, and co-operating with them. Without the nutritive salts they do not give nourishment. The idea of a perfect sort of food must be associated with three conditions; it must contain a certain quantity of albuminates, and there must also be a certain proportion of heat-giving substances, and of nutritive salts. We may accordingly speak of meat,

in addition, but she could not do it, and the figures were meaningless hieroglyphics to her. She said she could not read anything so as to understand it; that for the most part she knew the words which were actually under her eyes, but always when she reached any one word she had quite forgotten those that preceded it, and hence could not catch the meaning of even the shortest sentence. She could name anything she saw, but had great difficulty in recalling the names of things which were not present or visible. I saw her again about nine months later. I learnt then that all her symptoms had disappeared in the course of a few months, but that they had recently returned. She had had recurrence of stammering, but that was not present now. She presented, however, all the old spasmodic twitching of the facial muscles, all the old difficulty in writing and in the recognition of letters, and all the old forgetfulness of names of things not before her. She could not recollect the name of the day or of the month, and she told me that a few days previously she had had, as she thought, a serious and interesting conversation with a lady whom she knew, and who called upon her next day and asked her if she had not been ill, for she had talked nothing but nonsense. The patient had occasional attacks of unconsciousness, which, for anything I know to the contrary, may have been either attacks of syncope or epileptic seizures. I have no sufficient reason, however, for doubting that the case was essentially one of hysteria; and that was, I believe, the view of the medical man under whose charge she was.

(To be concluded).

REMARKS UPON THE CHOLERA BACILLUS OF KOCH.

By EDGAR M. CROOKSHANK, M.B.LOND.

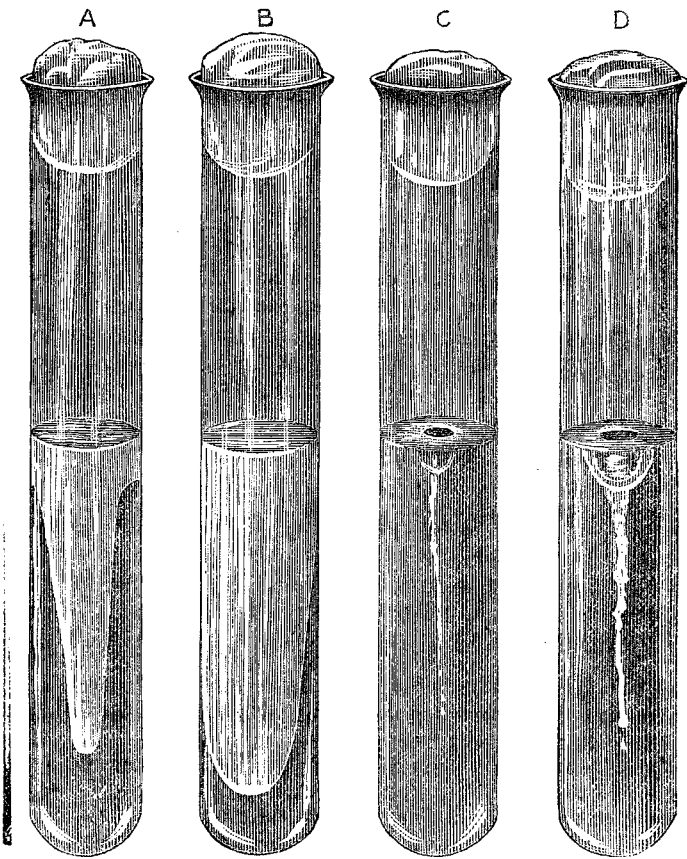
THE works of Koch and his collaborators, of Hueppe, Johne, Babes, and others, have made us acquainted with the most recent and trustworthy methods of bacteriological research, which are of such especial importance in enabling us to decide the burning question of the present day—the etiology of Asiatic cholera. The interest in the discovery by Koch of a cholera bacillus, and in his methods of its investigation, is increased by the approach of summer and the possibility of a fresh outbreak in Europe. Consequently the attention of the profession and of the scientific world has been drawn to this subject throughout Germany, Italy, Spain, and France—in fact, almost everywhere; while this interest especially finds expression in America in the demands in the medical journals for a thorough acquaintance with scientific bacterioscopy, and in this country in the recent discussion at the Royal Medical and Chirurgical Society. The result of this discussion, however, though convincing to those who know how to appreciate the importance of the arguments brought forward by Watson Cheyne, Heron, and Warden, is in several respects very unsatisfactory, more especially for those who do not fully realise the minute and searching methods employed in bacteriological research by the school of Koch, and in the face of contradictory opinions the medical public remains without any guide. Having through the kindness of Professor Virchow shared the bacteriological laboratory of Dr. Babes in the Pathological Institute of Berlin, and for some time almost entirely restricted my attention to the study of comma bacilli, it is my aim in these remarks, by describing in the simplest form possible the methods employed, to enable all to judge for themselves, and, I trust, not only to appreciate, but to be convinced of, the searching arguments brought forward by Koch to prove the essential rôle of this micro-organism in the causation of the disease, and to be provided with a practical and sure means of diagnosing true Asiatic cholera. By reference also to some of the most recent researches, and by commenting upon some of the points raised at the above-mentioned debate, one can clear up many difficulties which lie in the way of accepting the conclusions of Koch. Those who are acquainted with Koch's comma bacillus can easily demonstrate its presence in the intestine in all cases of true cholera. Hitherto nearly one thousand cases have been examined by capable observers, and have given positive results; and there does not exist one observation which is contradictory, nor can one find the same microbe in other

