Macmichael (Wm.)

IS THE

CHOLERA SPASMODICA OF INDIA

A

CONTAGIOUS DISEASE?

THE QUESTION CONSIDERED IN A LETTER ADDRESSED

TO

SIR HENRY HALFORD, BART., M. D.,

President of the Board of Health, &c. &c. &c.

BY

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My dear Sir Henry,

I have had the advantage of discussing most of the topics contained in this Essay in your society; and I know no one to whom it can with so much propriety be addressed as yourself, who, holding the highest rank in your profession, occupy also at this moment the important chair of President of the Board of Health. Looking at the enormous *cordons sanitaires* maintained by different nations of Europe, it might be supposed that all controversy on this subject had at length ceased; but, strange as it may appear, there are still persons who maintain the negative side of the question I now proceed to consider.

A few years ago, when it was proposed to abolish our laws of quarantine, I wrote a short pamphlet, which had for its title, 'A Brief Sketch of the Progress of Opinion upon the subject of Contagion.'

The arguments which were there advanced to prove that the notion of contagion is not an old prejudice, hastily and heedlessly adopted, which it remains for the liberal spirit of inquiry of the present day to ridicule and explode, have unfortunately received ample

illustration from the actual appearance of a new pestilence in the midst of civilized Europe, at whose fearful approach the world stands aghast. Some of the observations contained in that sketch, I shall beg leave to repeat, and, by showing how aptly they apply to the present question, Whether the Cholera of India be contagious or not? attempt to fill up that imperfect outline of a picture of former errors, and modern scepticism, so obstinate and perverse as to be scarcely credible.

The history of the eruptive fevers, which are now acknowledged, even by the most strenuous anti-contagionists, to be communicable from one person to another, first claims our attention. In this investigation, it will be curious to trace the dawn of suspicion in the minds of some of the early writers on these diseases; to watch the tardy development of truth, until finally the expression, 'febris contagiosa,' was introduced into definitions of them in all systems of modern nosology.

Rhazes, the oldest writer on the small pox, says, 'that the blood of youth is converted into that of manhood, as Must is changed into Wine, and that the symptoms of small-pox are the phenomena of that fermentation.'

Now, the original notion entertained by Sydenham of the essence of the small-pox was very similar to this, though not expressed in the same metaphorical language. In the first edition of his 'Methodus Curandi Febres' (1666), in his Chapter de Variolis, he says—

'I consider the small-pox to be a disease super-

induced upon the blood, assuming, as it were, a certain new condition, for the human blood is a fluid of a very rich and luxuriant nature, and contains within it many rich juices, so that, in the whole course of life, it rarely remains in the same state; but laying aside (as a slough) its original character and texture, once at least in our lives, it is disposed to undergo some striking change, and put on, if I may so express it, a new form.'

He then adds, and I will not venture to translate his words, lest I should misrepresent his meaning:—

'Atque hæc mihi, de his rebus sæpe meditanti, non inidonea ratio visa est, cur illorum sanguis qui nondum hanc mutationem passus est, eandem tam facile subeat: levissimo enim impulsu illuc inclinat, *vel ex ipsis effluviis ex ægrotantium sanguine transmissis:* non secus ac poma quæ ipsa matura sunt, etiam ea quæ in viciniâ reperiuntur, in eandem conditionem perducere solent.'—*De Variolis*, sec. 4, page 128.

This is a very near approach to the truth; though Sydenham immediately afterwards involves himself in a confused sort of refinement, and ultimately rejects the idea that the small-pox is caused by a poison admitted into the blood, and afterwards expelled from it by the various processes of the disease.

No very satisfactory reason can be assigned by the most enlightened modern pathologist, why we should be subject *once* only in our lives to an attack of the small-pox, and the theoretical explanation of Sydenham, that it is necessary that our blood should undergo a sort of fermentation, and put on a new form,

is perhaps as good as any other, that can at present be advanced.

But it is remarkable, that it was this very property of the disease (viz., of protecting against a future attack), which chiefly influenced the mind of Sydenham to reject the notion that contagion caused the small-pox; for he observes, that in other diseases, 'depending upon any malignant cause,' we daily see the contrary happen; in lues venerea, for instance, a person may be infected again and again, and receives no protection whatever from his former contamination. To the objection which might be urged, that persons sometimes have the small-pox twice, he answers, that he knows that fact, but that it happens so rarely, that it cannot invalidate his argument, for, generally speaking, he who once has had variolæ is considered safe for the rest of his life.

Another objection to the contagious nature of small-pox arose, in the mind of Sydenham, from the suddenness of its attack, and the rapid formation of so vast a quantity of purulent matter in the body of a person seized with this disease.

'Neque enim video quî fit, ut momento quasi temporis, sanguis alicujus, quem mox istâ contagie affectum supponemus, tanto purulento illius materiæ apparatu instructus sit, quantus in hoc morbo secernitur.' —Page 129.

