

**GILLKREST JAMES, FERGUSSON
WILLIAM**

**LETTERS ON
THE CHOLERA
MORBUS.**

William Fergusson
Letters on the Cholera Morbus.

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Содержание

| | |
|-----------------------------------|----|
| LETTERS ON THE CHOLERA MORBUS; | 5 |
| LETTER I | 5 |
| LETTER II | 11 |
| LETTER III | 19 |
| Конец ознакомительного фрагмента. | 21 |

William Fergusson
Letters on the Cholera Morbus. Containing
ample evidence that this disease, under
whatever name known, cannot be transmitted
from the persons of those labouring under
it to other individuals, by contact—through
the medium of inanimate substances—
or through the medium of the atmosphere;
and that all restrictions, by cordons and
quarantine regulations, are, as far as
regards this disease, not merely useless,
but highly injurious to the community

LETTERS ON THE CHOLERA MORBUS;
SHEWING THAT IT IS
NOT A COMMUNICABLE DISEASE

LETTER I

If we view the progress of this terrific malady, as it tends to disorganise society wherever it shows itself, as it causes the destruction of human life on an extensive scale, or as it cramps commerce, and causes vast expense in the maintenance of quarantine and cordon establishments, no subject can surely be, at this moment, of deeper interest. It is to be regretted, indeed, that, in this country, political questions (of great magnitude certainly), should have prevented the legislature, and society at large, from examining, with due severity, all the data connected with cholera, in order to avert, should we unhappily be afflicted with an epidemic visitation of this disease, that state of confusion, bordering on anarchy, which we find has occurred in some of those countries where it has this year appeared.

Were this letter intended for the eyes of medical men only, it would be unnecessary to say that, during epidemics, the safety of thousands rests upon the solution of these simple questions: – Is the disease communicable to a healthy person, from the body of another person labouring under it, either *directly*, by touching him, or *indirectly*, by touching any substance (as clothes, &c.) which might have been in contact with him, or by inhaling the air about his person, either during his illness or after death? – Or is it, on the other hand, a disease with the appearance and progress of which sick persons, individually or collectively, have no influence, the sole cause of its presence depending on unknown states of the atmosphere, or on terrestrial emanations, or on a principle, *aura*, or whatever else it may be called, elicited under certain circumstances, from both the earth and air? – In the one case we

have what the French, very generally I believe, term *mediate* and *immediate* contagion, while the term *infection* would seem to be reserved by some of the most distinguished of their physicians for the production of diseases by a deteriorated atmosphere: – much confusion would certainly be avoided by this adoption of terms.¹ Now it is evident, that incalculable mischief must arise when a community acts upon erroneous decisions on the above questions; for, if we proceed in our measures on the principle of the disease not being either directly or indirectly transmissible, and that it should, nevertheless, be so in fact, we shall consign many to the grave, by not advising measures of separation between those in health, and the persons, clothes, &c., of the sick. On the other hand, should governments and the heads of families, act on the principle of the disease being transmissible from person to person, while the fact may be, that the disease is produced in each person by his breathing the deteriorated atmosphere of a certain limited surface, the calamity in this case must be very great; for, as has happened on the Continent lately, cordons may be established to prevent flight, *when flight, in certain cases, would seem to be the only means of safety to many*; and families, under a false impression, may be induced to shut themselves up in localities, where "every breeze is bane."

Hence then the importance, to the state and to individuals, of a rigid investigation of these subjects. It is matter of general regret, I believe, among medical men, that hitherto the question of cholera has not always been handled in this country with due impartiality. Even some honest men, from erroneous views as to what they consider "the safe side" of the question, and forgetting that the safe side can only be that on which truth lies (for then the people will know *what* to do in the event of an epidemic), openly favour the side of *communicability*, contrary to their inward conviction; while the good people of the quarantine have been stoutly at work in making out that precautions are as necessary in the cholera as in plague. Meantime our merchants, and indeed the whole nation, are filled with astonishment, on discovering that neighbouring states enforce a quarantine against ships from the British dominions, when those states find that cases of disease are reported to them as occurring among us, resembling more or less those which we have so loudly, and I must add prematurely, declared to be transmissible. It is quite true that, however decidedly the question may be set at rest in this country, our commerce, should we act upon the principle, of the disease not being transmissible, would be subject to vexatious measures, at least for a time, on the part of other states; but let England take the lead in instituting a full inquiry into the whole subject, by a Committee of the House of Commons; and if the question be decided against quarantines and cordons by that body, other countries will quickly follow the example, and explode them as being much worse than useless, as far as their application to cholera may be concerned. It is very remarkable how, in these matters, one country shapes its course by what seems to be the rule in others; and, as far as the point merely affects commerce, without regard to ulterior considerations, it is not very surprising that this should be the case; but it is not till an epidemic shall have actually made its appearance among us, that the consequences of the temporising, or the precipitation, of medical men can appear in all their horrors. Let no man hesitate to retract an opinion already declared, on a question of the highest importance to society, if he should see good reason for doing so, after a patient and unbiassed reconsideration of all the facts. We are bound, in every way, to act with good faith towards the public, and erroneous views, in which that public is concerned, ought to be declared as soon as discovered. To show how erroneous some of the data are from which people are likely to have drawn conclusions, is the main cause of my wish to occupy the attention of the public; and in doing this, it is certainly not my wish to give offence to respectable persons, though I may have occasion to notice their errors or omissions.

