

Lecture on the action of the cholera poison on the body, and its nature and history outside the body : delivered at a meeting of the Bengal branch of the Brit. Med. Assn. on the 19th January, 1870 : and the discussion of the subject on the 31st January and 8th February, 1870 / by John Murray.

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LECTURE

ON THE

ACTION OF THE CHOLERA POISON ON THE BODY,

AND ITS

NATURE AND HISTORY OUTSIDE THE BODY,

DELIVERED AT A

MEETING OF THE BENGAL BRANCH OF THE BRIT. MED. ASSN.

ON THE 19TH JANUARY, 1870;

AND

THE DISCUSSION OF THE SUBJECT

ON THE 31ST JANUARY AND 8TH FEBRUARY, 1870.

BY

JOHN MURRAY, M.D.,

Inspector-General of Hospitals, Bengal Medical Department.

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1870.

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DR. MURRAY'S LECTURE
ON
CHOLERA POISON ON THE BODY,

DELIVERED AT
THE BENGAL BRANCH OF THE BRIT. MED. ASSN.
On the 19th January, 1870.

GENTLEMEN,

I HAVE been requested by several of my professional brethren, whose opinion I highly value, to deliver a lecture on the nature of the Cholera Poison, and its action on the human body; as a supplement or key to my practical report on the treatment of the disease, already in your hands. It may possibly be asked why these views were not given as a preliminary to those on the treatment of the disease? My answer is, that the enquiry then conducted was limited by the orders of Government to practical points, and as the views I now offer must, to a certain extent, be theoretical, they may be wrong, the weakness or fallacy of my reasoning on theoretical points might have been reflected on remedies that are valuable. It is possible that similar objections may be made to the limited nature of the present lecture, in reference to the restricted views which I now propose to submit to your consideration; but the subject is too comprehensive to be treated satisfactorily on one occasion, and other branches of it may be left for future discussion. One great object I had in view in consenting to deliver this lecture was to benefit by the criticism and opinions of so experienced and talented an assembly, who have resided long in what is

called in Europe, the cradle of the disease, more especially as on some points our views do not coincide.

In no disease are there more marked and characteristic symptoms, during one of its stages, than in cholera; but these specific symptoms, which are only found in one stage, are so dissimilar to those induced by the poison in an earlier stage, or milder form, that the latter have by many been overlooked. The value and importance of the early detection of the presence of the poison cannot be over-estimated; as we all know from experience, that after a certain stage, the vital organs are for a time little sensible to the action of remedies. They are paralysed by the presence of the poison, in the same manner as the voluntary organs are paralysed by the action of chloroform. It is in preventing the supervention of this stage, that most life has been saved; and the physician who can soonest detect the presence of the poison, will be the most successful in his profession.

The best method of ascertaining the symptoms induced by the presence of the poison, is to mark its action in its most intense form, before the other symptoms appear which follow the retention of those effete portions of the blood, which, for the preservation of health, require constantly to be eliminated by the secreting organs. This intense form of the disease has been noticed by many writers since 1817, but it is rarely seen in hospitals; in fact, it can only be observed by those who are searching for it among the people, as it proves fatal in from two to four hours. The patient falls down as if struck by a sudden shock, livid, cold, covered with clammy perspiration, with a broken voice and sinking pulse. The respiration becomes slow and intermitting, the only complaint is of uneasiness, burning, and sinking in the epigastrium; the intellect is clear; there is generally an evacuation of the ordinary contents of the bowels, and sometimes one or two of the peculiar congee-like discharges; but this ceases, the pulse disappears, intelligence cannot be roused, and life is at an end.

In the more ordinary form of the disease the patient is purged, generally in the early morning; the evacuations are at first natural, copious, and liquid, with a little tormina, and uneasy feeling in the epigastrium, approaching to heart-burn; the body is cool, the pulse quiet, and the intellect clear; but the countenance is dark, the eyes congested, the nails look blue, and there are occasionally flying cramps. This is soon followed by the vomiting of undigested food, eaten perhaps one or two days before—then copious congee-like evacuations from the bowels and stomach; intense burning pain in the epigastrium, low, sinking feeling, cold clammy perspiration, sometimes very profuse, feeble pulse, livid characteristic countenance, and cold breath. There is no secretion of bile or urine; the only gland that sometimes continues to act is the mammary in nursing mothers. There are generally cramps ascending from the toes and fingers towards the body or diaphragm; the higher they ascend the greater the danger implied; but they are often absent, and generally disappear in the worst cases, for some time before death. In these cases the vomiting and purging also cease; restless moving of the body comes on, whilst the patient lies with the eyes sunk, half-closed, congested and glazed; semi-conscious and occasionally wandering; when roused he still complains of the sinking, burning pain in the epigastrium, and calls for water. The pulse is not perceptible at the wrist, and there are long intervals in the breathing. It is wonderful how long this moribund state will sometimes last, before death closes the scene; and still more wonderful that in a few cases successful re-action takes place. There is great tendency to syncope or fainting, in collapse, and even after re-action has made considerable progress. The simple act of assuming the erect posture, or even sitting up in bed, has in many instances been followed by the patient falling back in a faint, from which he never rallied, or merely struggled on a little longer before death; in these cases coagula are found in the heart after death. In most cases there is an effort at re-action.

When it takes place, the burning pain in the epigastrium disappears, the restlessness subsides, the stools become coloured, warmth returns to the palms of the hands, the colour improves, the pulse become stronger, and sleep ensues. If, after this, the urine appears, a favorable prognosis may be given, provided no strong remedies have been exhibited during the previous stage, and that collapse has not lasted many hours. In such a case there are no marked sequelæ from the disease. I have, in several instances, seen the first evacuation after sleep, natural in colour and fœculent; but in general, especially when the collapse has been prolonged, the evacuations are first dark, sometimes pitchy, then green and yellow, and there is looseness for some time, and occasionally irritability of stomach. These symptoms are not essential, and may frequently be traced to the remedies employed. The sequelæ which follow collapse depend on different causes, either natural or artificial. The natural causes are imperfect re-action, when the secretion of the kidneys is not restored, when the liver acts imperfectly, and when uneasiness continues in the stomach with want of appetite. The pulse rises and becomes quick, the skin dry, the tongue coated, with sordes on the teeth; there is headache and wandering, sometimes delirium. Looseness continues, sometimes extending to bloody stools with tormina and tenesmus. Long protracted collapse, even though the re-action be satisfactory, induces slow convalescence. The secretions from the liver and kidneys are morbid, and the digestion imperfect. The accumulation of effete particles in the blood, caused by the suspension of their excretion during the protracted collapse, may have injuriously affected the brain, or other organs. This has to be worked off. Rest, gentle nursing and time, are required to allow the feeble organs to restore the blood to its healthy state. The artificial cause of these sequelæ are those remedies which would directly induce inflammation of the stomach, intestines, or brain; or indirectly suppress the secretion, or diminish the action of the kidneys, liver, or intestines,

by which the specific poison and the retained effete portions of the blood are eliminated from the system. The two remedies which, when given in excess, are likely to act most unfavourably in this way, are brandy and opium. The opinion of the practical Profession in India, on the exhibition of these remedies *in collapse*, in large quantities, is almost unanimous, that they are not beneficial, and in general, that they are decidedly injurious. Such, gentlemen, is my opinion, and I would it had influence to abolish a practice which has prevented many from surviving, who would otherwise have recovered from the attack of this terrible disease. But this is not the time to discuss the treatment of the disease.

We shall now consider one of most frequent forms in which cholera appears, *viz.*, premonitory diarrhœa, called by many "choleraic diarrhœa," and by some considered a distinct disease. We cannot over-estimate the value of correct information on this point. In the first place, it is a stage in which the system is still amenable to remedies, and the progress of the disease can generally be arrested. In the second place, the poison may be in process of elimination through these discharges, and the disease unwittingly disseminated over the country,—an evil which might be prevented by a correct diagnosis. Again, if the physician tells the patient that his disease is not cholera, should it advance to the next stage, the patient believes he has no chance of recovery, and at this critical period his heart sinks, and the despondency which ensues redoubles the power of the poison, and diminishes the hope of success.

Diarrhœa is always prevalent during cholera epidemics. It is painless, the evacuations are light coloured, copious, creamy, gradually becoming more watery, and devoid of feculent smell, with occasionally the peculiar smell of cholera perceptible. They may then become watery and colourless, the characteristic stool of cholera, with collapse and cramps. The period occupied by these changes varies from a few hours to several days. At first there is no appreciable difference between these

evacuations and those passed in ordinary diarrhœa, which may accompany disease of the liver, or kidneys, or be induced by eating unripe fruit, or indigestible food, which forms one of the most frequent exciting causes in cholera.

There are other appreciable signs of the presence of the poison at this period. There is numbness, or want of power, in the rectum, a want of appetite, a desire for stimulants, and sometimes uneasiness in the stomach, or heart-burn, and occasionally flying cramps. The secretion of urine is scanty, the countenance dark, and the eyes congested. The frequency of the stools may be diminished, and the consistence of them improved by opiates and astringents, without improving the colour, removing the tendency to recurrence, or curing the disease.

Diarrhœa is the first active symptom induced by the presence of the poison in the system; but previous to this, an experienced physician will detect its presence in what I have called the stage of "*malaise*,"—a feeling of depression of spirits, and want of appetite, with weight in the head, torpor of the bowels, scanty urine, and a want of brightness in the colour of the face. I have several times observed this state disappear suddenly after an inordinate flow of urine.

These are the earliest symptoms I have been able to trace of the presence of the poison; but in many instances, it appears to be present without exciting observable symptoms; if we may judge from the fact of individuals leaving an infected locality, apparently in perfect health, being attacked on the following or subsequent days in a distant place, where the disease had not previously existed. That the malaise, I have described, is caused by the presence of the poison of cholera, is an opinion strengthened by the fact, that during an epidemic attack when this feeling exists, at the action of a purgative, especially salts, will almost always be followed by collapse, cramps, and death.