maladies. Physicians can therefore accept the presence of the comma bacillus of Koch as a pathognomonic sign of cholera, and in so doing it is an essential part of one's duty to become acquainted with its nature. The character of the comma bacillus is manifested in its habitat, its form, its cultivation, and its pathological action. The morphological characteristics have been fully described in the writings of Koch and in the reports of the Berlin Cholera Conferences, and need not be repeated; but inasmuch as it is often difficult to distinguish other bacilli which are present in the ejecta from the bowels and resemble it more or less in microscopical appearance, it is most essential to become familiar with the methods of pure culture which have been employed by Koch.

It is necessary, however, to obtain a characteristic culture, always to employ a 10 per cent. gelatine, slightly alkaline (Babes), and as it is difficult to prepare, it would be best to procure it ready made. Provided, then, with ready prepared sterilised tubes, a platinum needle (two or three inches of fine platinum wire fixed at the end of a glass rod), a few dinner- and soup-plates, half a dozen small panes of glass (6 in. by 4), a solution of sublimate (1 in 1000), a common dinner knife, some potatoes, methyl violet, a microscope, and its usual appendages, the medical man (especially the medical officer of health), who has not access to the elaborate apparatus of a bacteriological laboratory, has all that is required for conducting the simple methods which Koch often had recourse to and employed with success in India. For the plate cultures improvise a damp chamber by utilising a common dinner plate and a soup plate, both previously washed with sublimate, the plate having a piece of filter paper laid upon it, which is also moistened with sublimate. A small plate of glass, sterilised by carefully heating one surface over a spirit lamp, is placed on the filter paper, as level as possible, with the sterilised surface uppermost. Prepare two such chambers, then liquefy the gelatine-peptone broth in two tubes, and introduce into one by means of the platinum needle a mucous flake from choleraic discharge, and stir it well up; from this inoculate the second tube by introducing the needle five times, and stirring round as before. Lastly, pour the gelatine out on the plates in the form of a ring, so that every part can be brought into the field of the microscope. At a temperature of from 16° to 20° C. little specks commence to be visible in about twenty-four hours. They consist of isolated colonies, and when examined with a low power and a small diaphragm have the following characteristics. They appear as little masses, granular, and slightly yellow, and of a faintly yellowish-red tinge. They liquefy the gelatine, sink down, making a little excavation, at the bottom of which one recognises the little punctiform colony. By means of the platinum needle, with the extremity bent into a miniature hook, one can, under the low power of the microscope, select and pick out a typical colony and inoculate a tube. Another colony can be selected in the same way and well spread out with a needle on a cover glass, and stained after the method of Babes. This consists in putting on the cover glass so prepared a drop of a weak aqueous solution of methyl violet (B) and leaving it for a minute; then place the cover glass on a slide, and with filter paper remove the excess of liquid, and examine. A rapid examination is thus made of a pure culture of the bacilli. In about twenty-four hours the tube-culture commences to display its absolutely distinctive appearance. Liquefaction sets in slowly, commencing at the top of the needle tract around an enclosed bubble of air, and forming a funnel continuous with the lower part of the needle tract, which preserves for several days its resemblance to a white thread (Fig. c, d). In about eight days, however, this too is liquefied, with the exception of the deepest part of the needle tract, which disappears only at the moment when all the rest of the gelatine underneath it is liquefied. Babes has shown that the cultivation on an oblique surface of agar-agar peptone-broth is also quite characteristic. By inoculating the surface vertically with the needle, in about two days a culture is developed in the form of a plate, semi-transparent, and with well-defined margin, while the liquid which exists at the bottom of the oblique surface becomes milky. If one compares this with the behaviour under similar circumstances of other comma bacilli, the difference is in all cases easily appreciated. The potatoes are prepared by removing eyes and rotten spots, well washing, soaking in sublimate for half an hour, and then steaming for the same length of time. Holding them, with

the fingers previously dipped in sublimate, they are cut in two with a knife which has been sterilised by heating in the flame of a spirit lamp, then placed in damp chambers extemporised as before, and inoculated by smearing the micro-organisms over the surface with the flat of the knife. Koch's comma bacillus will only grow at the temperature of the blood (37° C.), forming a slight brown transparent layer.

If, on the other hand, we are dealing with a case of cholera nostras, we shall by the same procedure produce the characteristic biological appearances of the Finkler bacillus. In the tube culture we find that the gelatine in two or three days is liquefied along the whole tract of the needle puncture, excavating the gelatine in the form of a conical sac, and resembling in some degree the finger of a glove turned inside out (Figs. A and B). In glass plate cultures the colonies have a slightly yellowish-brown colour; but they are larger and rounder than the colonies of Koch's bacillus, have a well-defined border, and under a higher magnifying power a distinctly granular appearance. They liquefy the gelatine much more quickly, and give rise to a penetrating odour and rapidly become irregular in shape. Even in two or three days the gelatine on the second plate with only a few colonies is altogether liquefied. On agar-



PURE CULTIVATIONS IN GELATINE-PEPTONE-BROTH.