Previous to proceeding to the consideration of other points, it may be observed, that all doubt is at an end as to the identity of the Indian, Russian, Prussian, and Austrian epidemic cholera; no greater difference being observed in the grades of the disease in any two of those countries, than is to be found

¹ As medical men in this Country employ the word *infection* and *contagion* in various senses, I shall, generally substitute *transmissible* or *communicable*, to avoid obscurity.

at different times, or in different places, in each of them respectively. At the risk of being considered a very incompetent judge, if nothing worse, I shall not hesitate to say, that if the same assemblage, or grouping of symptoms be admitted as constituting the same disease, it may at any time be established, to the entire satisfaction of an unprejudiced tribunal, that cases of cholera, not unfrequently proving fatal, and corresponding in every particular to the average of cases as they have appeared in the above countries, have been frequently remarked as occurring in other countries including England; and yet no cordon or quarantine regulations, on the presumption of the disease spreading by "contagion." For my own part, without referring to events out of Europe, I have been long quite familiar, and I know several others who are equally so, with cholera, in which a perfect similarity to the symptoms of the Indian or Russian cholera has existed: the collapse – the deadly coldness with a clammy skin – the irritability of the stomach, and prodigious discharge from the bowels of an opaque serous fluid (untinged with bile in the slightest degree) – with a corresponding shrinking of flesh and integuments – the pulseless and livid extremities – the ghastly aspect of countenance and sinking of the eyes – the restlessness so great, that the patient has not been able to remain for a moment in any one position – yet, with all this, nobody dreamt of the disease being communicable; no precautions were taken on those occasions "to prevent the spreading of the disease," and no epidemics followed. In the *Glasgow Herald* of the 5th ult., will be found a paper by Mr. Marshall, (a gentleman who seems to reason with great acuteness), which illustrates this part of our subject. This gentleman appears to have had a good deal of experience in Ceylon when the disease raged there, and I shall have occasion to refer hereafter to his statements, which I consider of great value. Nobody can be so absurd as to expect, that in the instances to which I refer, *all* the symptoms which have ever been enumerated, should have occurred in each case; for neither in India nor any-where else could all the grave symptoms be possibly united in any one case; for instance, great retching, and a profuse serous discharge from the bowels, have very commonly occurred where the disease has terminated fatally: yet it is not less certain, that even in the epidemics of the same year, death has often taken place in India more speedily where the stomach and bowels have been but little affected, or not at all. To those who give the subject of cholera all the attention which it merits, the consideration of some of those cases which have, within the last few weeks, appeared in the journals of this country, cannot fail to prove of high interest, and must inspire the public with confidence, inasmuch as they show, *beyond all doubt*, that the disease called cholera, as it has appeared in this country, and however perfectly its symptoms may resemble the epidemic cholera of other countries, *is not* communicable. On some of those cases so properly placed before the public, I shall perhaps be soon able to offer a few remarks: meanwhile, I shall here give the abstract of a case, the details of which have not as yet, I believe, appeared, and which must greatly strengthen people in their opinion, that these cholera cases, however formidable the symptoms, and though they sometimes end rapidly in death, still do not possess the property of communicating the disease to others. I do not mean to state that I have myself seen the case, the details of which I am about to give, but aware of the accuracy of the gentleman who has forwarded them to me, I can say, that although the communication was not made by the medical gentleman in charge of the patient, the utmost reliance may be placed on the fidelity of those details: —

Thursday, August 11th, 1831, Martin M'Neal, aged 42, of the 7th Fusileers, stationed at Hull, was attacked at a little before four a. m., with severe purging and vomiting – when seen by his surgeon at about four o'clock, was labouring under spasms of the abdominal muscles, and of the calves of the legs. What he had vomited was considered as being merely the contents of the stomach, and, as the tongue was not observed to be stained of a yellow colour, it was inferred that no bile had been thrown up. He took seventy drops of laudanum, and diluents were ordered. Half-past six, seen again by the surgeon, who was informed that he had vomited the tea which he had taken; no appearance of bile in what he had thrown up; watery stools, with a small quantity of feculent matter; thirst; the spasms in abdomen and legs continued; countenance not expressive of anxiety; skin temperate; pulse 68 and

soft; the forehead covered with moisture. Ordered ten grains of calomel, with two of opium, which were rejected by the stomach, though not immediately.

Eight o'clock a. m. The features sinking, the temperature of the body now below the natural standard, especially the extremities; pulse small; tongue cold and moist; a great deal of retching, and a fluid vomited resembling barley-water, but more viscid; constant inclination to go to stool, but passed nothing; the spasms more violent and continued; a state of collapse the most terrific succeeded. At nine o'clock, only a very feeble action of the heart could be ascertained as going on, even with the aid of the stethoscope; the body cold, and covered with a clammy sweat, the features greatly sunk; the face discoloured; the lips blue; the tongue moist, and very cold; the hands and feet blue, cold, and shrivelled, as if they had been soaked in water, like washerwomen's hands; no pulsation to be detected throughout the whole extent of the upper or lower extremities; the voice changed, and power of utterance diminished. He replied to questions with reluctance, and in monosyllables; the spasms became more violent, the abdomen being, to the feel, as hard as a board, and the legs drawn up; cold as the body was, he could not bear the application of heat, and he threw off the bed-clothes; passed no urine since first seen; the eyes became glassy and fixed; the spasms like those of tetanus or hydrophobia; the restlessness so great, that it required restraint to keep him for ever so short a time in any one position. A vein having been opened in one of his arms, from 16 to 20 ounces of blood were drawn with the greatest difficulty. During the flowing of the blood, there was great writhing of the body, and the spasms were very severe – friction had been arduously employed, and at ten a. m. he took a draught containing two and a half drachms of laudanum, and the vomiting having ceased, he fell asleep. At two p. m. re-action took place, so as to give hopes of recovery. At four p. m. the coldness of the body, discoloration, &c., returned, but without a return of the vomiting or spasms. At about half-past eight he died, after a few convulsive sobs.

On a post-mortem examination, polypi were found in the ventricles of the heart, and the cavæ were filled with dark blood. Some red patches were noticed on the mucuous membrane; but the communication forwarded to me does not specify on what precise part of the stomach or intestinal canal; and my friend does not appear to attach much importance to them, from their common occurrence in a variety of other diseases. It remains to be noticed, that the above man had been at a fair in the neighbourhood on the 9th (two days preceding his attack), where, as is stated, he ate freely of fruit, and got intoxicated. On the 10th he also went to the fair, but was seen to go to bed sober that night. The disease did not spread to others, either by direct or indirect contact with this patient.