We have now traced the action of the poison on the system from malaise to collapse, through diminished power

of the sympathetic nerve on the functions of the organs of organic life, inducing deficient secretion, or total suspension of excretion through complete paralysis. This diminished action produces want of appetite, with defective supply of gastric juice; the absence of which allows the natural changes of fermentation and putrefaction to take place in the food, rendering it irritating, and inducing diarrhœa. In ordinary circumstances this is Nature's cure for improper food; but at this time there is also present in the system a foreign body, which I shall now endeavour to describe. It may not be visible, but its nature can be traced by its effects. It is evidently vital, as it reproduces itself. It is infinitely small, like other lower vitalities, which produce fungi in fermentation, and vibriones, &c., in putrefaction. There are known chemical agents, called antiseptics, by which fermentation and putrefaction are suspended or restrained. It may, therefore, be inferred that there are agents by which this other vitality can also be restrained or destroyed. Nature provides a powerful agent of this character, *viz.*, the *gastric juice*; and it may naturally be inferred that it has a similar power over this analogous low vitality. The whole history of the progress of the disease tends towards this conclusion, and affords an explanation of the cause which induces trifling depression, and may develope in a few hours into violent collapse. In *malaise* the presence of the poison in the system is evident, but it frequently passes off without appreciable injury. The germ of the disease in this case is restrained in its reproduction by the presence of the gastric juice; but if during the presence of the poison the secretion of the gastric juice is diminished or suspended by exhaustion, mental anxiety, terror, or grief; or, along with the bile, it be removed from the bowels by a purgative, the restraining power being absent, the germ is allowed to multiply with its ordinary rapidity, and from tens becomes hundreds of thousands. That the germ can multiply in this excessive number, is known from large bodies of water becom-

ing contaminated in a very short period. This sudden development from the removal of the restraining power, affords an explanation of what long formed a difficulty in my mind,—how so virulent a poison exerts so little influence as it does in one stage. That the gastric juice is absent, in the most virulent form of the disease, is evident from the fact that food eaten one or two days before, is frequently vomited, or found undigested in the stomach after death. There is another fact in the history of the disease, which would be elucidated by this explanation; the most frequent hour of attack is in the early morning, about 3 or 4 a. m., when the process of digestion being long completed, the secretion of the gastric juice is, for a time, suspended. It is easy to imagine that if a small number of germs could diminish the secretion, which in its turn restrained their growth, and if the natural multiplication of these germs were allowed for a short period, they would be present in such numbers as completely to prevent the future secretion of the restraining power. The existence of such a specific germ is deduced from the production of all the effects which such a body, and only a living and reproducing body, could cause. That it is visible, or appreciable by the optic nerve, with the assistance of the powerful microscopes of the present day, is the conviction of many; and Hallier thinks he has detected it in the shape of a fungus, and Parkes saw a multitude of animalculæ in cholera stools. The most searching investigation is now being made in Calcutta, by Drs. Cunningham and Lewis, on this point. The presence of lively animalculæ in thousands, in cholera evacuations, is constant, and might lead to the inference of their being the cholera germ. The course of their existence passes through one stage, in which it has the form described by Hallier. It is intimately associated with the cholera germ, if it be not the real one.—“*Noscitur a sociis.*” My doubts of its being so rest on the fact, that a similar animalcule is found in every tank and drain in Calcutta, and in the evacuations in other diseases, though

in comparatively trifling numbers. At the same time I do not consider that the individuality of the germ depends on its being visible. There are many agents that cannot be seen that are appreciable by the olfactory nerve. Gases or vapours, that are invisible, yet may cause death, or lingering illness.

For practical purposes we have gained an important point. It may reasonably be inferred that this animalcule or infusorium thrives best where the cholera germ is present, and that whatever acts on it, will act in a similar manner on the cholera germ. A series of most interesting experiments has been begun to investigate this point. All ordinary antiseptics have been used, and also all the remedies recommended in cholera. Their action was uniform, and the result curious, and likely to lead to valuable consequences. In the first instance, the remedies were applied to the animalcule in its lively state, and the effect watched under the microscope. In some cases it was sudden death, in others the motion slowly but certainly ceased ; in others they turned round about for some time, and then ceased to move ; whilst with other supposed valuable remedies they continued lively and unaffected. In estimating the value of these experiments, it must be borne in mind that they were conducted in glass tubes ; whereas in the human body, they have to be conducted in the stomach and intestines. The effect of these agents was very curious, and watched with great interest. The gastric juice was first used in the shape of *pepsine*, a neutral, or rather alkaline preparation. For some time it had apparently no effect, but gradually the motions diminished, though they had not entirely ceased, in half an hour. The most interesting experiment was the addition of a very small quantity of acid to the pepsine, which renders it more like the natural gastric juice. The suspension of animation was instantaneous, more rapid than from any of the other agents that were employed ; the outer coat of the corpuscle was then dissolved. When bile was diluted and applied, the motions gradually subsided ; and when applied undiluted, the

cessation was immediate ; there was no solution of the germ, as under the action of pepsine ; in fact, after an interval of three days, the lively action returned. The effect of the Cholera Pill was curious ; for some minutes there was little change, but gradually the liveliness subsided, till in from five to eight minutes only one or two continued in motion, and in ten minutes all were quiescent. It was here I noticed the whirling round before death. The bodies remained in a quiescent state ; but were not dissolved as under pepsine, or acetic acid, which was the next remedy in rapidity and power. Carbolie acid, when strong enough, acted instantaneously in the glass tube ; but it is too strong to put ^{in the human stomach} with impunity. Sulphuric acid had no apparent action unless combined with pepsine. In exhibiting these antiseptic remedies to the human subject, their influence on the secretion of Nature's antiseptics—gastric juice and bile—must be considered. If they diminish these secretions, ^{secretions it is questionable if the} it is questionable if the substitute would prove more powerful in arresting the development of the germ. Even remedies which have no direct action on the germ, may prove powerful cures of the disease, by stimulating the secretion of the gastric juice and bile. This will account for the action of many remedies strongly recommended.

Another series of experiments was conducted by adding a little of the congee stool to tank water, and to separate specimens of this in test tubes, adding the various antiseptics and cholera medicines, and watching the effect on the reproduction of the animalculæ, compared with that in a standard test tube, to which no addition had been made. These experiments are still in progress under Drs. Cunningham and Lewis.

The explanation of action of the poison from excessive reproduction is very probable and satisfactory in cases which terminate fatally ; but in many cases the violent prostration, or collapse, suddenly ceases, and the ordinary healthy state of the body returns in a few hours. This may be explained by sup-

posing that the symptoms are caused by the movements and living action of the germ ; and that on these movements ceasing, the symptoms would subside ; but this cessation could not arise from the re-appearance of the gastric juice, for in collapse, none is secreted. In the innumerable cases which recover without use of medicine, *Nature* alone must supply the remedy. Simultaneously with the cessation of the collapse and commencement of re-action, there is a change observed in the character of the evacuations, showing the presence of *bile*. Could this be Nature's repressing influence ? *Post-mortem* examinations show that there is bile accumulated in the gall bladder, which, in the most intense cases, is often much distended. The bile evidently has not been allowed to enter the intestines, from some unknown cause. If this cause were spasm of the *ductus communis*, which would give way or cease, on the total paralysis of nervous system when death approaches, the bile might then escape, and entering the intestines come in contact with the active germ, and cause its death, or suspend its action ; and reproduction which, having been the cause of the collapse, would thus allow the restoration of the natural functions and of health.

The primary action of the poison is on the nervous system, and limited to that portion supplying the organic life. The secondary action is on the mucous surface of the intestines. I attribute the diminished secretion and excretions to diminished nervous energy ; and the feeble action of the heart to the same cause ; which when intense, induces total suppression of the functions, or paralysis of the sympathetic and ganglionic nerves. The term paralysis is generally associated with loss of muscular power, and may be considered inappropriate where the muscular contraction of the intestines forms one of the most prominent symptoms in one stage of the disease, after the other functions of the sympathetic have been suspended. But they yield in progression—first, the secretion of gastric juice—then of bile—then of urine—then the purification of the blood in

the lungs—and last of all, muscular action. Here the name of paralysis is appropriate, and the meaning of partial paralysis explained as indicating *diminished functions*. The secondary action of the poison I consider to be on the mucous surface of the intestines, causing irritation of the villous coat by the destruction of the epithœlium. This arises from a change in the course of the vitality of the cholera poison, from a state of dormancy or inactivity, to one of active life and reproduction : somewhat analogous to that of the *cysticerci*, which are quiescent and unproductive in the body, until they reach the mucous coat, where they reproduce. This is an increased, not a paralysed action ; paralysis here only comes on with the final expiring effort of Nature ; when the spasm of the *ductus communis*, which retained the bile, gives way, and may permit the oozing out of the accumulated bile, and allow re-action by discharging this vital, active antidote to the poison.

The primary action of the poison is considered, by some who have thought much on this disease, to depend on spasm of the extreme arteries, and that the vomiting and purging indicate spasm of the muscular coat of the intestines ; and the spasm in the *ductus communis*, and ureters, causes the retention, not suppression, of the bile and urine. One of our most intelligent writers, Dr. G. Johnson, considers that the primary action is spasm of the pulmonary artery, impeding the circulation in the right side of the heart ; and that the lividity in collapse is caused by the impeded venous circulation, and “ that when this is not present, lividity is absent, and the lips of a patient in pulseless collapse are often as florid as in perfect health.”

This must be a different disease from what we have in India, where I have never seen florid lips until re-action has set in. The *post-mortem* appearances, described by Dr. Johnson, in the heart and lungs, are in accordance with my experience ; but the gorged state of the right side of the heart may depend on diminished power of the heart to empty itself, as much as on

increased power of resistance to the passage of the blood through the pulmonary arteries. A theory to be correct must explain all the symptoms, and not be incompatible with the course of the disease. This theory does not explain all the symptoms of the disease, either in its milder development of *malaise*, when there is no oppression of breathing, or in its most intense form, when there is neither vomiting nor purging. In the former there is torpor of the intestines, with a dull dead feeling in the abdomen, well-known to all who have had much experience in cholera epidemics, which contra-indicates active spasm. It is evident that the muscular contractility of the intestines and *ductus communis* survives the failure of the secretions, in the milder form and intermediate stage; but this also subsides as the disease approaches to a fatal end. I have not observed any priority in the symptoms in the lungs to those in the liver and kidneys, or to the feeble action of the heart. The circulation can be restored, in the most advanced stage, by the transfusion of saline fluids, the pulse revives, colour and warmth return, the spasm of the vessels in the lungs is no longer present; but health is not restored, the abdominal secretions—the bile and the urine—do not appear, and death ensues. This experiment shows that there is something more than the diminution of the watery part of the blood, required to explain the course of the disease.

The *post-mortem* examination of those who die from the most intense form of the disease shows no signs in the intestines, different from what is found after sudden death from lightning, with the exception of the presence in the duodenum and jejunum of a whitish fluid, which, when diluted with water, resembles congee evacuations. When death takes place after partial re-action, there is often a pinkish tinge over the intestines; they feel doughy and thickened, with the mucous coat abraded, and deficient in epithelium. Where re-action has been completely established before death, there are often signs of inflammation of the mucous coat, with

ulceration. These secondary signs are not always present, yet they are considered by some writers as indicating that the disease is essentially an inflammation of the mucous coat of the intestines. As the ordinary symptoms of such inflammation are totally wanting in the earlier stages of the disease, and the morbid changes are not found in its most intense form, it may be inferred that this state supervenes as a secondary change, induced by the impurities retained in the blood, by irritation from the vitiated contents of the bowels, or from stimulant remedies exhibited, as well as from the action of the poison on the epithœlium. These signs are most prominent after protracted sickness. The rapidity with which convalescence takes place in slight cases, or when collapse has not long endured, contra-indicates any extensive inflammation of the bowels. In the stage of malaise, the employment of depletion, or depressing remedies, is most dangerous, often leading to collapse and death.

Having described the action of the poison on the system, we shall now consider how it enters. This is one of the most important points of the enquiry, embracing as it does the contagious or non-contagious nature of the disease; on which there were formerly very opposite or antagonistic opinions. More extended observation has clearly demonstrated, by an accumulation of evidence from independent intelligent observers all over the world, that the disease is communicable, or transmissible, from a diseased person to a healthy one. There are doubtless many apparent exceptions to this law; but negative evidence may be merely darkness, or want of light, which prevents the object being seen. From what we have learnt of the action of the poison when under restraining influence, it is easy to conceive the poison entering the system, and being so held in check by natural processes, as to be passed off without inducing any marked symptoms. There are remedies which assist Nature in this stage. There is also the curious fact that habitual contact renders the system less susceptible to the

influence of this poison; in the same manner as by habit, a man may enjoy swallowing doses of arsenic, corrosive sublimate, or opium, which would cause death to ordinary individuals. The exemption of physicians and hospital attendants, which is most marked in Calcutta, where the disease is endemic, and also elsewhere in cholera epidemics, may be explained by this theory, which is strengthened by the fact that visitors or strangers, coming to a place where cholera is epidemic, are more liable to suffer than the residents who have been some time under the influence of the attack. This immunity should encourage those whose duty takes them to the bed-side of the sufferers; but to allow those to approach whose presence is not essential, is to expose them to needless danger.