These two considerations had so much weight with him, as to persuade him to give up the notion of contagion, and ultimately to adopt the idea, with which he set out, that the phenomena of the small-pox were

those of a sort of spontaneous change or fermentation of the blood, which, for some secret purpose or other, that fluid was destined to suffer once during the period of our existence. Besides, it ought to be observed, that in the passage I have quoted, from which it appears that he had a transient glimpse of the truth, it seems also that he thought the effluvia of the sick were only one of the causes, and not the sole cause of the disease; for the expression, 'vel ex ipsis effluviis ex ægrotantium sanguine transmissis,' means evidently that he considered the contiguity of another patient as only one, and that even the slightest of the various impulses to which the small-pox might possibly owe its origin.

The passages which I have quoted, are from the first edition of Sydenham's Treatise, entitled 'Methodus Curandi Febres,' published when he was about forty years of age (and if it be true, as has been asserted, that he did not begin the study of physic till he had passed the period of youth), when he could have been only a few years in practice.

Now, whether it was owing to the censures of others, or was the result of his own more enlarged experience, by which he became gradually more averse from anything like speculation, and more attached to practical observation, I know not; but in the edition of all his works, published (1685) only four years before his death, there is no trace whatever of any of the theoretical views in which he at first indulged. He somewhere says, with the diffidence characteristic of real talent and genius, 'mihi vero,

qui non ultra quam res ipsa loquitur, sapere audeo,' &c.; and in conformity with this prudent maxim, he confesses, in the last edition of his treatise, De Variolis (which embraced many additional points of practice, and might be supposed to contain all that he ever knew or had thought upon the subject), that he does not know what the essence of the small-pox is, but thinks that it is an inflammation of the blood and humours, yet of a different kind from other inflammations.

To us, in the present day, it must seem strange, that any one could have witnessed the epidemic spreading of small-pox, without suspecting that one person caught it from another. So, however, it was with respect to the progress of opinion on the subject of Contagion, and so, upon inquiry, it will be found to have been with regard to many other truths in the science of medicine. Before the days of Harvey, many points were made out connected with the doctrine of the circulation of the blood—the valves of the veins, for example, were known, the pulmonary circulation was understood; still the great inference which Harvey drew, had never yet been drawn. So often are we on the very threshold of a discovery, which, by some fatality, we miss, and when it is at length made, have only to express our astonishment that it had been overlooked!

The great maxim which guided Sydenham in his practice, was to bestow the nicest and most accurate attention upon the symptoms of diseases, which he thought to be the most infallible method of succeeding in searching out the true means of their cure. Nor

can any method, generally speaking, be devised better than this; but the very modesty, candour, and sincerity for which he was so distinguished, would seem, in the instance we have been noticing, to have proved rather an obstacle in his way, towards the investigation of the truth. It has been said of Sydenham, that he employed hypothesis, as the mere vehicle by which he conveyed his ideas; if this be true, it seems singular that he did not retain the notion of contagion (if it were only as a conjecture), for it would certainly have assisted him in the explanation of some of the phenomena of the small-pox. But we have seen, that when he found this idea irreconcileable, in some respects, with his preconceived views of the nature of infection, as proved by what takes place in another disease, he abandoned it altogether. This is to be regretted; for though many theories in physic lead to no practical conclusion, yet, had Sydenham indulged this hypothesis a little longer, so as to endeavour either to establish or confute it, he must soon, with all his sagacity and discernment, have arrived at the truth: and the knowledge of the fact, that the small-pox was contagious, would not have been a barren piece of information, but must have immediately suggested the propriety of preparing, in some way, for its probable attack, those who were likely to be exposed to its infection. It would undoubtedly have indicated the employment of some measures of precaution similar to those now in use where the measles are expected. It was not, however, until after the experiment of inoculation, which evidently

originated in the knowledge of the contagious nature of the disease in another country, that this property of the small-pox was put beyond all doubt in England. And yet, so slow is the human mind in abandoning an established error, that even after this discovery, the influence of the atmosphere continued to be regarded as a cause of the disease, equally efficacious with the infection of another person. The fact is, that the physicians of that day had not learned to distinguish between the contagion which excited the disease, and that peculiar inexplicable disposition of the air which, occurring occasionally, contributes to promote its spreading. The co-operation of both these causes is necessary to render the small-pox epidemic; but the first only is capable of exciting it.

From these observations, it is clear that the notion of *contagion* is so far from being natural and obvious to the mind, that even when it presented itself to such an understanding as that of Sydenham, it was not admitted, because the evidence of its existence, in the small-pox itself, did not seem to him to be sufficiently conclusive.