Now let us be frank, and instead of temporising with the question, take up in one hand the paper on "cholera spasmodica" just issued, for our guidance, from the College of Physicians by the London Board of Health, and in the other, this case of Martin M'Neal (far from being a singular case this year, in most of the important symptoms), – let the symptoms be compared by those who are desirous that the truth should be ascertained, or by those who are not, and if distinctions can be made out, I must ever after follow the philosophy of the man who doubted his own existence. The case, as it bears on certain questions connected with cholera, *is worth volumes of what has been said on the same subject*. Let it be examined by the most fastidious, and the complete identity cannot be got rid of, even to the *blue skin*, the *shrivelled fingers*, the *cold tongue*, the *change in voice*, and the *suppression of urine*, considered in some of the descriptions to be found in the pamphlet issued by the Board of Health, as so characteristic of the "Indian" cholera; and this, too, under a "constitution of the atmosphere" so remarkably disposed to favour the production of cholera of one kind or other, that Dr. Gooch, were he alive, or any close reasoner like him, must be satisfied, that were this remarkable form of the disease communicable, no circumstance was absent which can at all be considered essential to its propagation. As the symptoms in the case of M'Neal, were, perhaps, more characteristically grouped than in any other case which has been recorded in this country, so it has also in all probability occurred, that more individuals had been in contact with him during his illness and after his death, as the facility in obtaining persons to attend the sick, rub their bodies, &c., must be vastly greater in

the army than in ordinary life; so that in such cases it is not a question of one or two escaping, but of *many*, which is always the great test.

Of the College of Physicians we are all bound to speak with every feeling of respect, but had the document transmitted by that learned body to our government, on the 9th of June last, expressed only a "philosophic doubt," instead of making an assertion, the question relative to the contagion or non-contagion of the disease, now making ravages in various parts of Europe, would be less shackled among us. People are naturally little disposed to place themselves, with the knowledge they may have obtained from experience and other sources, in opposition to such a body as the College: but as, in their letter to government of the 18th of June, they profess their readiness, should it be necessary, to "re-consider" their opinion, we, who see reason to differ from them, may be excused for publishing our remarks. It seems surprising enough that, in their letter to government of the 9th of June, the College should have given as a reason for their decision as to the disease being infectious (meaning, evidently, what some call contagious, or transmissible from *persons*) – "having no other means of judging of the nature and symptoms of the cholera than those furnished by the documents submitted to us." Now, according to the printed parliamentary papers, among the documents here referred to as having been sent by the Council to the College, was one from Sir William Crichton, Physician in Ordinary to the Emperor of Russia, in which a clear account is given of the symptoms as they presented themselves in that country; and, if the College had previously doubted of the identity of the Russian and Indian cholera, a comparison of the symptoms, as they were detailed by Sir William, with those described in various places in the *three volumes* of printed Reports on the cholera of India, in the college library, must at once have established the point in the affirmative. In fact, we know, that the evidence of Dr. Russell, given before the College, when he heard Sir William's description of the disease read, fully proved this identity to the satisfaction of the College. Had the vast mass of information contained in the India Reports, together with the information since accumulated by our Army Medical Department, been consulted, all which are highly creditable to those concerned in drawing them up, and contain incomparably better evidence, that is, evidence more to be relied on, than any which can be procured from Russia or any other part of the world – had these sources of information been consulted, as many think they should in all fairness have been, the College would probably have spoken more doubtingly as to cholera, in any form, possessing the property of propagating itself from person to person. Much of what passes current in favour of the communication of cholera rests, I perceive, on statements the most vague, assertions in a general way, as to the security of those who shut themselves up, &c. To show how little reliance is to be placed on such statements, even when they come from what ought to be good authority, let us take an instance which happened in the case of yellow fever. Doctor, now Sir William Pym, superintendent of the quarantine department, published a book on this disease in 1815, in which he stated, that the people shut up in a dock-yard, during the epidemic of 1814, in Gibraltar, escaped the disease, and Mr. William Fraser, also of the quarantine, and who was on the spot, made a similar statement. Now, we all believed this in England for several years, when a publication appeared from Dr. O'Halloran, of the medical department of Gibraltar garrison, in which he stated that he had made inquiries from the authorities at that place, and that he discovered the whole statement to have been without the smallest foundation, and furnishes the particulars of cases which occurred in the dock-yard, among which were some deaths; this has never since been replied to – so much as a caution in the selection of proofs.

To show, further, how absurdly statements respecting the efficacy of cordons will sometimes be made, it may be mentioned that M. D'Argout, French minister of public works, standing up in his place in the chamber, *on the 3rd instant (Sept.)*, and producing his estimates for additional cordons, &c., stated, by way of proving the efficacy of such establishments, that in Prussia, where, according to him, cordon precautions had been pre-eminently rigorous, and where "*le territoire a été défendu pied à pied*," such special enforcement of the regulations was attended with "*assez de succès*:" in the

meantime the next mail brings us the official announcement (*dated Berlin, Sept. 1*) of the disease having made its appearance there!

To conclude, for the present: if there be one reason more than another why the question of cholera should be scrutinized by the highest tribunal – a parliamentary committee – it is, that in the "papers" just issued by the Board of Health, the following passage occurs (page 36): – "But in the event of such removal not being practicable, on account of extreme illness or otherwise, the prevention of all intercourse with the sick, even of the family of the person attacked, must be rigidly observed, unless," &c. There are some who can duly appreciate all the consequences of this; but let us hope that the question is still open to further evidence, in order to ascertain whether it be really necessary that, in the event of a cholera epidemic,

"The living shall fly from
The sick they should cherish."

LETTER II

In my last letter I adverted to the opinion forwarded to his Majesty's Council on the 9th of June last from the College of Physicians, in which the cholera, now so prevalent in many parts of Europe, was declared to be communicable from person to person. We saw that they admitted in that letter (see page 16 of the Parliamentary Papers on Cholera) the limited nature of the proofs upon which their opinion was formed; but I had not the reasons which I supposed I had for concluding, that because they used the words "ready to reconsider," in their communication of the 18th of same month to the Council, they intended to *reconsider* the whole question. Indeed this seems now obvious enough, as one of the Fellows of the College who signed the Report from that body on the 9th of June (Dr. Macmichael) has published a pamphlet in support of the opinion already given, in the shape of a letter addressed to the President of the College, whose views, Dr. Macmichael tells us, *entirely coincide* with his own; so that there is now too much reason to apprehend that in this quarter the door is closed. Contagionist as I am, in regard to those diseases where there is evidence of contagion, I find nothing in Dr. Macmichael's letter which can make an impression on those who are at all in the habit of investigating such subjects,² and who, dismissing such inductions as those which he seems to consider legitimate, rely solely on facts rigorously examined. He must surely be aware that most of the points which he seems to think ought to have such influence in leading the public to believe in the contagion of cholera, might equally apply to the influenza which this year prevailed in Europe, and last year in China, &c.; or to the influenza of 1803, which traversed over continents and oceans, *sometimes in the wind's eye, sometimes not*, as frequently mentioned by the late Professor Gregory of Edinburgh. Who will now stand up and try to maintain that the disease in those epidemics was propagated from person to person? Could more have been made of so bad a cause as contagion in cholera, few perhaps could have succeeded better than Dr. Macmichael, and no discourtesy shall be offered him by me, though he does sometimes loose his temper, and say, among other things not over civil, nor quite *comme il faut*, from a Fellow of the College, that all who do not agree with him as to contagion "will fully abandon all the ordinary maxims of prudence, and remain obstinately blind to the dictates of common sense!" —*fort, mais peu philosophique Monsieur le Docteur*. The time has gone by when ingenious men of the profession, like Dr. Macmichael, might argue common sense out of us; it will not even serve any purpose now that other names are so studiously introduced as *entirely coinciding* with Dr. Macmichael; for, in these days of reform in every thing, *opinions*, will only be set down at their just value by those who pay attention to the subject.