In this disease the morbid symptoms are produced by the presence of a specific foreign body in the system; and, until that is removed, health cannot be restored. This implies the entrance of the foreign body and its exit. The entrance may be through any of the natural channels of communication between the body and the outward world, *viz.*, the skin, the lungs, or the bowels; and there is strong evidence that it may enter by any one of these. The handling or wearing the clothes of a person who suffered from the disease, has, in many instances, been followed by attack; this would indicate the skin as the channel. Visiting a cholera ward, attending cholera patients, or being present at funerals, are often followed by attacks. I have observed numerous illustrations of this, and suffered myself from the first two causes. Many instances are recorded of attacks following visits to barracks, or camping grounds, previously occupied by cholera patients, or using privies which cholera cases had used. A peculiar smell has been noticed, and places situated in a line with certain drains have been observed to be liable to attack. These cases indicate the lungs as one channel of entrance.

But there is every reason to suppose that the most frequent channel is by the intestines, and that through the water that is

drunk. We have seen how readily the ally of the cholera germ propagates in tank-water ; the rapidity with which this takes place, and the people become infected, was illustrated in two particular cases in village tanks in the Hurdwar epidemic. There are many instances of wells being contaminated, and the water-supply of towns, causing those who use the water to be attacked. Rivers, into which sewers are discharged, during epidemics, are found to communicate the disease to the inhabitants of ships and houses in the vicinity. This has been illustrated in ships anchoring near the sewers in the Hooghly, when cholera is prevalent in Calcutta. The severe attack in the cantonment of Peshawur in 1867 may be explained in the same way. It is supplied with drinking water, from an open canal. A large party of Hurdwar pilgrims returning to Afghanistan, passed the station on the 18th and 19th June. The disease appeared on the 20th. The roads to the Khyber Pass and Kohat, and the Canal converge at the northern end of the cantonment. Cholera appeared at Jellalabad and Cabool, when this party reached these places, as also along the line of road before reaching Peshawur. The presence of the disease with the party was not detected by the authorities at Peshawur ; but their enquiries may have led those suffering to diverge from the line of road, and it would be in accordance with universal custom to go to the vicinity of water, for the purposes of nature. One of those affected pilgrims might thus have vitiated the stream, which would have affected the whole cantonment the following day. This view is strengthened by the fact that the Europeans in cantonments were attacked before the inhabitants of the city, which is very unusual in other stations, or in Peshawur in other attacks of the epidemic both previously and since.

When the poison enters by the stomach through the food or drink, it may directly cause collapse without the preliminary stages of malaise or diarrhœa.

We come now to the investigation of the channels through

which the poison escapes, or is eliminated from the system; and these are nearly identical with those by which it enters. In the stage which does not advance beyond malaise the poison is thrown off, digested or rendered inert without any marked symptoms, by the natural secretions and excretions of the body. I have noticed immediate relief in this stage, following a copious discharge of urine, and the same has been mentioned to me by others. As it enters through the lungs, so it may be passed off. It is reported that priests, in the performance of the last sacred duties of their profession, have, on several occasions, been affected: this may have been from the breath of those to whom they ministered. The disease spreading in crowded or ill-ventilated wards may arise also from tainted breath; but here there are other sources from which the poison may have emanated.

There is a characteristic discharge from the skin of profuse clammy perspiration, which has a peculiar sickly smell; and this may be considered one of the channels by which the poison escapes, especially as the clothes of diseased victims are a marked means of disseminating the disease.

The evacuations from the bowels contain the germ in all stages of the disease—from diarrhœa to collapse; in fact, the congee stools appear to be the last vital channel through which Nature endeavours to throw off the poison. The presence of the poison in these evacuations is proved by the facts which have been observed from their having been communicated to water, and thence spreading the disease; by direct experiment of giving them to animals either fresh or in various stages of preparation; and also by inference, if not directly proved, by observation with the microscope. On this latter point, I speak with reserve; the low minute vitalities which have been found associated with the discharges in this disease may merely be *allied genera* and not the real cholera germ. I can conceive the existence of an active poison which is invisible. There is one direct test by which a blind man may detect its presence in the

congee stools, *the smell*—a peculiar, mawkish sickening odour, which it is difficult to describe; but easily recognised by any person who has seen much of the disease. This peculiar smell was remarked by Jameson, and many of the earliest writers; and, though it has not been noticed in the recent European works on the disease, it was one of the subjects discussed in our meetings at Simla. Every one then allowed that there was a peculiar smell from cholera patients; though some considered it as merely cadaverous, and some thought they had observed the same in other diseases. Since then, I have frequently discussed this point, and I have always found that it has been perceived in the congee evacuations, whether from the mouth, or the bowels, and in the profuse perspiration of this disease. I have noticed it in some cases of diarrhœa, but I am not certain at what earlier stage it may be perceived. It would form a valuable aid in diagnosis, and I hope the subject will be carefully investigated.

The natural history of the poison, or germ, outside the body has now to be considered, and is a subject of deep interest and importance. As a preliminary step, it is advisable to ascertain how far the lower vitalities, associated with that of cholera, as found in cholera stools, are influenced by the natural and artificial agents applied experimentally. These allied vitalities increase rapidly in water, especially if impure. This is also the history of the cholera germ. Tanks or wells to which cholera evacuations have had access, have immediately produced the symptoms of the disease in those who used the water from them. It is evident in this case that the germ must have remained in a moist condition during its external existence, and that no modification of soil, as to porosity or dryness, could influence its course. Professor Pettenkofer imagines that the external life of the germ is dependent on the sub-soil water, combined with a peculiar porous soil, and that both these are essential to the development of a certain stage of its existence, which renders it again capable of exciting the disease. This is a comprehensive

theory, as there is sub-soil water wherever there is soil or land ; but the disease may spread where this remains unaffected by the rainfall from 30 to 300 feet below the surface. This is of daily occurrence. It is also difficult to connect this theory with the dissemination or communication of cholera on ship-board, hundreds, if not thousands, of miles from land.

When these lower vitalities were dried all motion ceased ; but after an interval of some weeks, on being moistened, the germs immediately renewed their activity. There are many instances of the cholera poison adhering to pilgrims, or travellers, across a dry and sandy country, and then communicating itself to the people at their homes ; and there are instances when the disease appears to be contracted on the line of road, which had been contaminated by preceding affected parties. In these cases the germs may have continued active, if the place or season was moist, or they may have been dry and dormant, till revived by water from the fresh arrivals. The epidemic disease has often broken out with intensity, after a fall of rain. The state of the temperature accelerated or retarded the growth of the animalculæ under observation ; they developed with great difficulty in a low temperature, and here is a marked analogy between them and the cholera germ. A certain degree of moisture also appears necessary for its development, and this is always present in the human body, and in it the germ may be carried, and developed, at all seasons, and is capable of communication from it, in the hot dry winds of India, as well as in the cold of a Russian winter. During the moist rainy season in India the disease spreads most readily. In Europe and America autumn appears best suited to its propagation.

There appears to be some influence independent of, and more powerful than moisture regulating the reproduction or diffusions of the disease. This is supposed by some theorists to consist of a peculiar epidemic state of the atmosphere ; without explaining in what this peculiarity consists ; but that it is very general—even extending from Ajmere to Kurrachee in one day

in 1845, and more recently all over India, Europe, and America, at the same time in the epidemic of 1865. There is no difficulty in tracing the progress of an attack through this medium, whether by land or sea, and in any direction of the wind or compass. I confess, I do not understand this vast, inappreciable impregnation of the atmosphere of a place, country, or quarter of the globe; the local and partial manner in which individual buildings, ships, or towns are constantly being affected, or exempted, precludes the idea of the attack being general in the atmosphere. Personal communicability is allowed to be a fact by some authors, but only secondary to the epidemic wave in the atmosphere, about which no explanation is given, and which (if it exists) is completely beyond human control. It is highly objectional to propound theories which would lead to the neglect of practices, or precautions, which have proved efficacious in warding off attacks of this epidemic. It is more profitable to pursue investigations which may lead to the perfection of those means which experience has proved to be useful, and to render their application more practical.

The disease is found to rage with great intensity in places where conservancy is neglected, and crowding, want of ventilation, and bad food prevail. Some suppose that these alone are sufficient to cause cholera, and that their removal by proper sanitary rules would obviate the appearance of the disease. This fallacy is less dangerous than the theory just noticed, in so far as the removal of these defects tends considerably to diminish the virulence of the attack; but it cannot prevent its being, in many instances, very severe. Witness the severe attacks at Peshawur, Lahore, and Meerut, in 1867, and at Peshawur and Allahabad in 1869; where all practical sanitary measures have been carefully attended to and carried out at great expense, under the most intelligent Medical Officers in India. It is highly desirable that the most efficient sanitary rules should be carried out. I do not depreciate their advan-

tage ; but some people are inclined to over-estimate the value of conservancy.

The conclusions at which I have arrived are, that the cholera germ, or poison, is an animalcule excessively minute, and probably invisible. That it frequently enters the system through the lungs, but oftener through the stomach, and sometimes through the skin. Whilst mixed with the blood in the circulation, it acts on the sympathetic or ganglionic system of nerves, as a sedative inducing, when in large quantities, paralysis or diminution of their functions, or suspension of the secretion of the organs supplied by them. That this animalcule undergoes an active change in its existence in the mucous surface of the intestines, where it multiplies in excessive numbers, probably living or subsisting, on the epithelium, and causing the congee-like evacuations. That in its most intense form there is total paralysis or suspension of the functions of the sympathetic. That there are two natural agents by which this development of the germ in the intestines is restrained : one before collapse, *viz.*, the *gastric juice* ; and the other after collapse, *viz.*, the *bile*, which had been shut up or retained in the gall bladder. That the germ passes out of the system in its earlier stage through the lungs and skin, and in its developed stage, through the intestines ; that when outside the body, it retains its vitality, and continues to pass through its various stages, multiplying and reproducing when in a suitable medium, of which moisture is an essential, and decaying animal or vegetable matter are important elements. That it may be retained in the dry or dormant stage for days, or months, out of which it may be rendered active by the addition of moisture under certain circumstances, of which season forms an influential element. That the reproduction of the germ may be restrained, or its vitality destroyed, by certain chemical agents, as fermentation and putrefaction are by antiseptics. I do not say what this specific germ is ; but classify it with other lower vitalities, which possess properties very much analogous ;

and indicate the means by which its dissemination may be restricted and guarded against, and its action on the body restrained. I hope I have thus afforded a connected view of the action of the cholera poison, and of the restraining influence of the *vis medicatrix naturæ*, which furnishes a guide to the treatment of the various stages of the disease. The uncertainty which now regulates the treatment, and the increasing and fearful mortality from the disease all over the world, show the importance of the subject. I entreat all who now hear me,—and I could not have a more experienced or intelligent audience,—to aid me in illucidating this most difficult subject. The eyes of the profession in Europe are directed to India for information; and I have every confidence that our reply will be worthy of the old Indian Service, over which I have the honor to preside.