About the time of Sydenham, another eminent writer, Willis, made a nearer approach to the truth; but still he considered contagion as one only of the causes of the small-pox, which, according to him, arose from three; and to the infection of another person he assigned no pre-eminence over the others. The two other causes were, a 'disposition of the air,' and an immoderate disturbance of the blood and juices; in proof of which, he says that he had known some per-

sons who, solely from excess in diet or violent exercise, had fallen ill of this distemper, though no one in the neighbourhood was labouring under it.

The opinion of Morton, another contemporary writer, on this subject, is obscurely worded: he says, that the cause of small-pox is an active poison, either engendered within, or admitted from without by contagion; but this can scarcely be understood as clearly expressing his belief that it was caught from another person. The word contagion seems, by these authors, to have been often used to denote the malignant state of the atmosphere which they imagined produced epidemic diseases.

There is a very distinct avowal of the infectious nature of small-pox, in a *Treatise on Inoculation*, published in 1727*. 'The small-pox,' says this author, 'is either epidemic, depending upon a particular constitution of the air; or contagious, communicated by morbific effluvia, which arise from another person affected with the same disease.'

With respect to scarlet fever, so far was Sydenham from considering it contagious, that he says he looked upon the disease, which, according to him, generally made its appearance in autumn, as nothing more than a moderate effervescence of the blood, arising from the heat of the preceding summer.

The observation, which he could not avoid making, that at that time it attacked '*integras familias*,' did

* Dissertatio in Inoculationis Variolarum Methodum, à Jacobo à Castro.

not suggest any suspicion of its being communicated from one to another.

Of the measles he says, *Infantes plerumque aggreditur omnesque adeo ex illis, iisdem mœniis conclusos.* But he says nothing of a peculiar infection, and, indeed, was of opinion that the measles in its nature resembled the small-pox. In his description of the measles which prevailed in 1674, he relates his attendance upon the family of the Countess of Salisbury, one of whose children was ill of that complaint, and soon after the rest, to the number of five or six, were attacked; but he makes no comment upon the circumstance.

Morton prudently observes, in his account of the measles, that in pronouncing an opinion upon the probable nature of the fever, before the appearance of the eruption, the physician should inquire whether the patient has previously had the measles, (for he had never in his whole practice known more than one boy seized with the disease a second time,) whether the constitution of air, or, to use his own words, *an constitutio aëris præsens sit morbillosa, an æger nuperrime consuetudine ac familiaritate morbillosorum usus fuerit.*

And here again it must be observed, that an equal agency in the production of the complaint was attributed to the atmosphere, as to the contact or approach of a person labouring under the same disease. So far were these writers from the detection of the simple truth, that in all cases these diseases were occasioned by contagion alone, and became epidemic when the

state of the air, or various other circumstances, predisposed those who had not previously had them to receive the infection.

A few years after the writers whom I have quoted above, Boerhaave expresses himself, in his Aphorisms, in the following words:—'The small-pox, though epidemical, is caught by contagion, communicated by another who has first laboured under it.'—Aphor. 1382.

To this exposition of the truth, the discoveries of modern days, and the advancement of medical knowledge, have nothing to add, though it has required many years to subjoin to the catalogue of contagious diseases, scarlet fever, the hooping cough, and, perhaps, some other diseases.

If we come now to consider the history of the Plague, we find one of the best authors on that subject, Dr. Mead, expressing himself in the following terms, when he is referring to the causes that spread that disease. This is done by contagion. 'Those who are strangers to the full power of this, that is, those who do not understand how subtile it is, and how widely the distemper may be spread by infection*, ascribe the rise of it wholly to the malignant quality of the air, in all places wherever it happens; and on the other hand, some have thought that the consideration of the infectious nature of the disease must exclude all regard to the influence of the air; whereas the contagion accompanying the disease,

* Mead, it may be observed, here uses the words, contagion and infection, synonymously—and they are so employed in this Essay, indifferently.

and the disposition of the air to promote the contagion, ought equally to be considered; both being necessary to give the distemper full force.'—*Dr. Mead on the Plague,* p. 41.

Again, when describing the circumstances which attended the introduction of the plague into Marseilles, 1720, he observes, 'Possibly there might be some fever of extraordinary malignity in Marseilles, such as is commonly called *Pestilential*, before the arrival of these goods. But no such fever has any indisputable right to the title of *Pestilence*, as I have before shown—on the contrary, these two, the real pestilence and such pestilential fevers, must carefully be distinguished, if we design to avoid all mistake in reasoning upon these subjects. Some such fever of uncommon malignity, I say, might perhaps be in Marseilles before the arrival of these goods. There might likewise be an instance or two of fevers attended with eruptions bearing some resemblance to those of the plague; for such I myself have sometimes seen here in London.'—p. 55.

Misled by some fallacious reasoning, similar, perhaps, to that which now exists on the subject of Indian Cholera, the French physicians would not allow the disease at Marseilles to be contagious, and the pestilence swept away 50,000 inhabitants in a few months. Here the existence of a real danger was denied at a time when, had it not been for this fatal security, the progress of the contagion might have been stopped, and an immense loss of life prevented.