Referring once more to the Report of the 9th of June, made by the College to the Council, and signed by the President as well as by Dr. Macmichael, the cholera was there pronounced to be a communicable disease, when they had, as they freely admit, "no other means of judging of the nature and symptoms of the cholera than those furnished by the documents submitted to them." The documents submitted were the following, as appears from the collection of papers published by order of Parliament: — Two reports made to our government by Dr. Walker, from Russia; a report from Petersburg by Dr. Albers, a Prussian physician; and a report, with inclosures, regarding Russian quarantine regulations, from St. Petersburg, by Sir W. Creighton. Dr. Walker, who was sent from St. Petersburg to Moscow, by our ambassador at the former place; states, in his first report, dated in March, that the medical men seemed to differ on the subject of contagion, but adds, "I may so far state, that by far the greater number of medical men are disposed to think it not contagious." He says, that on his arrival at Moscow, the cholera was almost extinct there; that in twelve days he had been

² I presume that I shall not be misunderstood when I say, *Would that the cholera were contagious*— for then we might have every reasonable hope of staying the progress of the calamity by those cordon and quarantine regulations which are now not merely useless, but the bane of society, when applied to cholera or other non-contagious diseases.

able to see only twenty-four cases, and that he had no means of forming an opinion of his own as to contagion. In a second report, dated in April from St. Petersburg, this gentleman repeats his former statement as to the majority of the Moscow medical men not believing the disease to be contagious (or, as the College prefer terming it, infectious), and gives the grounds on which their belief is formed, on which he makes some observations. He seems extremely fair, for while he states that, according to his information, a peculiar state of the atmosphere "was proved by almost every person in the city (Moscow), feeling, during the time, some inconvenience or other, which wanted only the exciting cause of catching cold, or of some irregularity in diet, to bring on cholera;" that "very few of those immediately about the patients were taken ill;" that he "did not learn that the contagionists in Moscow had any strong particular instances to prove the communication of the disease from one individual to another;" and that he had "heard of several instances brought forward in support of the opinion (contagion), but they are not fair ones: " he yet mentions where exceptions seem to have taken place as to hospital attendants not being attacked, but he has neglected to tell us (a very common omission in similar statements), whether or not the hospitals in which attendants were attacked were situated in or near places where the atmosphere seemed *equally productive of the disease in those not employed in attending on sick*. This clearly makes all the difference, for there is no earthly reason why people about the sick should not be attacked, if they breathe the same atmosphere which would seem to have so particular an effect in producing the disease in others; indeed there are good reasons why, during an epidemic, attendants should be attacked in greater proportion; for the constant fatigue, night-work, &c., must greatly predispose them to disease of any kind, while the great additional number always required on those occasions, precludes the supposition of the majority so employed being *seasoned* hospital attendants, having constitutions impenetrable to contagion. Those questions are *now* well understood as to yellow fever, about which so much misconception had once existed. The proofs by disinterested authors (by which I mean those unconnected with quarantine establishments, or who are not governed by the *expediency* of the case) in the West Indies, America, and other places, show this in a clear light; but the proofs which have for some time past appeared in various journals respecting the occurrences at Gibraltar, during the epidemic of 1828, are particularly illustrative. By the testimony of three or four writers, we find that *within certain points*, those in attendance on sick, in houses as well as hospitals, were attacked with the fever, in common with those who were not in attendance on sick; but that, where people remained at ever so short a distance beyond those points, during the epidemic influence, *not a single instance* occurred of their being attacked, though great numbers had been in the closest contact with the sick, and frequently too, it would appear, under circumstances when contagion, had it existed, was not impeded in its usual course by a very free atmosphere: —*sick individuals, for instance, lying in a small house, hut, or tent, surrounded, during a longer or shorter space of time, by their relatives, &c.* A full exposure of some very curious misstatements on these points, made by our medical chief of the quarantine, will be found from the pen of the surgeon of the 23d regiment, in the *Edinburgh Medical and Surgical Journal*, No. 106.³ Those who are acquainted with the progress of cholera in India, must be aware how a difference in the height of places, or of a few hundred yards (*indeed sometimes of a few yards*) distance, has been observed to make all the difference between great suffering and complete immunity: – the printed and manuscript reports from India furnish a vast number of instances of this kind; and, incredible as it may appear, they furnish instances where, *notwithstanding the freest intercourse*, there has been an abrupt line of

³ The writer of this, who may be known by application at the printer's, when the present excitement is at an end, is not only prepared to show, *on a fitting occasion*, the correctness of the statements of Dr. Smith as well as those by Dr. O'Halloran just referred to – but also, that in the investigations, in 1828, connected with the question of yellow fever at Gibraltar, facts were perverted in the most scandalous manner, in order to prove the disease imported and contagious: – that individuals had been suborned: – that persons had been in the habit of putting leading questions to witnesses: – that those who gave false evidence have been, in a particular manner, remunerated: – that threats were held out: – and, in short, that occurrences of a nature to excite the indignation of mankind, took place on that occasion; and merited a punishment, not less severe, than a Naval Officer who should give, designedly, a false bearing and distance of rocks.