Since this lecture was prepared I have received a very valuable work on cholera, written by one of our most diligent students of this difficult subject, Dr. Macnamara. Our concurrence of opinion on all the more practical points is, to me, most gratifying. I am particularly pleased with this author's observations in reference to the gastric juice. He is near the truth, but diverges from my road in supposing its influence to be merely chemical—an acid, which neutralises the alkaline contents of the intestines—instead of a vital agency destroying the poison.

The adjourned annual meeting of the Bengal Branch of the British Medical Association was held at the rooms of the Asiatic Society, on Monday, the 31st January, 1870, at 8½ P. M.

DR. EWART, President, in the chair.

The Secretary having read the proceedings of the last meeting, these were confirmed, *nemo contradicente*.

DR. EWART then said that the present meeting had been convened for the discussion of some of the more important points contained in Dr. Murray's excellent Lecture on Cholera,

delivered at the annual meeting of the Association held on the 19th instant. He would take the liberty of indicating a few of the leading questions which had been noticed in the paper.

First.—Much weight has been attached to the recognition of "*Cholerine*," or what English Physicians have called "*the preliminary diarrhœa*," so prevalent during cholera seasons, or epidemics, as simply a stage of the disease. There can be little doubt that Nature often succeeds in this way in relieving herself of the something, which, when allowed full and undisputed sway in the alimentary canal, results in the evolution of the subjective and objective symptoms of confirmed Asiatic cholera. Most of the physicians who practised in India, immediately after the great outbreak of 1817, regarded the "*premonitory diarrhœa*," or "*choleraic diarrhœa*," as an indication, during the spread of epidemic cholera, of the presence of the efficient cause of the scourge in the intestinal canal. In Calcutta, I believe, the general opinion of late years has been in harmony with this view. Dr. Murray, therefore, appears to me to be in accord with his professional brethren, when he says, that "we cannot over-estimate the value of correct information on this point. In the first place, it is a stage in which the system is still amenable to remedies, and the progress of the disease can generally be arrested. In the second place, the poison may be in process of elimination through these discharges, and the disease unwittingly disseminated over the country,—an evil which might be prevented by a correct diagnosis."

Secondly.—The abuse of the administration of opium and brandy is dwelt upon. The judicious employment of these agents, at appropriate periods of the disease, in certain cases, is doubtless beneficial, whilst excess of them, particularly in the collapsed stage, is prejudicial to recovery in the last degree. On this question Dr. Murray says:—"The opinion of the practical profession, on the exhibition of these remedies in collapse, in large quantities, is almost unanimous, that they are not beneficial, and in general that they are decidedly injurious."

Thirdly.—In the paper, the antiseptic power of the gastric juice and the bile is mentioned as a potential protective against attacks of cholera. This property has long been recognized by physiologists. The gastric juice, in this respect, appears to occupy the same relation to the body, as the lowest protozoal forms of animated beings do to the animal kingdom generally. The gastric juice, whilst affording a perfect solvent of the albuminous components of the food, arrests putrefactive changes and decomposition, and reduces these compounds to the condition best fitted for absorption and subsequent utilization in the nutrition of the tissues. The protozoa, in like manner, arrest the progress of the decomposition of organic substances, in their advance towards the formation of inorganic compounds, and fit them for being utilized by beings of a higher type and organization. In both cases, the supremacy of vital over mere chemical forces is apparent.

Fourthly.—Another question of immense importance to the human race is the contagious or non-contagious nature of cholera. If cholera be regarded as contagious in the same sense as typhus fever and small-pox, then the advocates for quarantine would have a strong case to go upon in periodically putting a stop to the commerce of large portions of the civilized world. But if it be proved that cholera is not a contagious disease in identically the same way as small-pox and typhus are acknowledged to be; that it is not directly conveyed from the sick, during an attack of the disease, to the comparatively healthy, but that it is in all probability, a communicable disease somewhat analagous in this respect to the typhoid fever of Sir William Jenner, then we have no alternative but to look upon quarantine as an apology for imperfect or neglected sanitation.

DR. MACNAMARA said, that with regard to the communicability of cholera he entirely agreed with Dr. Murray, and it seemed to him we were under no small obligation to Dr. Murray for having, since 1851 and 1856, so clearly enunciated this truth. The committee appointed by the College of Physicians, London,

had, in their report on the Epidemic of 1849, published in 1854, laid down the doctrines of the spread of cholera through means of human beings, for, in the majority of instances examined by them, outbreaks of cholera were preceded by the arrival of infected persons from other localities ; this seemed so constant a circumstance, that the College of Physicians appeared to have no hesitation in giving a positive and clear opinion as to the communicability of cholera through means of human agency ; but before this doctrine had been sent forth to the world, Dr. Murray had come to a similar conclusion, and although his views had hardly taken root among the profession in India, he has clung to them until they have become recognized by the majority of medical officers in this country. It appeared to Dr. Macnamara that unless we recognized this principle of the communicability of cholera, we could hardly explain the geographical distribution of the disease ; for instance, the circumstances of the inhabitants of many parts of Australia must be the same as those in Europe, and yet cholera had never been known in Australia, and this is to be accounted for by the limited trade between the two countries, and the length of the voyage (it has been ascertained that in 1868, only 17 vessels sailed from British India to Australia.) But take cases nearer home, in the Andamans, for instance, situated in the Bay of Bengal, and receiving convicts from India ; neither winds or any other media have ever produced cholera in these Islands, unless it has been directly imported by vessels taking cases of cholera from India, and landing them on the island, and then it has hardly spread, because the evacuations of all those effected have been destroyed as soon as passed. So again with the aborigines of the Rajmehal Hills : these people live actually in the endemic area of the disease, and yet are never affected with epidemic cholera in their houses, because, being outcast as far as all social ties are concerned with the inhabitants of the plains, the disease is not communicated to them by human beings, and all other influences fail to reach them ; they

remain, therefore, free from cholera till they come among us, and drink our cholera contaminated water, and then out breaks cholera among them, and kills them by hundreds, as we have seen too surely in the instance of those passing through Bengal to the tea districts. In fact, the study of the disease leads one to assert that epidemic cholera has never yet broken out in any part of the world, unless it has been traced, or may have been traced, by a train of cases to Bengal. If there be a case on record which disproves this fact, or rather contracts it, then I give up the point at issue, and say cholera may arrive *de novo*, or is not always traceable to a previous case of the disease, which in its turn may be carried back through a succession of cases to India. This is the broad principle upon which I base the doctrines of the communicability of cholera, and in this I agree with all Dr. Murray has written on the subject; but beyond this, I differ *in toto* with him when he asserts that the disease may spread by contagion. I assert that the contagiousness of a disease and the communicability of a disease are different things: the cholera, I believe, spreads by the latter means but not by the former. It is not necessary for me to enter here on the question as to what this communicability is, or the laws which I consider govern it; I am not called on to discuss my own views, but to consider those of Dr. Murray. As to the theory advanced by Dr. Murray, regarding the nature and *modus operandi* of the cholera poison, I may remark that three prominent difficulties occur to me in the line of argument.

1st.—If the disease is a germ or vitality it must grow and increase at the expense of the blood, into which he supposes it enters by the skin, lungs, or intestines. But we have no evidence of any such “vitalities” in the blood; we fail to find any poison or indication of any such changes as he hints at in the circulating fluid.

2nd.—Supposing such “vitalities” to exist; we know too little of the action or function of the sympathetic to assume, as Dr. Murray does, that it is paralysed and the secretion of gastric

juice thus stopped, &c., &c., the coeliac plexus may be removed, and yet gastric juice is secreted ; in fact, we know so little of the sympathetic, that I cannot argue from the unknown what effects would follow an alteration in an unknown action.

It seems to me with regard to these ideas, I might in like manner argue that the sun contains inhabitants, and that the people are given to such and such practices ; no one could deny this who had not been to the sun, but scarcely few would act upon a statement of the kind. And so with these "vitalities," and the "sympathetic ;" we know nothing about either with any certainty, and cannot, therefore, argue on the matter. Dr. Murray may be right, but he cannot demonstrate the fact, nor can I be sure that he is wrong. I must, however, refer to one point more. Dr. Murray says the poison of cholera is eliminated through the channels by which it enters the body, if so, I cannot but ask how is it then by restraining, and yet more by actually stopping, the discharge in the early stages of cholera, we cure the disease ? All those who have had long practice in this terrible disease know that if we can only stop the vomiting and purging in the first stage we save the patient. But if we admit this, which practice teaches us over and over again, are we then to suppose that by locking up the poison in the body we destroy its action and save the patient ? this objection seems to me to undermine the various theories built on the eliminating treatment of the disease.

MR. BARNARD said :—Mr. President and Gentlemen,—I will confine my remarks especially to the subject of contagion, as that appears to me to be the chief point at issue between Dr. Murray and the bulk of the medical profession in India. It is as difficult to define the special amount and kind of contagion of cholera, as it is to define what we mean by inflammation and the special bearing of the kind and degree on the issue of a case, whether the supposed poison, supposed to be in the system, produces all the symptoms *per se*, or through the degree of abnormal tissue action, its presence in the intes-

tine or the system may give rise to in certain constitutional states, or in certain textural predispositions, natural or acquired. We all know how degrees of contagion vary in different zymotic diseases, each having its own peculiar mode of communicability from man to man, constituting it a contagious disease. Phthisis has been, and still is considered contagious to an uncertain and very limited degree, never taken into consideration by English physicians; again others dispute the contagiousness of maculated typhus. Speaking of the exanthemata, Sir Thomas Watson says :—"You will hear persons disputing about the term contagion; but such disputes can only arise from the want of a distinct definition of the sense in which it is employed. I understand a disorder to be contagious, when it is *in any way communicable* from one person to another." Now we must, I believe, admit that cholera is communicable in the following way :—"Some ferments associate themselves indistinguishably with one or more of the common excretions, and have not their full force till some time after they have left the diseased body, as from the bowel in cholera." Now Dr. Macnamara tells us of nineteen persons drinking from a vessel of water, which was accidentally, but positively, known to have been contaminated with the excreta from the bowel of a cholera patient, in a locality where cholera was neither endemic nor epidemic; the water had neither taste nor smell to attract attention, had been exposed to the sun for a whole day, and was used for drinking purposes early the following morning; five of the nineteen suffered from a well-marked attack of cholera before the third day was past, and fourteen escaped with impunity. Thus the disease was communicated from one diseased person to five sound persons. If then a disorder, which is communicable in *any way* from man to man, is to be considered contagious, cholera, being communicable in the above way, is contagious. There can be no doubt of that. Then, as Simon says,—The cholera ferment can, under some circumstances, bring into similar contagious