In reasoning upon this subject, it is not sufficient to say, that there are persons who, from some fortunate

inaptitude to take infection at some particular time, have been exposed to the influence of the contagion of the plague, and yet have escaped unhurt; the same is true of the small-pox, the measles, and scarlet fever. For it is well known to medical men that children will occasionally resist the most free and unreserved exposure to the infection of these different diseases, and will at other times most unexpectedly catch them, without its being possible to trace the source of the infection. Nay, it is a well authenticated fact, that a prisoner shall appear at the bar of a court of justice, himself free from the symptoms of fever, and yet infect all around him, from the contagion with which his clothes are saturated.

On the other hand, Dr. Mead observes, 'that a bale of goods, which shall have imbibed the contagious aura when packed up in Turkey, or any remote parts, when unpacked here, may chance to meet with so healthful a temperament of our air, that it shall not do much hurt.' For, as he shrewdly remarks, 'to breed a distemper, and to give force to it when bred, are two different things.' But it would be extremely imprudent to calculate upon the chance of such an immunity. Is it to be supposed that so many countries should agree in the use of the precautions of quarantine without weighty reasons? Do the Frank inhabitants of Constantinople, during the plague, shut themselves up in their houses, without any necessity? and is their safety from its attacks not to be attributed to these measures of prudence?

The evidence upon which the various accounts of

the importation of the plague into different countries of Europe from the East, rest, is as conclusive as that of any other well authenticated historical facts, and it will require something more than mere bold assertion to shake our confidence in such testimony.

When Dr. M'Lean was asked by the select committee, which was appointed in 1819, to examine into the validity of the doctrine of contagion in the plague, if he considered the plague as not contagious? He answered, Yes.

Explain how you caught the fever?—By the air.

It is very difficult to obtain from the advocates of the doctrine of non-contagion, any definite explanation of what they consider to be the nature of that malignant state of the air, or epidemic constitution of the atmosphere, that gives rise to the plague. According to them it is neither extreme heat, nor extreme cold, nor dryness, nor moisture, nor is it any great change from one of these conditions to another. Dr. Mitchell replies to the question, How do you account for the revival of the plague at stated times, in Turkey, for instance?

'The chief exciting cause seems to be the particular state of the air, and winds blowing from certain quarters, from the *south*.'

In answer to the question—

In what manner do you account for an epidemic disorder being prevented by simply shutting a house in an infectious place? Dr. M'Lean says, 'According to his ideas of the subject, the benefit to be derived from shutting up must entirely depend upon the air in

which the house is situated, and the other conveniences enjoyed, and its degree of elevation from the ground. On these circumstances principally, and upon shutting the windows at the most dangerous periods of the day, so as not to allow a thorough draft of air, during the pestilential season in the town, depends the prevention, not of contagion, but of the entrance of the pestilential blasts which cause the malady*.'

Now it may be worth while to inquire what is the exact situation of those Frank inhabitants of Constantinople, who, during the height of the plague in that city, shut themselves up and adopt the precautions of a voluntary quarantine; and I will select the residence of the British Embassy, which is usually called the English Palace, as an example. It is situated in Pera, and stands in the centre of a large garden, which is surrounded by high walls. It immediately adjoins a Turkish cemetery, where multitudes are buried daily, during the season of pestilence. All the windows of the apartments usually inhabited, look to the south and south-west; they are almost always kept open, and the freest ventilation constantly maintained. The inmates of the palace take exercise in the garden, which is of several acres extent, at all hours, and expose themselves, without the slightest reserve, to every change of temperature; in short, the only precaution they adopt, is to remain within their walls, and avoid the possibility of touching any one infected with the plague.

* Minutes of Evidence before the Select Committee.

If it were possible that the disease should be excited by the air, what could save the English residents from its attacks? They are as much exposed to the influence of the atmosphere, particularly to the pestilential blasts from the south, as if they were walking the streets of Constantinople, and yet they uniformly escape. But it may be observed that the wind here blows generally from the east or west, that is, up or down the channel of the Bosphorus; and when it sets in from the west, which is often the case, the gales are charged with the effluvia from the city of Constantinople. Nor is the assertion true, that the Turks themselves have no idea of the infectious nature of the plague; many of them believe it to be so, and the most enlightened of them all, the Pasha of Egypt, adopts a quarantine for his own security. When the plague is at Cairo, he either retires to a garden situated about two leagues from the city, and surrounds himself by his troops, or he shuts himself up in a fortress on the other side of the Nile at Gizeh. It is not my intention to enumerate the various plagues that have, at different times, made their appearance in Europe, but the features of the following story are so striking, and all the particulars are detailed with so much circumstantiality, that it carries with it an irresistible conviction of its truth. It is an account given by Mead, of the introduction of the plague into the Peak of Derbyshire; and whether we consider the remoteness of the spot from the original source of infection, the distinctness of the channel by which the contagion was conveyed, or the judicious and effectual

means adopted to put a stop to the progress of the disease, the relation cannot fail to be thought interesting.