- A. Finkler's bacillus, twenty-four hours old.
 B. " " two days old.
 C. Koch's cholera bacillus, twenty-four hours old.
 D. " " " two days old.

agar the Finkler bacillus forms a white layer much more quickly; the liquid at the bottom of the oblique surface also becomes milky, but after a certain time a brown, coffee-coloured layer appears, which is very characteristic. On potatoes the Finkler bacillus grows rapidly at the ordinary temperature of the air, the culture has a brownish colour with a whitish border, and the surface of the potatoes appears to be eaten in.

Other comma bacilli differ in that they cannot be cultivated on gelatine, with the exception of a comma bacillus very frequently found in water. This liquefies gelatine very slowly, and forms on its surface a white pellicle (Babes). The last link in the chain of evidence is afforded by the effect of the inoculation of a pure culture of Koch's comma bacillus in animals. Nicati, Rietsch, Koch, Ermengen, and Babes have inoculated the bacilli with success in the duodenum. Babes, who used all practical antiseptic precautions, had three successful cases out of six; the guinea-pigs, which were the animals experimented upon, exhibited at the post-mortem examination the appearances produced by cholera without any trace whatever of peritonitis or putrid infection. In the intestine, in the mucus typical

comma bacilli were found and pure cultivations obtained. Recently it is reported that Koch, by a new method, without forming any lesion, has produced choleraic symptoms, without exception, in a great number of animals. Koch and Babes have also studied the pathological appearances produced by the cholera bacillus. They have found in all cases, more or less, in the intestine that the epithelium to a great extent is missing, the superficial tissue necrosed, the glands dilated, their epithelium swollen, in the glands and sometimes in the inflamed tissue more deeply, and in the necrosed parts they have established the presence of the micro-organism. In internal organs of chronic cases affected with parenchymatous degeneration, round or oval bacteria are sometimes seen; they are also pathogenic (Babes), but these latter are not in any way characteristic of cholera, but are considered to be merely the result of septic complication.

From these investigations we are led to the following conclusions:—That the presence of the cholera bacillus of Koch is quite characteristic of cholera; that the comma bacilli of Koch exist in the tissue of the intestine; that they are, there is every reason to believe, the actual *causa causans* of cholera. With regard to the second statement, one can only conclude that if observers have failed and do fail to find the bacilli in the tissues, it must be that either they are not masters of the technique, or that they have not sufficiently sought for them. The method employed by Babes is to cut very thin sections of a recent case of cholera, if possible immediately after death, and especially from the immediate neighbourhood of a Peyer's patch. Then leave the sections for twenty-four hours in a watery solution of fuchsin (fabrique de Bale), wash a little in sublimate (1 in 1000), pass rapidly through alcohol and oil of cloves, thoroughly dry with filter paper, and preserve in Canada balsam. From material supplied by a case examined by Babes in Paris I have also succeeded, by closely following his method, in staining the bacilli in the tissue and demonstrating their presence with a Zeiss $\frac{1}{2}$ oil immersion lens. With regard to the inoculation of animals with cholera bacillus, one must confess that it is not easy to avoid peritonitis or accidental infection, but it is easy to be convinced that if one does avoid these errors one will have a certain number of cases which give positive results. That Dr. Klein has so often encountered accidental infection may account for his want of success. Emmerich has asserted that he has found in the internal organs a special microbe which causes choleraic symptoms by inoculation. This, however, is in direct contradiction to all other observers, who have shown, like Babes, that if there be a pathogenic microbe present in the organs, it has penetrated through the necrosed tissue of the intestine, and that it is nothing more nor less than a pyogenic or septicæmic bacterium.

FERROCYANIC TEST PELLETS FOR ALBUMEN.

By F. W. PAVY, M.D., F.R.S.

THESE pellets afford a ready, portable, reliable, and delicate clinical test for albumen. They require no spirit lamp to be employed and supply information which can be accepted without needing the application of any other test for correction or corroboration. When albumen is present it does not fail to be indicated by the production of a precipitate; and, on the other hand, when a precipitate is produced albumen, as far as I yet know, is the only principle that occurs in the urine to occasion it. With these qualities the ferrocyanic test commends itself as specially adapted for general use in medical practice.

The sketch on the following page shows the portable character of what is needed for the application of the test. The whole appliance can be carried in the pocket like a pencil case, which it only to a trifling extent exceeds in size. There is a celluloid tube (C) for holding the pellets, and this fits into a glass tube (B), which is employed as the test tube. No heat being required for the application of the test permits the glass tube to be made of sufficient thickness to be carried in the pocket without danger of breaking. The celluloid tube has a partition inside to keep the acid and ferrocyanide pellets separate, and at either end there is a screw top. The glass tube is provided with a metal cap (A),