fermentation the excrement with which they are mingled in privies, drains and cess-pools, and thus convert the effluvia and leakage from such sources into means of extensive secondary infection of air and water. Now this actually occurred, without a doubt, in the civil cantonments of Shillong in the Cossya Hills, about sixty miles due south of Gowhatty, on the Brahmapootra, and forty miles north of the plains of Sylhet and Cachar, about five thousand feet above the sea, in July last. In April, the head-quarter wing of the 44th Native Light Infantry returned from Cachar to Shillong, and the morning after arrival at Cherrapoonjee, 4,000 feet on the southern aspect of the Cossya Range, a sepoy was seized with the disease and died: this was the 3rd of April, and the first case that had occurred in the hills this last year, 1869. The disease was imported into Cachar in January, by a boat-load of coolies from Calcutta, and two sepoys of the 44th died of the disease at Cachar, shortly after the arrival of the boat, under charge of the Civil Surgeon, Dr. Monteith. The 44th left Cachar then for the frontier and returned in March, cases of cholera being still frequent in the district. On the 3rd and 6th of April, I had two more cases in the regiment at Cherra, and one on the 7th; and on the 10th, we marched northwards for Shillong, arriving on the 12th; on the 13th, one of the men of our party was taken ill and died at Shillong: this was the first case at Shillong. The next was on the 15th, and then we had none until the Eurasian Battery arrived in May; and a lascar was attacked on the 12th, two or three days after arrival from the plains on the south. Isolated cases continued to occur, either in the regiment, or the battery, or in villages round at intervals, all being, I believe, fresh importations from the south by coolies coming up with provisions; and I ventured to predict that if strict attention was paid to the burial of all human excreta, some nine or ten inches beneath the soil, and the pure water of the hill streams kept perfectly free from contamination, that the disease could not and would

not spread epidemically in cantonments. A remarkable and very virulent epidemic outbreak occurred, however, much to my surprise, about the 12th of July in the jail and police lines, on the *north* side of the station. The jail and police lines were clean, but between them in a deep little valley, not two hundred yards across, a boggy bottom had served for nearly three years as a convenient hiding place for filth; three or four days before the outbreak, this cess-pool, for it had become nothing more or less, was dug up by the prisoners to form a tank and drain it, the effluvia was very offensive and filled the whole of the little valley. The "thing" which causes cholera here found a fitting nidus for increase and multiplication, affording "means of extensive secondary infection of air and water." This was an isolated instance of extension, and epidemic, brought about by means of human communications in the first place, beyond the immediate influence of the great epidemic of 1869, into an isolated valley, 5,000 feet above the sea, and across hills quite 6,000 feet above sea-level. If the unknown cess-pool had been left alone, or still better covered with fresh dry soil, I firmly believe no epidemic outbreak would have occurred in the cantonments of Shillong in 1869. All the cholera then that occurred in the Cossya Hills in 1869 was traceable directly in the sporadic cases to human communications with the plains on the south, and the epidemic to "secondary infection of air;" in consequence of the effluvia arising from the bog in the little valley when turned up. If *this* is what Dr. Murray means by contagion, I believe we all agree, but if he means the contagion of actual contact, or from the *fresh* effluvia from bedding or person of a cholera patient, as in small-pox or scarlatina, I believe he disagrees with the whole medical profession in India.

DR. D. B. SMITH said that he did not come to the meeting prepared to speak, indeed, his intention was *not* to speak; but as the subject was a very important one, and as all had been invited to express their opinions regarding Dr. Murray's Lecture,

he would—with all due respect for the author of the paper—presume to indicate what appeared to him to be its weak points.

He thought Dr. Murray's views regarding the so-called "cholera germ" were highly unsatisfactory and ideal. We were asked to attach the utmost pathological importance to an entity, the very existence of which was a matter of inference; a thing which had no name, and which was said to be "very much analogous" to other things—"low vitalities"—which were also unnamed. We were told that the cholera germ "is probably invisible;" and yet in another part of the paper, these words occur: "that it is visible is the conviction of many." Again, notwithstanding its being "probably invisible," the *germ* has been distinctly seen to struggle and die under the action of the cholera pill.

Regarding the entrance of the infusorium into the system,—at one part of the lecture, Dr. Murray tells us, "the *most frequent channel* is by the *intestines* (p. 10)—whilst at p. 13 we learn that "it *generally* enters the system through the lungs."

The germ is said to subsist upon epithelium; but Dr. Smith thought that as there was no actual proof afforded of the bare existence of the animalcule, the question of what it subsisted on must be regarded as a fruitless speculation.

Dr. Murray had confessed that he had found an animalcule "exactly similar" to the specific cholera germ "*in every tank and drain of Calcutta.*" This observation by itself seemed to overturn Dr. Murray's theories.

Dr. Macnamara's microscopical observations, with the highest powers, tended to disprove the fact of there being a *specific* germ of cholera.

Dr. Murray, in his recent report on the Treatment of Cholera, had written:—"It is reproduced, therefore it is vital; but whether vegetable or animal is uncertain."

Dr. Smith thought that such a formula could not be accepted by men of science. Electricity and magnetism are reproducible, but they are not therefore vital. Such a statement is fundamentally incorrect.

Dr. Murray's views regarding the action of the cholera poison on the nerves and blood appeared to Dr. Smith to be purely hypothetical, and therefore liable to support a false pathology.

The ideality of the cholera poison, as believed in by some, Dr. Smith thinks is pushed to inconsequent conclusions. The Constantinople Conference had expressed the opinion that ships having no cholera on board might yet convey and disseminate the disease. Logical proof is wanting.

Dr. Smith thought it very important that it should, if possible, be determined, whether Dr. Macnamara was correct in saying that no cholera had ever prevailed in any part of Europe which had not originally its beginning in India.

With regard to the non-importation of cholera at the Andamans, alluded to by Dr. Macnamara, Dr. Smith said that he happened to be in charge of the six or seven hundred convicts which were landed at the Settlement from the Steam-ship "Golden Fleece." Cholera had prevailed amongst the prisoners on the voyage, and several deaths had occurred; yet, although the men, had been considerably crowded in lower decks—the sick and healthy in close proximity to each other—the disease was not imported amongst the convicts at Port Blair. Precautions had certainly been taken, and the men from the ship had, on landing, been subjected to careful cleansing and isolation; yet the fact remained that men who had been closely crowded with infected persons did not spread the disease. Dr. Smith also made allusion to the following facts:—

(a.) No cholera had occurred at the Great Hurdwar Fair for ten consecutive years.

(b.) Dr. Russell, at Gya,—(one of the great centres of Indian pilgrimage)—had observed and reported that *cholera prevails most when pilgrims least frequent the place.*

(c.) Dr. Davis, at Balasore, had remarked and reported that independent outbreaks of the disease sometimes occur in Orissa, just when the pilgrims are *not* found, whilst it is absent where they are congregated.

(d.) Dr. Jameson, in 1820, had reported that of 250 or 300 Doctors who were on duty in the midst of cholera epidemics—only 3 were attacked—and but one death had occurred.

Dr. Smith thought that these illustrations—the number of which might easily be increased—went to prove that the development and transit of cholera are not necessarily dependent on man, and therefore that quarantine cannot reasonably be expected to control the spread of the disease.

Dr. Smith said he had seen much of cholera at different times, that he had been in the heart of numerous severe visitations, but that he had never been able to observe a single fact tending to prove that the disease was a truly contagious one. He did not deny the *possible communicability* of the disease through the media of fomites, water, or other media; but this by no means proved the possibility of true contagion. He thought that general considerations as to level, site, temperature, season, crowding, and the like should be most minutely studied. But Dr. Murray's speculation seemed to him to lead away from a sound pathology; to be beyond the range of all proof, and therefore unsatisfactory.

DR. CHUCKERBUTTY said that he had not intended to speak on the present occasion, from the great respect he felt for the author of the paper, as the views advocated in it were in many respects diametrically opposed to his own. But from the turn the discussion had taken he found that he was bound in duty to express his opinion. He would do so, however, with all due respect to Dr. Murray. The two questions raised were the contagion and origin of cholera, and they were both most important to the practical physician. In asserting that cholera was a contagious disease the author had evidently in view certain practical measures in the treatment of cholera patients in reference to their admissibility into General Hospitals, their separation from relations and friends, and the necessity of quarantine.

They had, therefore, to consider, not so much the abstract question of contagion, as whether cholera was such a contagious

disease as to make it dangerous to admit persons affected with it into General Hospitals, to permit their friends and relatives to attend on them, and to allow them to enter places which had not been already attacked by the disease.

There were certain diseases which were decidedly contagious, such as small-pox and typhus fever, and it was allowed on all hands that it was dangerous to admit persons suffering from them among other patients. There were other diseases, again, the contagion of which was very slight or doubtful, such as catarrh and influenza, and these were unhesitatingly admitted into General Hospitals. Now it had never been proved that cholera was more contagious (if at all) than either of these latter affections. He had had a very large experience of this disease during his twenty years' connection with the Medical College Hospital, and he could not recall to mind a single instance in which it could be satisfactorily proved that the disease had been contracted by contagion from cholera patients. He admitted that cholera attacks had occasionally occurred among the patients in the hospital suffering from other diseases, but he denied that their percentage was greater than among an equal number of individuals out of the hospital. He would make the same remark with regard to the students of the college, who were a portion of the general community, and who did not suffer in a greater ratio than any other section of it. He had never known a case of death from cholera among the Medical Officers of the hospitals with which he had been connected, and the other attendants upon the sick enjoyed an equal immunity.

He would, therefore, enter a strong protest against the theory of contagion, especially as he believed that the author considered it to be of a most dangerous character, and that he was eager to take advantage of any opportunity to separate the cholera from the general sick, and to establish special cholera hospitals not only in Calcutta, but throughout the country. An instance of the length to which his enthusiasm would carry him had occurred not very long ago in the College Hospital. An opportunity had occurred for establishing a cholera hospital,

and Dr. Murray was most urgent that a house should be immediately hired, and all the cholera sick sent to it. The only building available for the purpose was a small demi-upper-roomed house, badly ventilated, surrounded by all kinds of abominations, and situated in the filthiest locality in the neighbourhood; and rather than lose the opportunity, into this building they were told to place all their cholera patients, European and Native, male and female, till they were convalescent, when they were to be brought back to the College Hospital. When this proposal was submitted to a committee of the hospital officers, they unanimously rejected it, as they had no fear of the contagion of cholera, and as they were not prepared to sacrifice their cholera patients by consigning them to almost certain death. Speaking for himself, he would rather give his cholera patients the smallest nook in the college hospital than send them to such a place. When he had advocated a special hospital for cholera cases, it was only as a matter of convenience, as in his experience the proportion of cholera deaths to deaths from other causes was one in every three, or one to two. Next, as to the danger of cholera spreading by contagion to the friends and relatives of the sick, he did not believe in any such thing. He had never known a case in which they were affected, except under certain circumstances. He would cite a case to illustrate this point. A family of four came from Cuttaek to Calcutta about 18 months ago. They took up their residence in a damp lower-roomed house in a most unhealthy locality, and got their drinking water from a bad tank. The very first day the two children were attacked and carried off. Next the mother and father were attacked, but immediately removed to a better house in a more healthy locality. The mother died, the father recovered, and not another person in the second house, although crowded with inmates, suffered. Here it might be said that it was the water which caused the attacks, and very probably it did, but it did it in no other sense than the "unripe fruit, &c.," of the author. Attacks of cholera were caused not only by bad air, bad water, and unripe fruit, but also by other articles of food either from