'The plague,' says he, 'was likewise at Eham, (Eyam,) in the Peak of Derbyshire, being brought thither by means of a box sent from London to a tailor in that village, containing some materials relating to his trade. There being several incidents in this latter instance, that will not only serve to establish in particular the precepts I have been giving, in relation to goods, but likewise all the rest of the directions that have been set down for stopping the progress of the plague from one town to another, I shall finish this chapter with a particular relation of what passed in that place. A servant, who opened the aforesaid box, complaining that the goods were damp, was ordered to dry them at the fire; but in doing it, was seized with the plague, and died: the same misfortune extended itself to all the rest of the family, except the tailor's wife, who alone survived. From hence the distemper spread about, and destroyed in that village, and the rest of the parish, though a small one, between two and three hundred persons. But notwithstanding this so great violence of the disease, it was restrained from reaching beyond that parish by the care of the rector; from whose son, and another worthy gentleman, I have the relation. This clergyman advised, that the sick should be removed into huts, or barracks, built upon the common; and procuring by the interest of the then Earl of Devonshire, that the people should be well furnished with provisions, he took effectual care that no

one should go out of the parish, and by this means he protected his neighbours from infection with complete success.'—*Mead on the Plague*, p. 149.

The name of the clergyman spoken of by Mead was Mompesson, and the following particulars of him are extracted from the European Magazine, July, 1793.

'Mr. Mompesson, who appears to have been an ailing man, never caught the plague, and was enabled, during the whole time of the calamity, to perform the functions of the physician, the priest, and the legislator of his afflicted parish, assisting the sick with his medicines, his advice, and his prayers.—This fatal disease visited seventy-six families, out of which two hundred and fifty-nine persons died. The churchyard not being able to contain the bodies of those who perished by the plague, many persons were buried in the hills, and the fields adjoining. Many of the tombstones erected to their memory are still visible. The plague broke out in the spring of 1666, and ceased at the beginning of October in the same year. To prevent the contagion from spreading into the neighbourhood of Eyam, the Earl of Devonshire, then resident at Chatsworth, six or seven miles from Eyam, caused provisions and the necessaries of life to be placed upon the hills at regular times, and at appointed places, to which the inhabitants resorted, and carried them off with them. By the persuasion and authority of the excellent rector, the inhabitants were prevailed upon to remain within a certain district.'

The same magazine contains three original letters of Mr. Mompesson, written during the time of the

plague, which were sent to the editor by a gentleman of Eyam, and which contain a very affecting account of his own domestic calamities; for though his children were sent away, his wife remained with him, caught the disease, and died of it. She is buried in the churchyard, and a monument with a Latin inscription erected to her memory.

The fact of the introduction of the plague into Rome, in 1656, from Naples, by clothes and other wares, appears to be well authenticated. At Marseilles in 1720, the contagion was brought by a vessel from the Levant, one of the crew of which first had the distemper; next those who attended upon the infected cargo while in quarantine, and soon after the surgeon who examined the bodies of those persons who died of the disease.

The particulars of the commencement of the plague at Messina, in 1743, are as follows.

A Genoese ship arrives from Patras, in the Morea, laden with cotton—a sailor dies on board—the ship is put in quarantine—but the cotton is privately landed—the master and some of the sailors die three days after—the vessel is burnt, but the goods are concealed and publicly sold—the plague appears, and spreads through the city.

In 1769 a war broke out between the Russians and Turks. On the following year, the Turks brought the plague into Wallachia and Moldavia, and many Russians died of it at Yassy, the capital of the latter province. The succeeding summer the pestilence extended into Poland, and reached Kiow, and towards the end

of November, 1770, it made its appearance in Moscow. Much doubt was entertained at first, as to the nature of the fever, and there were many consultations of the physicians on the subject of the public health; but the disease continued to extend its ravages, and spared no part of the city, except the Imperial Foundling Hospital, which contained about one thousand children and four hundred adults; and this exception was solely the result of interdicting all communication with the neighbouring houses.

In the late plague at Malta, in 1813, a ship arrived from Alexandria with the plague on board, and very soon after the disease appeared on the island. Now to say nothing of the coincidence of the arrival of the vessel from Egypt and the appearance of the plague in Malta, the very channel by which the contagion was conveyed from the infected ship in the harbour into the town of Valetta, was as distinctly pointed out as the desire of concealment, and the nature of so illegal and clandestine an intercourse, would permit.

It is quite idle to say, that all these accounts are fabulous, or that the minds of those who related them were so warped and perverted by preconceived notions and unfounded prejudices, as to have been rendered incapable of observation.