demarcation observed, beyond which the disease did not prevail. A most remarkable instance of this occurred in the King's 14th regiment, in 1819, during a cholera epidemic, when the light company of the regiment escaped almost untouched, owing to no other apparent cause than that they occupied the extremity of a range of barrack in which all the other companies were stationed! so that there would truly seem to be more things "on earth than are dreamt of in the philosophy" of contagionists. This seems so remarkable an event, that the circumstance should be more particularly stated: – "The disease commenced in the eastern wing of the barracks, and proceeded in a westerly direction, but suddenly stopped at the 9th company; the light infantry escaping with one or two slight cases only." – (*Bengal Rep.* 311.) It appears (*loc. cit.*) that 221 attacks took place in the other nine companies. We find (*Bombay Rep.* p. 11.) that, from a little difference in situation, two cavalry regiments in a camp were altogether exempt from the disease, while all the other regiments were attacked. Previous to closing these remarks, which seemed to me called for on Dr. Walker's second Report, it is fair to state, that in certain Russian towns which he names, he found that the medical men and others were convinced that the cholera was brought to them "*somehow or other,*" an impression quite common in like cases, as we learn from Humboldt, and less to be wondered at in Russia than most places which could be mentioned. It will not be a misemployment of time to consider now the next document laid before the College, to enable them to form their opinion, – the Report of Dr. Albers, dated in March, and sent from St. Petersburg; – this gentleman, who was at the head of a commission sent by the Prussian government to Moscow, states, that at St. Petersburg, *where the disease did not then reign*, the authorities and physicians were contagionists; but at Moscow, where it had committed such ravages, "almost all strenuously maintain that cholera is not contagious." The following extract seems to merit particular attention: —

"When the cholera first reached Moscow, all the physicians of this city were persuaded of its contagious nature, but the experience gained in the course of the epidemic, has produced an entirely opposite conviction. They found that it was impossible for any length of time completely to isolate such a city as Moscow, containing 300,000 inhabitants, and having a circumference of nearly seven miles (versts?), and perceived daily the frequent frustrations of the measures adopted. During the epidemic, it is certain that upwards of 40,000 inhabitants quitted Moscow, of whom a large number never performed quarantine; and notwithstanding this fact, *no case is on record of the cholera having been transferred from Moscow to other places*, and it is equally certain, that in *no situation* appointed for quarantine, *any case of cholera has occurred*. That the distemper is not contagious, has been yet more ascertained by the experience gathered in this city (Moscow). In many houses it happened, that one individual attacked by cholera was attended indiscriminately by all the relatives, and yet did the disease not spread to any of the inmates. It was finally found, that not only the nurses continued free of the distemper, but also that they promiscuously attended the sick chamber, and visited their friends, without in the least communicating the disease. There are even cases fully authenticated, that nurses, to quiet timid females labouring under cholera, have shared their beds during the nights, and that they, notwithstanding, have escaped uninjured in the same manner as physicians in hospitals have, without any bad consequences, made use of warm water used (a moment before) by cholera patients for bathing.

"These, and numerous other examples which, during the epidemic (we ought, perhaps, to call it endemic) became known to every inhabitant of Moscow, have confirmed the conviction of the non-infectious nature of the disease, a conviction in which their personal safety was so much concerned.

"It is also highly worthy of observation, that all those who stand up for contagion, *have not witnessed* the cholera, which is, therefore, especially objected to their opinion by their opponents." He closes by the observation, "The result of my own daily experience, therefore, perfectly agrees with the above-stated principle, namely, notwithstanding all my inquiries, *I have met with no instance which could render it at all probable that the cholera is disseminated by inanimate objects.*" The words in italics are as in the Parliamentary papers on Cholera, pp. 8 and 9. Here is something to help to

guide people in forming opinions, and to help governments on quarantine questions; but owing to a portion of the "perverseness" which Dr. Macmichael in anger talks about, Dr. Albers still *speculates* upon cholera being contagious, and the College, it would seem, take up his speculations and sink his very important facts. Sir William Creighton's Report gives what purports to be an extract from a memorial of his on cholera, given in to the St. Petersburg Medical Council, tending to establish the contagious character of the disease; and with this a report by the extraordinary committee appointed by the Emperor to inquire into the Moscow epidemic. The disease had not appeared at St. Petersburg when he drew up his Memorial, and it does not appear from any-thing which can be seen in the extracts he furnishes, that he had personal knowledge of any part of what he relates. He gives the reported progress of the disease on the Volga and the Don, but is extremely deficient exactly where one might have expected that, from the greater efficiency of police authorities, &c., his information on contagion would have been more precise, viz., the introduction of the disease into Moscow, which could not, it would seem have been by material objects, for, according to the Committee, composed "of the most eminent public officers," – "the opinion of those who do not admit the possibility of contagion by means of material objects, has for its support both the majority of voices, and the scrupulous observance of facts. The members of the Medical Council have been convinced by their own experience, as also by the reports of the physicians of the hospitals, that, after having been in frequent and even habitual communication with the sick, their own clothes have never communicated the disease to any one, even without employing means of purification. Convalescents have continued to wear clothes which they wore during the disease – even furs – without having them purified, and they have had no relapse. At the opening of bodies of persons who had died of cholera, to the minute inspection of which four or five hours a day for nearly a month were devoted, neither those who attended at their operations, nor any of the assisting physicians, nor any of the attendants, caught the infection, although, with the exception of the first day, scarcely any precautions were used. But what appears still more conclusive, a physician who had received several wounds in separating the flesh, continued his operations, having only touched the injured parts with caustic. A drunken invalid having also wounded himself, had an abscess, which doubtless showed the pernicious action of the dead flesh, but the cholera morbus did not attack him. In fine, foreign *Savans*, such as Moreau de Jonnés and Gravier, who have recognized, in various relations, the contagious nature of the cholera morbus, do not admit its propagation by means of goods and merchandise." (*Parl. Papers on Chol.* p. 13.) With the above documents the Council transmitted to the College a short description of the process of cleaning hemp in the Russian ports; and, lastly, the copy of a letter to the clerk of the Council from our ever-vigilant, though never-sufficiently-to-be-remunerated, head guardian of the quarantine department, who, taking the alarm, very properly recommends, as in duty bound, that a stir be forthwith made in all the pools, and creeks, and bays, &c., of the united kingdom, in order that all those notoriously "susceptible" old offenders, skins, hemp, flax, rags, &c., may be prevented from carrying into execution their felonious intention of covering the landing of a dire enemy. In truth, from the grave as well as from the sublime, there often seems to be "but a step;" and in reading over this gentleman's suggestions about *susceptibles* and *non-susceptibles*, one may fancy himself, instead of being in the land of thinking people, to be in the land of Egypt, where, as we are informed (Madden, 1825), the sage matrons discuss the point, whether a cat be not a better vehicle for contagion than a dog: – a horse may be trusted, they say, but as to an ass, he is the most incorrigible of contagion smugglers; – of fresh bread we never need be afraid, but the susceptibility of butcher's meat is quite an established thing: – or we might fancy ourselves transported to regions of romance, where it is matter of profound deliberation, whether an egg shall be broken at the large or the small end. Such things are too bad for the nineteenth century; and in England, too, with her enlightened parliament! But until these questions are better examined, our guardian must bestir himself about articles susceptible of cholera contagion, while he enjoys his good quarantine pay, his good half pay