bad quality or from peculiar idiosyncrasies of individuals. The water did not in this case communicate a contagious disease, for only those suffered who had actually drunk it and nobody else. He would repeat then that there was no danger of the disease spreading by contagion to the friends and relatives of the sick, and he would warn the gentlemen assembled that were a contrary opinion to go forth from them that day, it might cause the greatest distress and alarm to the public without any reasonable excuse whatever. He would now speak of the portability of cholera. This question necessarily pre-supposed the existence of a poison or material borne in the bodies or clothes of persons. The evidence upon this point was of the following character:— A thousand persons leave Calcutta, while there is cholera in it, apparently in good health. They disperse in different directions. But upon the arrival of one of the number at a particular village he is seized with cholera, and a great many other persons in it get it about the same time. The conclusion is immediately jumped at, that the new-comer had brought the disease to the village, as it had no cholera before he came to it, and as he came from an infected city. And all this, although the nine hundred and ninety-nine who left the city under precisely similar circumstances, did not carry the disease to the places to which they went. Then, again, a man, apparently in good health, arrives at a village where cholera is raging. He gets the disease and dies of it, and the inference is drawn that he got it because he was a new-comer. This latter was a logical deduction, for there being cholera in the atmosphere of the village, and the constitution of the new-comer being disturbed by the change of climate, water, &c., implied by a change of place, he was naturally predisposed to suffer from the disease more than its permanent residents. The former conclusion was erroneous, for cholera may have existed in the atmosphere of the village before the man from the infected city came, and he may have been the first to suffer, not because he brought the disease with him, but because he was probably the most predisposed from the accident of change of place. Dr. Macnamara had stated

that although most parts of the world had suffered from cholera, originally derived from India, Australia and the Andamans were still exempted. That was a most important fact. The Andamans, upon which Dr. Maenamara laid so much stress, although nearer than Australia, had yet not the same value as the latter. The commercial intercourse between the Australian and Indian Ports had been of far greater duration and extent. Hundreds and thousands of ships had plied between those ports, and yet the disease had not been carried to Australia. Here were instances of ships loaded with merchandise and men passing from the infected ports of India to the uninfected ports of Australia, and yet the latter had no cholera. How was this? Because the peculiar atmospheric influence necessary to the development of cholera was absent, or had not yet shown itself, in Australia. Had it been there the disease would have occurred, and the ships would have got the blame of having carried it from India. This naturally brought them to the question of quarantine, which rested entirely on this notion of portability. If Dr. Murray's ideas were correct, that the cholera poison was a germ which reproduced itself within the human body, and could act by contagion, then the question as to what should be done with a cholera case on boardship became one of the most perplexing character. The longer he was permitted to remain on boardship, the greater would be the danger to the passengers and crew, and a conscientious Captain would have to consider whether he should not throw the man overboard, to prevent his body becoming the nursery of cholera germs, especially if he could not land him on account of quarantine. The fact of the matter was, there was a great deal said upon this subject upon very little knowledge. They knew that the true contagious diseases had a period of incubation of a definite kind in each case. What was the length of this incubative stage of cholera? Was it 24 hours, 48 hours, one week, one month, or six months? Nobody seemed to have any information upon that subject, and yet instead of confessing this

ignorance, quarantine was recommended for ships and passengers at the end of a voyage, to do that in a few days which the voyage itself had not accomplished in a month. Could absurdity go any further?

The last matter he would allude to was the nature of the so-called cholera poison. On this point, as has been very ably pointed out by his friend Dr. D. B. Smith, the author of the paper seemed to have made up his mind that it was an animal germ; for he spoke of its history out of the body, the channels by which it entered the body, its history within the body, the channels by which it escaped from the body, its behaviour under re-agents, and the explanations which that afforded of his own plans of treatment. From all this they might infer that he had seen and handled this germ. But a little further on he stated that the germ "was invisible," and that proved that he had not really seen it. In truth nobody had ever seen it, and the whole hypothesis about it was based on the supposition that it was something like the virus of small-pox, and that therefore it necessarily possessed analogous stages. Dr. Chuckerbutty denied that there was any poison, vegetable, animal, or mineral, required to produce the phenomena of cholera. He conceived that cholera was generated, in the same manner as catarrh and rheumatism, by an atmospheric influence. It was not maintained that catarrh and rheumatism were produced by any poison introduced into the body from without. No more was cholera produced by any poison introduced into the body from without. Catarrh and rheumatism arose from exposure to cold and damp, and it might be said that the poison here was generated within the body. But that was only begging the question, for nobody had ever proved the presence of any poison in the blood in these cases. All that they knew was, that a morbid process was set up in the body by the chill, and that the force of it fell upon the lungs or the joints, according as the case was one of catarrh or rheumatism. Similarly in cholera a morbid process was set up in the body by an atmospheric

influence, and the force of it fell upon the alimentary canal. The morbid process selected for its operation that part of the body which was most susceptible to it. This atmospheric influence was a matter of inference of the same kind as the law of gravitation. It was a common observation for more than a thousand years that bodies fell to the earth, but the law of gravitation was not known till it was discovered by a philosopher. They did not know at present what the nature of the influence was which produced cholera. Probably it was generated by variations of temperature, moisture, electricity, and other atmospheric phenomena. It was not identical with that which produced catarrh and rheumatism, but it was analogous to it, although of a peculiar nature. This influence might be limited to a confined area, or it might overspread the whole face of the earth. It might come into action quite suddenly, and last for one week or six months, and then as suddenly disappear without their being able to discern its cause. He would mention one case to illustrate this point. Lately for about three months the cholera ward of the Medical College Hospital was entirely empty. All at once a change took place in the weather. The sky became cloudy and the temperature warm, leading the incautious to change their winter garb for a lighter one. Then there was rain, and chilly weather. Now, on the very first day of the rain, cholera made its appearance, and cases of the most virulent type poured into the hospital in great numbers. Here the disease was evidently produced by the action of chill upon the skin, favoured by the imprudent change of clothing. With these few remarks he would conclude for the present, but before he did so he would repeat that he did not believe in the contagion of cholera; that he did not consider that there was any danger in admitting cholera cases into a General Hospital; that he did not admit that cholera ever spread from the sick to their attendants; that he did not believe in the portability of cholera; that he did not believe that there was any such thing as a cholera germ; and that he thought that the disease arose

from an atmospheric influence, the precise nature of which was not yet definitely determined.

DR. CHARLES said :—Mr. President,—Rising as I do at this late hour of the evening, for many reasons I think it better that I should confine the remarks I have to make to the one question of communicability, on which so many speakers have already expressed their views. There are very many other points in Dr. Murray's paper which would have deserved discussion had time permitted, but it seems to me of considerable importance that the views held by each member of the society on the question of the communicability of cholera should now be freely expressed, and to allow of this I shall make my remarks as brief as possible.

There is one phase of the arguments which, I think, in to-night's discussion has hardly been fairly dealt with. In assuming a *materies morbi*, Dr. Murray does nothing more than we are all daily in the habit of doing. It does not seem to me just to meet him with a protest that, unless it can be shown under a microscope, unless it has form and shape, unless it has been described under a name, and so forth, that such a hypothetical existence is simply a creation of the fancy. How do we deal in the case of other diseases in which the agency producing them is not recognisable by any method of research at our command. We assume that there is a cause for the effect which we see, and we are not unwilling to learn as much as we can gather regarding the nature of the cause and its behaviour under varying conditions. We all believe in a malaria which we can in no way distinguish by any means which we have yet brought to bear on it. A contagion of typhus and small-pox, of scarlatina and measles, and hooping cough, finds a place in every medical man's belief, although in no case can we say we have seen the cause which leads to such results. It is true that an American observer thought at one time, he had seen a fungus which he believed to have the power of producing measles, but as yet his observations do not seem to have met with

confirmation. Even when we can recognise a product of the body as containing a power of producing a specific disease, we are foiled when we attempt to go further into the question, and can in no instance say we have seen the essential element to which it owes this power. When we place under the microscope, the saliva of a dog suffering from *rabies canina*, can we affirm, that we see what it is that makes that specimen of saliva differ in any way from other specimens by which the disease could not be communicated? If we put vaccine or the virus of small-pox under a microscope, can we discover why such serum or liquor sanguinis, or pus, or whatever material we may be examining possesses the patent power of producing vaccinia or variola, which we certainly know it to be capable of doing?

Members of our profession are in the habit most legitimately of assuming a cause for disease, and I think not one of us will differ from the writer of this paper in the mode of dealing he has adopted with regard to this question. Every speaker, this evening, has gone on the assumption that some such cholera-producing agency exists. It has been, it is true, spoken of as a "peculiar influence," "a poison," "an allied vitality," "a germ," and so forth; but for the purposes of the present discussion the difference may be simply regarded as one of words, as all are agreed that there is some specific agency which is capable of producing cholera, be it matter itself or simply a property of matter. So far I say we all go with Dr. Murray; for myself I am free to confess that for the present I feel myself justified in believing that the *materies morbi* is to be met with in the stools of cholera patients, and, in some instances, they seem to have been the vehicle through which the disease was conveyed. I think I myself have been a witness to cholera so induced. Early in 1857, I was with the 1st Fusiliers when they made their celebrated forced march from Dugshaie to Umballa. At neither station was there any cholera, but we had to pass through a tract of country in

which the epidemic was raging. Our belief was that the men contracted the disease in this infected locality probably by the foul water which they had to drink freely of, and that the outbreak which followed among them was probably greatly aggravated by a rapid multiplication of the cholera poison in a dirty latrine which was made use of by the regiment at Umballa. The barracks had been unoccupied for some time, and the civil population had been using the latrines without their being ever cleaned out, and when the regiment arrived with choleraic diarrhœa, the cholera poison in their evacuations seems to have found a very suitable medium in which to multiply, by being deposited in the accumulations of unremoved fœces in this very neglected latrine. Possibly the poison may have met with the pabulum most suitable for its wants in one or other of the various stages of decomposition in which it found the uncleanness in this latrine. At all events, we regarded the latrine as the source of much mischief, and we believed it was attended with danger entering it.

Another probable instance of cholera, conveyed by the latrine, I met with later on in the same year, while in camp during the siege of Delhi. We noticed the comparative immunity of severely wounded men from cholera. I can only recollect a single instance of a badly wounded man taking the disease. While those not so severely wounded seem to have suffered just as much as their other comrades did, who were in hospital. I think we are justified in believing that in this case the severely wounded man, being confined to his bed, was saved from the disease which those who were able to frequent the hospital latrines now and then contracted from a source of the disease to be met with in this locality. As already stated, I believe water may sometimes produce cholera in those who drink it, and I believe that I have seen instances traceable to both latrines and drinking water, both causing cholera ; but there is another form of communication, in which I believe, on the evidence of others, for

on no occasion has any such instance fallen under my own observation. It seems as if under certain conditions a hospital which would otherwise have been free of taint has communicated the disease to those who frequented it, in consequence of having been contaminated by cholera patients. I repeat I have never met with any such example, and when it does occur, I think we can ascribe it to defective care, over-crowding, defective ventilation, bad conservancy, and such like.

On being asked in Russia at the time of an impending epidemic, what was our experience on this point in India, parodying a French epigrammatic sentence, I replied, "God sends cholera, man makes contagion." And here, I think, is the point at which I begin to differ from Dr. Murray. Up to this point our views are similar, if they do not exactly coincide, they at least run very parallel with one another. Now, however, they diverge completely. It is most important that it should be fully understood that those of us who have gone so far with him now no further subscribe to his views; that our paths here gradually begin to diverge, till at last they proceed in exactly different directions.