The fallacy of those who support such opinions consists in their confounding the contagion itself with the different causes which spread and give effect to it; in other words, with those agencies which render the infection epidemic. There can be no doubt that sudden and extreme vicissitudes of temperature, deficiency of

nourishment, depression of the mind, are causes which induce debility, and predispose the body to yield to infection, when applied. There may also be some condition of the atmosphere which favours the diffusion of contagion, even more than the obvious causes above enumerated, which is not appreciable by any of our senses, and may always elude discovery, and refuse the precision of accurate definition.

Whether a disease be communicable from one person to another, must, to a superficial observer, appear to be a question that ought not long to admit of any rational doubt; but the reader of the preceding remarks will see, that the contrary has always been the case. There are two modes by which a disease may be communicated; the first is, by actual contact, or by the insertion or absorption of some visible matter from one body which is diseased into another in a healthy state—the second, by a near approach to the diseased person, so that the breath, or some emanation from his body, may convey the infection to another, which is previously in a healthy state. The measles, scarlet fever, and some other febrile diseases, are now familiarly known to be propagated in this latter manner. As an example of both modes of propagation, the small-pox may be cited, which may be conveyed to another, either by inoculation, or simply by approaching a patient labouring under this disease, or coming near or in contact with clothes that have been worn by him. In general it may be said, that noxious matters, propagating diseases, are either something visible and

substantial, as seen in the pustules of small-pox, or the vesicles of cow-pock; or something invisible, the existence of which is known only by its effects, as proved in the spreading of measles, the scarlet fever, hooping-cough, &c.

That the itch and lues venerea are communicable there never was any doubt; the experiment was too clear, and the result too palpable to admit of any hesitation; and it is a curious fact, furnishing an example of the proneness of the human mind, to run into opposite errors, that soon after the appearance of the latter of these two diseases in Europe, it was supposed to be communicable also by the breath; for whispering in the king's ear, knowing himself to be affected with venereal distempers, was made one of the articles of impeachment against Cardinal Wolsey.

But that certain emanations, not perceptible by any of the senses, can communicate a disease from one person to another, at a short distance from him, is an idea that has never been adopted, except as a last resource, when the mind of the inquirer, having in vain attempted every other sort of explanation, has been obliged to abandon them all in turn, as untenable. The source of this blind and obstinate perseverance in error must be sought for in the pride of the human intellect, which is more gratified by an elaborate theoretical view of the operation of sensible causes upon the body, than by the simple avowal of an ultimate fact, viz., that one person is infected by his approach to another labouring under disease, in a manner inscrutable, unknown, and inexplicable. The

history of all the present known contagions, as I have endeavoured to show, fully bears me out in this assertion; but, as if by some fatality, no advantage was to be derived from former experience, no lesson to be learned from the records of past times, whenever a new disease of the same character arises, which, becoming prevalent, (epidemic as it is called,) attracts public attention, the same difficulties again occur, the same blunders are to be again committed, and medical men, who ought to be the very first, are the last to acknowledge the true nature of the disorder.

That this is true will, I think, not be difficult to demonstrate by a few references to the voluminous Reports printed in India, on the subject of the Cholera Spasmodica, which appeared in August, 1817, first at Jessore, one hundred miles north-east of Calcutta, and, in less than two years, had spread from the most northern parts of Indostan to Ceylon, and from the Indus to China.

As a specimen of the manner in which the practitioners in that part of the world reasoned upon this important subject, I shall first quote the following remarks, to be found in the Bengal Report:—

'In matters of daily observation, and especially in those narrowly concerning the interests and safety of all, there is, perhaps, no fairer criterion of truth, than the common judgment of mankind. The progress of any generally fatal disorder is exactly of this description; and, accordingly, we find, by looking into the histories of all the great epidemical and

infectious distempers, to which the race is subject, as the plague, small-pox, measles, and scarlet fever, that the people were never slow to discover their true nature, and ordinarily passed such judgments regarding them, as corresponded not merely with the opinions of more learned observers, but with the truth itself: so it is in the present epidemic.'

'*The whole body of the medical officers in Bengal, who have had an opportunity of seeing and remarking on the disease, without a dissenting voice, concur in declaring that it is not contagious**.'

How loose and inaccurate this reasoning is, indeed how diametrically opposite to the fact†, the broad assertion about the histories of the great epidemics really is, has been already clearly shown; and yet, upon this hasty decision hung the fate of thousands, I might say millions.

That the truth forced itself upon the minds of the most candid of the Indian medical practitioners, will appear from several extracts which I shall give from the different Reports, and how, with the numberless opportunities afforded by daily experience of verifying that opinion, a just and right conclusion was not come to by all of them, is scarcely conceivable. When placed in juxtaposition, they exhibit a strong

* Report of the Epidemic Cholera Morbus of Bengal, 1817-18-19, by Mr. Jameson, Calcutta.