from another department as I believe, and withall, if we are not misinformed, a smart pension from the Gibraltar revenue, for what granted nobody can tell.

The documents above referred to, would appear then to be the whole on which the College admit that they formed their opinions, and people may now judge whether the verdict be according to the evidence, or whether it be not something in the *lucus a non lucendo* mode of drawing conclusions: – most persons will probably think that, on such evidence, there might at least have been a qualified opinion. It appears, however, that having come to a *decision* on the 9th of June, that the disease was communicable from person to person, they in three days after, approved of persons being sent to Russia to find out whether they had decided rightly or not. Are we now to expect that, should the occasion need, they will heroically make war against their own declared opinion? For my part I expect from them all that should be expected from men; and the liberal part of the world will not fail to see from this, that I do not despair of even Dr. Macmichael, being still open to conviction. Let it not be for a moment understood that, in any-thing which has been said, or which may remain to be said respecting this gentleman, or in any-thing which may be hereafter said respecting Dr. Bisset Hawkins's work, I mean to insinuate that contagion in cholera is not with them a matter of conscience; but I certainly do mean to say that their zeal has manifestly warped their judgment; and not only this, but that it has prevented them from laying statements before the public on the cholera questions with all the impartiality we might have expected from gentlemen of their character in the profession.

In Dr. Macmichael's pamphlet, consisting of thirty-two pages, and professing to be a consideration of the question, "Is cholera contagious?" we scarcely find the disease mentioned till we come to page 25; the pages up to this being occupied chiefly by a recapitulation of opinions formerly given "on the progress of opinion upon the subject of contagion;" – on the opinions of old writers as to the contagion of plague, small-pox, measles, &c.: – he would infer that whereas small-pox and certain other diseases have, by more accurate observations made in comparatively modern times, been taken from the place they once held, and ranged among diseases decidedly contagious, so ought cholera also to be now pronounced contagious! As an inducement to us to adopt this as good logic, he assures us that the list of diseases deemed contagious by wise men is on the increase – that non-contagionists are *perverse* people, *blunderers*, and so forth! As to his epithets, it shall only be said that among the disbelievers of contagion in cholera, and certain other diseases probably reputed contagious by Dr. Macmichael, are to be found hundreds possessing as much candour, as cultivated minds, and as much practical knowledge of their profession, as any contagionists, whether they be Fellows of a College or not; but as to the statement of Dr. Macmichael, is it true that we have been adding to the list of contagious diseases? Not within the last fifty years certainly. Even the influenza of 1803 was, if I mistake not greatly, termed, very generally, "infectious catarrh," but what professional man would term the influenza of 1831 so? Are there not yet remaining traces of the generally exploded doctrine of even contagion in ague, at one time attempted to be maintained? M. Adouard, of Paris, still indeed holds out. Do we not know that Portal, at one period of his life at least, would not, for fear of "infection," open the body of a person who had died of phthisis? Where is the medical man now to be found who would set up such a plea? or where, except in countries doomed to eternal barbarism, are patients labouring under consumption avoided now, as they were in several parts of the world at one time, just as if they laboured under plague, and all for the simpleton's reason that the disease *often runs through families*? What disinterested man will, on due examination of all that has been written on yellow fever, stand up now in support of its being a contagious disease, of which some thirty or forty years ago there was so general a belief? On croup, and a few more diseases, many still think it *wise to doubt*. Is dysentery, known to make such ravages sometimes, especially in armies, considered now, as at one time, to be contagious? If Dr. Macmichael's pamphlet was intended altogether for readers not of the profession, *which seems very probable*, his purposes will perhaps be answered, at least for a time, but I do not see how it can make an impression on medical men. Why not have been a little more candid when quoting Sydenham on small-pox, &c. and have quoted

what that author says of the disease which he (Dr. M.) professes to write about, – the cholera? The public would have means of judging how far the disease which was prevalent in 1669, resembled the "cholera spasmodica," &c., of late years. Many insist upon an identity (Orton among others), and yet Sydenham saw no reason for suspecting a communicable property. It might have been more to the point had Dr. Macmichael, instead of quoting old authorities on small-pox, measles, &c. quoted some authorities to disprove that Orton and others are wrong when they state it as their belief that some of those old epidemics in Europe, about which so much obscurity hangs, were nothing more or less than the cholera spasmodica. Mead's short sketch of the "sweating sickness" does not seem very inapplicable: – "Excessive fainting and inquietude inward burnings, headach, sweating, vomiting, and diarrhœa."⁴ In the letter to the President of the College we see no small anxiety to prove that the malignant cholera is of modern origin also in India, for the proofs from Hindoo authorities, as given in the volume of *Madras Reports*, are slighted. These Reports, as well as those of the other presidencies, are exceedingly scarce, but whoever can obtain access to them will find in the translations at pp. 253 and 255 (not at page 3, as quoted by Dr. Macmichael), enough probably to satisfy him that cholera is the disease alluded to there. But I think that we have at page 31 of Dr. Macmichael's letter, no small proof of a peculiarity of opinion, when we find that he there states that the evidence in the *Madras Reports* of the existence of epidemics of malignant cholera in India, on several occasions previous to 1817, rests on imperfect records, and that the description of the disease is too vague to prove the identity with the modern spasmodic cholera; for in this opinion he seems, as far as I have been able to discover, to stand alone among writers on cholera; – indeed it seems established, *on the fullest authority*, that cholera, in the same form in which it has appeared epidemically of late years, has committed ravages in India on more than one occasion formerly: – this is fully admitted by Mr. Orton, an East India practitioner, who is one of the few contagionists.