I have stated that I believe cholera may be communicated in various ways, indirectly from those who have been affected to those who might have otherwise escaped the disease, but in all cases with which I am acquainted cholera so produced seems to have no power of reproduction, (to use Dr. Bryden's phraseology), it dies after having exercised its evil influence, and it never goes on acting through individual to individual till large communities have suffered. Take the case of a cholera outbreak, produced by a shirt which has been worn by a cholera patient; the mother, who has received it from a distance, takes cholera, another couple of individuals in the same house also suffer; and some of the neighbours may also participate in the outbreak,—but here it ends. I do not think a whole "parish" in England can have an epidemic of cholera brought into it in this way, or that a "district" in India can be

ravaged by a cholera epidemic so imported. While freely admitting the possibility of a small community or a regiment of soldiers receiving cholera from a previously affected individual, I confess that I do not know of any carefully observed facts which would lead me to the conclusion that these infected communities may be the source of such widely devastating epidemics that we are asked to believe may be lit up in this way. A very large body of evidence seems to show that this is not the usual way in which the disease spreads, and that it is disseminated over vast areas by the wind ; while this mode of diffusion tends to such a destruction of human life as to reduce to comparative insignificance the number of deaths which can be traced to any intervention of human agency. I look upon the evidence on which such belief is founded as quite conclusive, and to be as much worthy of being relied on as any on which any medical doctrine is based. Under such circumstances, it can well be asked how much help in excluding the disease can we expect from quarantine. Even should we be able to succeed in carrying out a system of quarantine, to use the words of Simon, with the certainty of a chemical experiment, we should fail to exclude the disease in the epidemic form, that form in which it alone has been found to be the terrible destructive agent, we are called upon to provide against. The disease would overstep our barriers as if they had no existence. When we condescend to enquire what sort of a quarantine those can enforce who are the strongest advocates of such a repressive precaution, we meet with a result which entirely negatives any hope that we might have been led to entertain from a previous one-sided reasoning on the question. When we hear that cholera has been kept out of a military cantonment by the establishment of quarantine, we on enquiry will be told that it was found that the same means which they adopted to exclude cholera they found were also efficient, as Dr. Barnard has to-night said, to separate them from their beef and their beer, and other requisites, without which they found

it impossible to subaist. Hence originates a system of passes. In one notable instance it is said that cholera spared a body of troops in consequence of a quarantine, during which fourteen hundred passes had been issued. I confess on such a point as this there is reason to be sceptical ; my mind refuses to conceive of an efficient quarantine carried out on a system under which fourteen hundred passes have been granted. Again, were cholera the freely communicable disease we are asked in this paper to believe that it is, surely in our large General Hospitals we would find some evidence of such communicability in observing the spread of the disease from the numbers of infected persons who are allowed admission to these hospitals in every stage that the disease can present. We should expect to find not only that considerable numbers of the patients should receive cholera from those affected and brought to hospital with the disease, but that the hospital attendants should also in some degree suffer. A long experience extending back over many decades proves that such extension of the disease is not met with. All the Medical Officers who have successively held posts in the General Hospital of Calcutta have had their attention directed to the point, and none of them believe that they have ever seen cholera spread in this way. At one time when for years I had been connected with this institution, I investigated the point carefully, and I could not get one single instance of cholera so communicated. Once I thought I had secured one instance, and felt the greatest anxiety to prove conclusively that it has no mere coincidence, but a *bonâ fide* case of cholera resulting in consequence of being brought into relation with the sick. One of the hospital barbers took cholera, and on investigation I found that not only did the man not reside within the hospital enclosure, but that he was the only one of that hospital servants who was never known to have entered the cholera ward. In pursuing the enquiry I found that servants of very long standing—if my memory serves me, in one instance of thirty years' service, and in

another of forty-two years of continuous connection with hospital—could not tell me of any one of the servants having died of cholera. These men had seen generation after generation of medical men and hospital attendants succeed one another, and in the course of these long years had never had reason to suspect that any of their fellow servants had contracted cholera in the hospital. Ask the servants who administer the medicines to cholera patients; those who feed them when they begin to take food; those that rub them when they suffer from cramps or apply the various remedies, which they are directed to make use of, and none of them knows of any of their number having been carried off by cholera. Again, ask those who supply water to the cholera ward; those who freely handle the cholera evacuations; those who wash the clothes soiled with discharges from the sick, and you always get the same negative evidence. None of them die of cholera. Extending back as it does now for a long series of years, such negative evidence has a distinct value of its own; it can not be ignored, and it tells its own story in a most positive way, that cholera patients may be admitted day after day into a General Hospital, inhabited by large numbers of both sick and healthy individuals, and that the disease will not spread to others already in the hospital. I look on this as established on much more positive evidence than that on which we believe that the disease may pass indirectly over from one individual to another, and in this I differ from the views held by Dr. Murray. When we are asked to believe otherwise, and have held up before us a contagion which differs so essentially from all other contagions with which we are acquainted; are invited to accept that a contagious disease does not show its usual manifestations when studied in a hospital in which one would expect to be able to demonstrate in any single month, that it possesses such properties, then I say we must hesitate, and before accepting views so much at variance with all known facts, we must ask for other evidences to be detailed

to us, very different from any which have yet been brought before us by the author of this paper.

DR. FAWCUS said that he concurred in most of the objections which had been made to Dr. Murray's theory. He mentioned that quarantine had been tried in the Bengal jails, and found inefficacious. Cholera prevailed in both the Alipore and Deegah jails, although there were no cases in the quarantine wards attached to those jails. He saw no reason to suppose that such a creature, as Dr. Murray's "invisible ally," exists. He nevertheless does not disbelieve in the existence of a specific cause, in respect of which cholera is capable of propagation. He doubts whether paralysis of the sympathetic arrests secretion, as asserted by Dr. Murray. Neither does he believe that an invisible or any other animalcule eats up the epithelium. He regards the action of the gastric juice and the bile, as described by Mr. Murray, as purely imaginary, and cannot understand how he has arrived at the conclusion that the invisible animalcule passes through various stages, or in fact, how he knows anything about it. He thinks Dr. Murray's position would have been stronger if he had maintained that the animalcules he saw were the cause of cholera. He doubts whether clots in the heart ever cause death, as stated by the author of the paper.

DR. WOODFORD said :—The lateness of the hour which is verging on midnight precludes the possibility of my doing more than express my personal experience regarding this dire disease, cholera. Although that experience extends over a period of 25 years, I have never had occasion to consider it contagious. For many years I had charge of a *low-roofed* building for hospital use, which was badly ventilated and often greatly over-crowded ; as I was not at liberty to refuse admission to any applicant at the so-called Pauper "Hospital," cholera cases were therefore received, and had of necessity to herd with the rest, but the disease did not spread, or if it did occur *in* hospital at the same time, not more than one, two, or three out of 200 became affected. The inference therefore is that they would probably

have been attacked had they *not* been in hospital, and I can call to mind only two instances over a period of 20 years in which the hospital attendants have fallen victims to the disease. During the same period only two instances of endemic cholera occurred: one at the Government Gowkannah Pauper Hospital, the other at the new Native Police Hospital, (both under my superintendence,) but it died out almost as soon as the anxiety arose; in the first instance some 14 cases occurred in 24 hours, yet upwards of 200 escaped. Although opinion appears to be divided as to the contagious nature of the disease, of the specific "cholera germ," Dr. Chuckerbuddy says that "there is a peculiar something which induces cholera;" many are of that opinion, but what is that "*peculiar something*," is it a germ, or a noxious gas? There is some reason for the latter conclusion, as most, if not all, of our profession in Calcutta have remarked that after continued dry weather, a heavy downpour of rain washing out the drains is immediately followed by cases of cholera in the vicinity; but when the rains become continuous, and the foul drains become in a measure cleansed, the same result does not occur. The fleet of shipping in the Hooghly forms a striking example, as those moored opposite the large drains which empty their fetid refuse into the river are precisely those vessels which suffer the most severely from cholera. In the desert, where there are no noxious drains or emanations, cholera is unknown. This theory, however, would also appear to be negatived by the fact that the *Dangahs*, who are chiefly employed in cleansing the drains, do not suffer more, if as much as other people from cholera; also to the fact that in some instances it can be distinctly traced to fear, and fear alone; I once saw a remarkable instance in illustration. In conclusion, I think it can safely be said that all we know at present of this terrible disease is this—that it is shrouded in obscurity, and depends upon that "*peculiar something*" which has yet to be discovered.

DR. MURRAY then inquired whether any member was prepared to furnish him with a definition of the word contagion.

After some conversation on this point, in which Drs. SMITH and MACNAMARA took part, Dr. EWART rose and said, that any definition which could be given to the word contagion was open to objection ; he had been in the habit of regarding a disease as contagious, which was capable of spreading directly from the affected to the apparently healthy, and which was, therefore, in a measure, dangerous to the physician, friends, and attendants. Typhus and small-pox were, in this sense, unmistakably contagious diseases. That is to say, that something, or some influence, was produced in the bodies of the typhus and small-pox patients, which emanating from them, was capable of producing typhus and small-pox to those attending upon such cases. The experience of centuries has confirmed this view of contagion regarding these two diseases.

Now this much cannot, I think, be said of cholera. Throughout the whole of Lower Bengal, and notably in such cholera homes as Berhampore, Dacca and Calcutta, physicians and attendants have always enjoyed as much immunity from the disease as the general or surrounding population. Had cholera been contagious in the same sense as typhus or small-pox, the deaths among civil surgeons and physicians, since 1817, would have been numbered by the score ; whilst among the menial attendants, they would have been counted by the hundred. No man can possibly be more exposed than the physicians to the Medical College, Chandney, General, and the Howrah Hospitals, who, during eight or nine months of each year, may be said to be brought into daily contact with cholera cases. Yet what has been the experience of these gentlemen as regards the doctrine of contagion ? It has been this, *viz.*, that the discharges and emanations from the cholera-stricken patient are powerless to produce the disease, until they have undergone some metamorphoses or change outside the body, either on articles of furniture, bedding and clothes, or in the soil or water. When these changes have been effected, then the potential cause of cholera results, and its introduction into

the stomach by the food or water leads to the production of the phenomena characterizing an attack of the disease. Cholera, therefore, does not spread by contagion, like typhus or small-pox. In its endemic form, it spreads rather like the typhoid fever of Sir William Jenner, through the dejecta, but with this difference, that the potential cause of typhoid fever probably exists in the alvine evacuations at the time they are passed from the body, whilst in cholera some ulterior changes in the dejecta, external to the body, are essential to the perfecting of the causative agency of the malady.

Hence it happens that when the hygienic and sanitary condition of a cholera hospital is properly administered, the disease does not spread. By boiling all the clothes and bedding of a cholera patient, and by disinfecting the wards, and either destroying or disinfecting the vomited matters and the alvine evacuations, the physicians, students, and attendants of the Medical College Hospital, though constantly in contact with cases of cholera, have enjoyed extraordinary immunity from the disease. If, however, these precautions were neglected, doubtless the disease would spread, because time would be allowed for the completion of the ulterior changes in the cholera stools and vomit, which result in the production of a something which, on gaining access to the alimentary canal of a predisposed individual, causes cholera.

Whenever cholera has been supposed to spread by direct contagion from person to person, or by fomites like typhus and small-pox, its diffusion has been capable of explanation independently of contagion. Take, for instance, an outbreak at sea, some weeks after a ship's departure from a port, following the opening of a box containing the non-disinfected clothes of a cholera patient. Here experience would indicate that time and opportunity had been allowed for the development of those changes in the cholera discharges in the clothes, essential to the perfection of its cause. Experience teaches that immediately after the soiling of these clothes, the cholera discharges on,

or in them, did not possess the power of producing the disease in another person however predisposed. How different is this from the way in which typhus or small-pox can and does spread! The causes of these diseases are reproduced in the system, and are potential enough to produce them in other persons, during their course. Not so with cholera. There is nothing thrown off from the cholera patient, during the course of the attack, which can directly produce the disease in another individual. It is this peculiarity which, aided by good conservancy and the use of disinfectants, renders cholera absolutely innocuous to the physicians, and other attendants and pupils of the Medical College Hospital,—as harmless, in truth, as typhoid fever, or as an ordinary intermittent.