† This is the very blunder which the anti-contagionists made in 1825, when they denied the contagious nature of the plague. 'No one,' said they, 'can doubt, no one ever did doubt, that the small-pox is contagious.'—*Westminster Review*, No. V. p. 147. And yet it appears that this disease had been well known in Europe for nearly 1000 years before it was admitted to be contagious.

body of evidence to prove that the Cholera Spasmodica was of a contagious nature in India. 'It is supposed to exist in the atmosphere, from its pervading everywhere so extensively; but how comes it to spread in opposition to a continual current of air, namely the S.W. monsoon? Nevertheless, the idea of its being contagious, is entertained by few.'—*Bombay Reports*, p. 140.—*Mr. Surgeon Anderson.*

'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.'—*Bombay Reports*, p. 173.—*Surgeon Jukes.*

'With respect to the origin and nature of the malady, I am incompetent to give an opinion; but that its progress is independent of the air, I think there are many circumstances to justify the belief. In the first place, we see that it has made its way, independent of a permanent S.W. wind, from Jaulna, down to Punderpoor. Its effects were not instantaneous in the country, but its progress may be traced by a slow advance from fifteen to twenty-nine miles a day, as if it had been communicating gradually by persons travelling from town to town.'—*Bombay Report*, p. 118. —*Captain Sykes, Punderpoor, August* 15, 1818.

'If the disease were occasioned by a distempered state of the atmosphere, it would have spread over the country with some sort of regularity; but it seems generally to have travelled in lines along the post

roads, and always to have required a succession of subjects for its propagation.'—*Mr. Surgeon Coates.*

'Its introduction to Bombay has been clearly traced to a person who came from the Deccan, and passed through Panwell when this disorder was raging there; and it has been observed here, that whenever it appeared in any particular spot or family, a considerable proportion of the family or of the neighbours were attacked within a very short period of each other; on many occasions, I have seen three or four of a family lying sick at once. In bringing forward these facts, however, it may be proper at the same time to state, that of the forty-four assistants employed under me, only three were seized with the complaint.'—*Dr. Taylor to the Medical Board at Bombay, Nov.* 16, 1818, p. 195.

This last is a most unlucky observation; for the negative proofs, however numerous, ought certainly not to be put in the scale against the positive instances of contagion: besides, the observation, though it might perhaps have been frequently true, was not uniformly so, for we have the authority of another Indian practitioner to the contrary:—

'As every epidemic, by accumulation of subjects, has a tendency to propagate its virus, I am cautious in reporting this disease not infectious. *Almost every attendant in the hospital, in the short space of six days, has had the disease. There are about thirty attendants in the hospital.*'—*Dr. Burrell, Seroor, July* 27, 1818. *Bombay Report*, p. 9.

And yet to this exemption from infection enjoyed by

many who were exposed to it, may be traced the error into which the Indian practitioners generally fell, when they declared the disease not to be contagious. One gentleman, Mr. Surgeon Coates, says, 'I consider the disease infectious, but though this opinion should be well founded, *it ought to occasion no alarm*, for it is only under some peculiarity of constitution, and that fortunately very limited, that the poison acts. About one in forty in our camp was attacked, and I should think this is above the usual proportion.' Thus those who have the candour to admit that it is infectious, think that it ought to occasion no alarm, and those who are advocates for its not being so, dwell only on the fact that many who are exposed to catch it, escape altogether, as if this exemption were not observable in all other contagious diseases.

Whatever may have been the opinion in India in 1818, yet in the course of this pestilence since that time, moving east, west, south, and north, from China to the Baltic, from the straits of Sunda to Archangel, there surely have now been victims enough to satisfy the most sceptical, and induce us to lay no great stress upon the happy exemptions from its attack.

In the Indian Reports the following observation frequently occurs, and it seems to have had great weight with the medical practitioners and the other authorities there, in deciding them to declare peremptorily that the cholera was not a contagious disease.

'If it is not contagious,' say they, 'and the general belief should prevail of its being so, the most serious distress and inconvenience would arise from the dereliction of the sick, to which it would give occasion.'

So that, lest the few sick, who might fall victims to the disease on its first breaking out in India, should be deprived of medical assistance and attendant nurses, the whole human race was to be exposed to the dire calamity of an universal pestilence! But the argument is really worth nothing; for, admitting it to be contagious, no such alternative as the cruel and inhuman desertion of the sick would have been necessary. Are the unfortunate beings attacked with the plague deserted and left to die without remorse? The mode by which that fatal disease is propagated having been well studied, (which is now acknowledged to be by near approach to, or, as is the general belief, by actual contact with infected persons or things,) the sick of the plague are attended, and medicines administered to them, with as much humanity, as those of other diseases; care only being taken to observe the requisite precautions. Typhus fever is allowed to be contagious where free ventilation is not adopted, and when the breath of the patient is incautiously inhaled; but if these measures of prudence be observed, no physician, however timid, dreads to visit and prescribe for a person ill of the most malignant form of continued fever.