For one piece of tact the author of the letter deserves great credit; for whereas his College collectively, when forming their opinion on the questions proposed to them by the Council, seemed to throw all India records overboard, – he, in his individual capacity, as author of the letter, sends after them all the Russian reports in support of contagion; for anxious as he is to prove his point, not a word do we get of the *on dits* so current in Russia about persons being attacked with the disease from smelling to hemp arrived from such or such a place; from having looked at a boatman who had been up the Volga or down the Volga, &c. &c.: all which statements, when duty inquired into, prove to be unsupported by any thing in the shape of respectable authority, and this is now, in all probability, pretty generally known to be the case, as Dr. Macmichael must be quite aware of.

To the medical gentlemen of India who have been concerned in the official reports, which do them, *en masse*, so much credit, Dr. Macmichael is little disposed to be complimentary; and, indeed, he seems to insinuate that those were rather stupid fellows who did not come to what he is pleased to consider "a just and right conclusion," as to contagion; he thinks, however, that he has got a few of "the most candid" to join in his belief. We shall see whether he had better reason to look towards the Ganges and Beema for a confirmation of his doctrines, than he had toward the Don or the Volga. How does the case stand with respect to one of the gentlemen whom he quotes, – Mr. Jukes, of the Bombay Establishment? This gentleman, like all who speak of cholera, mentions circumstances as to the progress of the disease which he cannot comprehend, and Dr. Macmichael shows us what those circumstances are; but Dr. Macmichael does not exhibit to us *what does* come perfectly within Mr. Jukes's comprehension, but which is not quite so suitable to the doctor's purpose. This omission I shall take the liberty to supply from an official letter from Mr. Jukes in the Bombay Reports: – "I

⁴ If the progress of the sweating sickness was similar to that of cholera, the advice of the King to Wolsey was sound; for instead of recommending him to rely on any-thing like cordon systems, or to shut himself up surrounded by his guards, he tells him (see *Ellis's* letters) to "fly to *clene* air incontinently," on the approach of the disease. I use the words *approach of the disease* occasionally, as it is a manner of expression in general use, but it is far from being strictly applicable when I speak of cholera; *the cause* of the disease it is which I admit travels or springs up at points, and not the disease itself in the persons of individuals, or its germs in inanimate substances.

have had no reason to think it has been contagious here, neither myself nor any of my assistants, who have been constantly amongst the sick, nor any of the hospital attendants, have had the disease. It has not gone through families when one has become affected. It is very unlike contagion too, in many particulars." &c. – (*Bombay Reports*, page 172.) – Ought we not to be a little surprised that so great an admirer of candour, as Dr. Macmichael seems to be, should, while so anxious to give every information to his readers, calculated to throw light upon the subject of cholera, omits the above important paragraph, which we find, by the way *immediately precedes* the one upon opinions and difficulties which he quotes from the same gentleman? But let us examine what the amount of force is, which can be obtained from that part of Mr. Jukes's paper, which it does please Dr. Macmichael to quote: – "If it be something general in the atmosphere, why has it not hitherto made its appearance in some two distinct parts of the province at the same time? Nothing of this kind has, I believe, been observed. It still seems creeping from village to village, rages for a few days, and then begins to decline." I find myself unable, at this moment, to ascertain the extent of Mr. Jukes's means of obtaining information as to what was passing in other parts of his province; but I think the following quotation, on which I am just now able to lay my hand, will not only satisfactorily meet what is here stated, but must, in the public opinion, be treasured, as it serves at once to displace most erroneous ideas long prevalent, and which, I believe, greatly influenced men's decisions as to contagion: – "It may, then, first be remarked, that the rise and progress of the disorder were attended by such circumstances as showed it to be entirely independent of contagion for its propagation. Thus we have seen that it arose at nearly one and the same time in many different places, and that in the same month, nay, in the same week, it was raging in the unconnected and far-distant districts of Behar and Dacca." (*Bengal Reports*, p. 125.) Again (p. 9), that in Bengal "it at once raged simultaneously in various and remote quarters, without displaying a predilection for any one tract or district more than for another; or any thing like regularity of succession in the chain of its operations." In support of what is stated in these extracts, the fullest details are given as to dates and places; and at page 9 of those Reports, a curious fact is given, "That the large and populous city of Moorshedabad, from extent and local position apparently very favourably circumstanced for the attacks of the epidemic, should have escaped with comparatively little loss, whilst all around was so severely scourged." This seems to have been pretty similar to what is now taking place with respect to the city of Thorn, which remains free from cholera, though the communication is open with divers infected places in every direction. Should Thorn still be attacked by the disease (as it sooner or later will, in all human probability), the contagionists *par métier* will try to establish a case of hemp or hare-skin importation, I have no doubt. I wonder much that Dr. Macmichael or Dr. B. Hawkins, when favouring us with eastern quotations, did not give the public the opinion of Dr. Davy, who is so well known in Europe, and who saw the cholera in Ceylon; his conjecture (quite accessible, I believe, to every medical man in London) may perhaps be as valuable as that of any other person. The following is a copy of it: – "The cause of the disease is not any sensible change in the atmosphere; yet, considering the progress of the disease, its epidemic nature, the immense extent of country it has spread over, we can hardly refuse to acknowledge that its cause, though imperceptible, though yet unknown, does exist in the atmosphere. It may be extricated from the bowels of the earth, as miasmata were formerly supposed to be; it may be generated in the air, it may have the properties of radiant matter, and, like heat and light, it may be capable of passing through space unimpeded by currents; like electricity, it may be capable of moving from place to place in an imperceptible moment of time." Dr. Davy is an army physician, and the report of which this is an extract, may be seen at the Army Medical Office, a place which, of late years, has become a magazine of medical information of the most valuable kind in Europe. There is this difference between army and other information on cholera, that (whether in the King's or E. I. Company's service) the statements given by the medical gentlemen have their accuracy more or less guaranteed by a certain system of military control over the documents they draw up: thus, in the circumstance already noticed as having occurred in the 14th regiment, we have

every reason to rely upon its accuracy, which we could not have in a similar statement among the population of any country; and we have, I think, no reason to believe that in pronouncing the cholera of Ceylon not contagious, Dr. Davy, as well as two other gentlemen of high character and experience (Drs. Farrel and Marshall), have not gone upon such data as may bear scrutiny.