When physicians in India speak of the communicability of cholera, they do not mean that this term is synonymous with contagion. Far from it. What they mean is, that there is a something contained in the discharges, which, under favouring circumstances, can, after a lapse of time—not altogether defined—undergo those metamorphoses necessary to the constitution, of the efficient cause of cholera. For example, on clothes, furniture, bedding, uncleaned and non-disinfected, the discharges can and do undergo these metamorphic changes, and when completed, the cause may be introduced “or communicated” from these into the intestinal canal, and produce the symptoms of cholera; or the discharges may be deposited on the soil, and therein become subject to the changes alluded to, and the causative agency may thus percolate, through the soil, into the drinking water of the population, and whilst in a state of activity diffuse cholera amongst the people using such water. This is what the medical profession in India, and I believe in Europe too, mean by “*the transmissibility or communicability*” of cholera. They do not intend it to be understood that the disease is directly conveyed from person to person during an attack of the disease.

Moreover, the doctrine of contagion does not explain the extra-

ordinary rapidity with which cholera spreads in its epidemic form. Here, there must be something super-added, some peculiar atmospheric condition to facilitate its propagation. But though this is probably the case, we know enough of the disease and the mode in which it is apparently disseminated, to justify us in believing that it is greatly preventible by an improved system of hygiene and sanitation. The spread of the disease will not be materially affected by quarantine ; but it may be greatly modified or diminished, I think, as has over and over again been illustrated in the jails of Lower Bengal, by practical attention to the laws of sanitary science.

The ordinary monthly meeting of the Bengal Branch of the British Medical Association was held at the Rooms of the Asiatic Society on Tuesday, the 8th February, 1870, at 8½ P.M.

DR. EWART, President, in the chair.

The proceedings of the adjourned annual meeting having been approved and confirmed, the discussion on cholera was reopened.

DR. MURRAY, in reply, said:—I shall notice the observations made at the last meeting in the order in which the subject under discussion is arranged in my paper.

First.—The specific poison. The names applied to it are numerous, but the meaning is a vital energy, whose vitality is proved by its being capable of reproduction. It is possibly invisible, though by some supposed to be an appreciable infusorium, fungus, or animalcule. On this point, Dr. Smith said:—“Dr. Murray in his recent report had written, ‘it is reproduced, therefore it is vital ; but whether vegetable or animal is uncertain.’ Dr. Smith thought that such a formula could not be accepted by men of science. Electricity and magnetism are reproducible, but they are not, therefore, vital, such a statement is fundamentally incorrect.” He did not believe in the existence of a special cholera poison, as neither he nor any one else had seen it, and that he considered the theory groundless, and that

it would not only be a waste of energy to follow up this idea, but also working in a wrong direction, and likely to prove injurious. I fear I shall be unable to convince Dr. Smith that my views are founded on fact, as I cannot show him the identical poison; in fact, I have allowed that it may be invisible, but there is no more reason to doubt the individuality of the cholera poison than that of plague, typhus, small-pox, venereal, measles, and numerous other zymotic diseases which have never yet been seen, but whose action on the body has been the object of diligent study, since Esculapius first taught men to apply appropriate remedies to specific diseases. In all these diseases our opinion is founded on circumstantial evidence which has been accumulating for many centuries. The disease, we are considering, has only been under observation for the last fifty years. In individual experience connecting links are often wanting, but facts observed by others may supply these deficiencies, and the patient accumulation of the evidence of independent observers is advancing our knowledge. It would be unreasonable to expect more, it is from the history of the disease, its symptoms and its progress that we judge its nature. The opinions of the profession are settling on this point. The non-appreciability of the poisons in other diseases was ably argued by Dr. Charles, and it is my impression that Dr. Smith stands alone in this opinion, as also with the addition of his friend Dr. Chuckerbutty, in that of spontaneous production of specific germs. Dr. Smith also said that I was in error in stating that 99 per cent. of the intelligent part of the profession in India believe in the communicability of this disease, under certain circumstances. He states that he is a non-contagionist. This I do not question, and I suppose he knows what he means. This statistical collection of Indian medical opinion is highly approved in Europe, and it thus attains a force which it did not possess until thus united.

The second point in the paper, *viz.*, the primary action of the poison on the system, was ably discussed by Dr. Macnamera. He fully agreed with me as to the course of the symp-

toms induced by the presence of the poison in the system, especially in the earlier stages of the disease, and also as to the diminished and ultimately suspended secretion from the organs supplied by the sympathetic nerve, yet he considers that this does not depend on what I have called "paralysis." He gives as his reason that when this nerve was divided, in some experiments on animals, there was an apparent increased secretion, particularly of the gastric juice. We know little about the functions of the sympathetic nerve, but so far as I comprehend, it is a general law that secretions are the result of nervous energy, and that deficiency of that power is followed by deficiency of secretion. If the experiments mentioned by Dr. Macnamara (which were quoted to me some time since by Dr. George Johnson as disposing of my theory of paralysis) be correctly reported, it would prove that want of nervous energy increased secretion. The symptoms reported to have been produced by cutting the sympathetic nerve are not found in cholera. I allowed that the name "paralysis" was not strictly appropriate; we are agreed as to the facts of the case, and, what is more important than a name, in the practical indications to be drawn from them. I should be happy to find a word to convey more clearly my idea. I am satisfied that we agree so far as is practical.

Dr. Macnamara asked how it is that by restraining, and yet more by actually stopping, the discharges from the bowels in the early stage we cure the disease? This was long a difficulty in my mind, as I considered purging to be generally an effort of Nature to get rid of irritating matter, and so it is when the irritating cause is undigested food, the removal of which is necessary to health, but *in the present instance* there is, in addition, in the intestines a poison which is only restrained from active life by the presence of the gastric juice or bile which are removed by purging, whether induced by natural causes or by purgatives. If the remedies used to check the purging would at the same time check the secretion of the gastric juice

or bile, they would be likely to aggravate the danger ; but if by their action they detain the bile and gastric juice on the mucous surface, more especially if they promoted these secretions which rendered the poison inert, the explanation is afforded of their beneficial action. We know that in the great majority of cases of malaise, the poison is passed off without active symptoms. In this stage the poison I consider to be in the circulation, in a primary or inert stage of its existence, which Dr. Macnamara does not recognise.

The third important point discussed was the communicability of the disease, and this attracted the most general attention. Here the want of a strictly defined meaning to the word "contagion" caused considerable discussion and apparent divergence of opinion, which gradually diminished on subsequent explanation. Many considering the communication of the specific disease from a sick to a healthy person by the transmission of the contagion or germ to mean *contagion*, whether transmitted directly or by means of an intermediate agent ; others holding that the disease must be communicated directly from hand to hand to constitute contagion, and that this cannot be done in cholera. They allow that the disease may be communicated indirectly, but that the germ has to pass through a certain stage of its existence in the external world, before it can enter, and produce the disease in a healthy person. The period required for this change is not stated, nor the medium in which it has to take place. On this point only, the immediate communicability of the disease, was there any strong opinion expressed at variance with my experience. One day at Agra in 1861, I saw the clergyman who was comforting a dying patient seized with vomiting. On the same day the officer on duty in the next hospital I visited was attacked while I was there ; he died, as well as the commanding officer, who had attended a funeral that morning. I was once attacked within two hours of attending cases in the bazar at Indore in 1848, and in the cholera ward at Agra in 1856, where during the same year, Dr. Farquhar was attacked a few minutes after leaving the hospital.

On the other hand, I have been thousands of times in contact with cholera patients. I have even laid with my child's hand in mine when she was in collapse, because it gave her confidence, without suffering; yet I have come to the conclusion that it may be communicated, and I am anxious for your assistance to ascertain the reasons of exemption, for all must allow the poison is in most dangerous proximity, if it has not entered the system. This latter is the conclusion at which I have arrived, and that being there it is restrained by Nature's agents—the gastric juice and bile—which are sufficient to check it, and avert any dangerous symptom till it is eliminated from the system. This theory will give the physician and attendants full confidence in performing their duties to the sick, while, at the same time, they are aware of the danger that would be incurred by any action of their own calculated to diminish these safeguards of the *vis medicatrix naturæ*, and are prepared to use remedies known to be efficacious in the earlier stages of the disease, should suspicious symptoms arise. I attach great importance to the observations of Dr. Woodford, that the number of hospital attendants attacked in the cholera hospitals under his charge was not great, but that it equalled the number attacked in the small-pox hospital, which is also under his charge. The impossibility of directly transmitting the disease by personal contact, and hence the inference that there is no danger from being in the vicinity of cholera patients, was strongly insisted on by Dr. Chuckerbutty, and several other members, who asserted that with perfect sanitary arrangements and the immediate disinfection and burial of the evacuations, there could be no communication of the disease. Even independent of direct communication, safety here hinges on the perfect destruction of the discharges and perfect sanitation. I fully agree with those who say the more perfect the sanitary arrangements, the less danger of the disease spreading; yet I must add that hitherto the most perfect sanitary arrangements that we have been able to introduce into our military cantonments or jails, have not, in some instances, averted a lamentable mortality. I consider that

knowingly to admit a person suffering from cholera into a healthy town, still more into the common wards of a hospital, or into a jail (where I have seen the imported disease kill hundreds of prisoners) is as culpable as to allow a man with a lighted cheroot in his mouth, to enter a powder magazine. Dr. Fawcett considers that quarantine regulations in jails in Lower Bengal are useless. The mortality in these jails has been very much in excess of that in the Upper Provinces since the introduction there of quarantine rules, even though they are allowed to be imperfect. Yet in the year of the great Hurdwar epidemic, when the Upper Provinces and Punjab were severely attacked, and there was no unusual sickness in Bengal, the loss of life in the Bengal jails from cholera was 7.26 per 1,000, while in the Upper Provinces it was only 0.65 as shown in Dr. Bryden's tables. At the Bengal rates the mortality would have been 291 instead of 27, which actually occurred. In the epidemics of 1856 and 1861, before quarantine was established, the mortality in the Upper Provinces was 24.93, and 37.93 per 1,000, which at the same ratio would have given a mortality of 966 and 1,530 in the epidemic of 1868, showing a relative saving in these two years of 2,442 lives. It is the sacred object of our profession to save life.

I think we have gained a step in this investigation by the experiments with the gastric juice on the allied vitality. It appears to be the nature of the cholera poison to pass through its active stage of existence on the mucous surface of the intestines; here during health the gastric juice and bile are also present, and their action we have seen to be destructive to the germ. Under these circumstances, even the congee stools may be swallowed and rendered inert if not digested. This applies equally to the poison entering the system through the lungs or skin probably in another stage of existence. It may remain comparatively inert until the absence or removal of the restraining agent allows it to develope, when it will run its ordinary rapid course.

I am an earnest searcher after truth in this most difficult and important disease, by a true knowledge of which we may confer

great benefits on mankind. My means and powers of observation are limited, and they would be materially extended by your cordial support and assistance.