When the Indian practitioners saw a formidable disease spreading around them, they might have assumed, in the first instance, for greater security, that it was contagious. Had they instantly separated the sick from the healthy, and immediately endeavoured to ascertain all the facts connected with the intercourse that had taken place between the first who were

seized and those who *appeared* to catch the disorder from them, they would probably have discovered the mode by which the Spasmodic Cholera was propagated; and perhaps we might not now have to dread the approach of that fatal pestilence to our own shores. If, indeed, after all these prudent measures and anxious inquiries, it had turned out that no care, no means of prevention which the mind of man could employ or suggest were available, then indeed the desperate and desponding conclusion might at length have been adopted, that all human aid was in vain.

There is another circumstance alluded to in one of the ablest of all the Indian documents, *viz.* 'The Report of the Epidemic Cholera in the Presidency of Fort St. George, by Mr. Scot, Surgeon and Secretary to the Medical Board at Madras,' which I ought not to omit to mention, because more importance seems to be attached to it, than upon consideration it will appear to deserve. That Report states, that 'a complaint similar in all its symptoms had pre-existed in India many years before its late epidemic appearance in 1817; though this fact was reluctantly admitted, and too much kept out of sight in reasoning on its pathology.' That such a fact should now be ostentatiously brought forward by the anti-contagionists is very natural; since they imagine that a disease which had appeared at intervals from 1680 to 1784, committing ravages to a certain extent, and then stopping without diffusing itself universally, could not possess the property of being communicable from one person to another. To this, it may be replied, that the Records

from which these statements are drawn are very imperfect; that the description of the disease is too vague to prove its identity with the present Spasmodic Cholera; and finally, that allowing the anti-contagionists all the force and advantage of their argument, it must never be forgotten that the history of the origin of all contagious diseases is involved in great obscurity. An ingenious treatise has been written to prove that the eruptive fevers have prevailed from the earliest ages; that the small-pox, measles, and scarlet fever were known to the Greek and Roman physicians; nor is the position altogether void of plausibility. But that it should ever be a matter of dispute, whether a disease so well marked, and so uniform in the appearance of its symptoms, did or did not exist at these remote periods, is a certain proof, at all events, that the small-pox never could have spread epidemically, and committed the ravages which it is observed in modern times to do in all countries that it invades, where no measures of precaution have been previously employed to modify or prevent it.

As small-pox is conceived to be *adumbrated* in the writings of Hippocrates and Galen, (who, by the bye, have described other diseases very precisely and accurately,) so cholera is supposed to be described in the medical writings of the Hindoos, some of which are of great antiquity, as may be inferred from their being attributed to *Dhanwantari,* a mythological personage, coinciding in character with the Æsculapius of the Greeks. The same ingenious persons, who recognize the small-pox and the measles in the writings of

the Greeks, will have no difficulty in detecting the Spasmodic Cholera of the present day in the *Sitanga*, a species of the *Sannipata;* which is described as 'chilliness, like the coldness of the moon, over the whole body, cough and difficulty of breathing, hiccup, pains all over the body, vomiting, thirst, fainting, great looseness of the bowels, trembling of the limbs;' or rather, perhaps, may discover it in the *Vishúchi*, whose symptoms are 'dimness of sight in both eyes, perspiration, sudden swooning, loss of understanding, derangement of the external and internal senses, pains in the knees and calves of the legs, griping pains in the belly, extreme thirst, lowness of the bilious and windy pulses, and coldness in the hands, feet, and the whole body*.'

Be this as it may, no one can deny, that in the Spasmodic Cholera of India, which has now spread over a great part of Europe, we see at this moment a disease which, in its progress and mode of propagation, possesses all the properties of a contagion; and to maintain that it is not a pestilence of that sort, is, in my humble opinion, wilfully to abandon all the ordinary maxims of prudence, and to remain obstinately blind to the dictates of common sense.

Should the disease unhappily reach this country, will the perverse ingenuity of the anti-contagionists find out some plausible argument, by which they will endeavour to prove, that, though the very ship which may import it should be pointed out, yet there was some accidental coincidence between the arri-

* Madras Reports, page 3.

val of the vessel and the breaking out of the pestilence, and that the two events had no necessary connexion; and, as in the case of the Topaze frigate, at the Mauritius, that the two circumstances were independent of each other?

That no such opportunity for subtle disputation may ever present itself in England is most devoutly to be prayed for: in the mean time, due diligence should be exercised to observe the strictest and most inexorable quarantine.

To conclude, whether the arguments I have employed in my Letter will be deemed sufficiently convincing by others, I cannot presume to anticipate; but it was a great satisfaction to me to perceive, in the frequent conversations we have had together upon this important subject, that your views entirely coincided with my own.

I am,

My dear Sir Henry,

Very faithfully, yours,

W. MACMICHAEL.

September, 1831.

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