LETTER III

Having given, in my last letter, Dr. Davy's views as to the cause of cholera, I may so far remark just now regarding them, that they are not new, or peculiar to him; and that it may be well, before Dr. Macmichael or others pronounce them vague, that they should inquire whether some of those causes have not been assigned for the production of certain epidemics, by one of the soundest heads of Dr. Macmichael's college – Dr. Prout, who seems, if we have not greatly mistaken him, to have been led to the opinion by some experiments of Herschell, detailed in the *Philosophical Transactions* of the year 1824. They should recollect that other competent persons devoted to researches on such subjects (Sir R. Phillips among the number) admit *specific local atmospheres* (not at all *malaria* in the usual sense of the term), produced by irregular streams of specific atoms from the interior of the earth, and "arising from the action and re-action of so heterogeneous a mass." For my part I feel no greater difficulty in understanding how our bodies, "fearfully and wonderfully made" as we are, should be influenced by those actions, re-actions, and combinations, to which Sir Richard refers, and of "whose origin and progress the life and observation of man can have no cognizance," than how they are influenced by other invisible agents, the existence of which I am compelled to admit. – If the writer of the article on cholera in the *Westminster Review*, for October, 1831, do not find all his objections met by these observations, I must only refer him to the *quid divinum* of Hippocrates: – but I must protest against logic such has been employed by certain members of our Board of Health, who lately, on the examination of gentlemen of the profession who had served in India, and who had declared the disease not to be communicable, came to the conclusion that it must, nevertheless, be so, as those gentlemen could not show *what it was* owing to.

Most extraordinary certainly it does appear, that while Dr. Macmichael goes to the trouble of giving us (p. 27) the views of *a captain* (!) as to the progress of cholera at a certain place in India, he should have refrained altogether from referring, on the point of contagion or non-contagion, to the report of such a person as Dr. Davy, or to the reports of this gentleman's colleagues at Ceylon, Drs. Farrell and Marshall. Had Dr. Macmichael added a little to his extract from Capt. Sykes, by informing us of what that gentleman states as to the great mortality ("350 in one day") in the town of Punderpoor, "when the disease first commenced its ravages there," people would have means of judging how unlike this was to a contagious disease creeping from person to person in its commencement.

It is painful to be obliged to comment on the manner in which Dr. Bisset Hawkins has handled the questions relative to the Ceylon epidemic, which seems far from being impartial; for, while he quotes (p. 172) Dr. Davy, "a medical officer well known in the scientific world," as stating that the cause of the disease is not in any *sensible* changes in the state of the atmosphere, he breaks off suddenly at the word *atmosphere*, proceeds to talk of the changes in the muscles and blood of persons who die of the disease, and passing over the part quoted from Dr. Davy, near the close of my last letter, Dr. Hawkins leaves his readers to draw a very natural conclusion – that, as Dr. Davy admitted that there were no prevalent *sensible* states of the atmosphere to which the cholera could be attributed, *he, therefore*, believed it to have been propagated by contagion, an inference which we now see must be quite wide of the mark. Dr. Hawkins had, it appears, like many other medical gentlemen, access to the reports from Ceylon, &c., in the office of the chief of the army medical department in London, and it is to be regretted I think that, with respect to one of the Ceylon reports, he only tells us (p. 174) that "Mr. Staff-Surgeon Marshall reports from Candy, that of fifty cases which had occurred, forty died." Why more had not been quoted from a gentleman who had such ample means of witnessing the disease in its very worst form, I must leave to others to say; but, referring again to the highly interesting letter from Mr. Marshall on cholera, which appeared in the *Glasgow Herald*, of the 5th of August last, and in which, from many important observations which every body interested in cholera should read and study, the following remarks will be found: – "In no one instance did it seem to prevail

among people residing in the same house or barracks, so as to excite a suspicion that the contact of the sick with the healthy contributed to its propagation." "The Indian Cholera, as it is sometimes called, appears not to be essentially different from cholera as it occurs in this and all other countries." "I consider it, therefore, impossible for a medical practitioner to speak decisively from having seen one, or even a few cases of cholera in this country, and to say whether they are precursors of '*the epidemic cholera*' or not. That the disease is ever propagated by means of personal contact, or by the clothes of the sick, has not, as far as I know, been satisfactorily proved. The quality of contagion was never attributed to the disease in Ceylon, and I believe no-where did it occur in greater severity. I am aware that an attempt has been made to distinguish the ordinary cholera of this country from the '*epidemic cholera*,' by means of the colour or quality of the discharges from the bowels. In the former it is said the discharge is chiefly bile, while in the latter it is said to bear no traces of bile, but to be colourless and watery. How far is this alleged diagnosis well founded? I am disposed to believe that, in all severe cases of cholera, whether it be the cholera of this country, or the epidemic cholera, the secretion of bile is either suppressed, or the fluid is retained in the gall-bladder." Mr. Marshall, it may be observed, is the gentleman who was selected by the late Secretary at War, in consequence of his known intelligence, to remodel the regulations relative to military pensioners; and I understand that, in consequence of the manner in which he executed that very important duty, he has since been promoted. After what appears from the above quotations, how perfectly unwarrantable must the assertion of Dr. Bisset Hawkins seem, that "from the Coromandel coast it seems to have been transported by sea to Ceylon!"

We shall, I think, be able to see that the assumption of Drs. Macmichael and Hawkins, as to the importation of the disease into the Mauritius from Ceylon, is equally groundless with that of its alledged importation into the latter island; and here we have to notice the same want of candour on the part of those gentlemen, in not having furnished that public, which they professed to enlighten on the subject of cholera, with those proofs within their reach best calculated to display the truth; be it a part of my duty to supply the omissions of these gentlemen in this respect. The following is a copy of a letter accompanying the medical commission report at that island forwarded to General Darling, the then commanding officer, by the senior medical gentleman there.

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